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# PROCEEDINGS

OF THE

# LITERARY AND PHILOSOPHICAL SOCIETY

OF

# MANCHESTER.

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# INDEX.

AINSWORTH T.—Rainfall in 1862, p. 67. Rainfall in 1863, p. 227.
ALCOCK THOS., M.D.—Young Salmon, p. 100.
ATKINSON JOHN, F.G.S.—Rainfall in 1862, p. 68. Bi-focal Lenses for Spectacles, p. 190. Rainfall in 1863, p. 226.

BAXENDELL J., F.R.A.S., Hon. Sec.—Remarkable Atmospheric Phenomenon, p. 29. Variable Star, p. 43. Oscillations of Temperature at Greenwich, p. 90. Iris Diaphragm, p. 169. New Variable Star, p. 195. Periodic Changes in the Magnetic, &c., Condition of the Earth, p. 251. Velocity of Rifle Balls, p. 271.
BINNEY E. W., F.R.S., F.G.S., P.—Recent Marine Shells near Mottram-in-Longdendale, p. 15. Permian Formations in England, p. 35. MSS. of Dalton, p. 43. Wooden Box in the Lower Level Gravel, p. 74. Carboniferous Permian Strata of Cumberland and Dumfries, p. 162. Lancashire and Cheshire Drift, p. 214.
BOETTGER Prof.—Cæsium, Rubidium, and Thallium in a Mineral Water, p. 264.
BOTTOMLEY JAMES.—Method of Estimating Sulphur, p. 269.
BREGUET M.—Crystal of Iron, p. 10.
BROTHERS A., F.R.A.S.—Diaphragm for Determining the Magnitudes of Stars, p. 109. Circulation in Plants, p. 122. Photography by Magnesium Light, p. 235, p. 241.
BUNSEN Prof.—Properties of Rubidium, p. 37. Atomic Weight of Cæsium, p. 103.

CALVERT F. CRACE, F.R.S.—New Method of Producing Carbonic Oxide, p. 183.
CARRICK THOMAS.—Wave of High Water, p. 125.
CASARTELLI J.—Registering Barometer, p. 130.
CLAY CHARLES, M.D.—Existence of the Moa in New Zealand, p. 181. Habits and Character of the Maoris, p. 182.
CLIFTON Prof. R. B., M.A., F.R.A.S—Application of the Mechanical Theory of Heat to the Phenomena of Incandescence and Fluorescence, p. 92. Instrument to Observe Phenomena of Conical Refraction, p. 131.
COCKLE The Hon. JAMES, M.A., F.R.A.S.—Linear Differential Equations, p. 16. Theory of Equations, p. 171.
CROOKES WM., F.C.S.—His Discovery of Thallium, p. 73.

**Dale** J. G., F.C.S.—Picrate of Aniline, p. 99. Transparent Injections, p. 199.

**Dancer** J. B., F.R.A.S.—Oxycalcium Microscope, p. 248.

**Darbishire** R. D., F.G.S.—Coins in Spain, p. 156. Marine Shells at High Levels in Caernarvonshire, p. 177.

**Dickinson** W. L.—Solar Eclipse, p. 66.

**Dyer** J. C., V.P.—Defective Iron Screw Bolt, p. 9. History of Steam Navigation, p. 48. Action of Heat and Force on Matter, p. 77. Letter Relative to Dr. Rankine's Reply, p. 105. Reply to Dr. Rankine, p. 113. Recent Discoveries in Elemental Physics, p. 207. Nature of Friction in Mechanics, p. 245. Notes on Spinning Machines, p. 265.

**Fairbairn** W., LL.D., F.R.S.—Corroded Boiler Plates, p. 240.

**Fryer Alfred**.—Observations of the Barometer, p. 5.

**Gerard Jules**.—Africa in its Relations with Europe, p. 1.

**Harley** Rev. **Robert**, F.R.S., F.R.A.S.—Linear Differential Equations, p. 11, p. 17. Bring's Reduction of Equation of 5th Degree, p. 69. Theory of Equations, p. 173.

**Heap John**.—Rainfall in 1862, p. 112. At Oldham, 1836-62, p. 129. Rainfall in 1863, p. 228.

**Heelis Thomas**, F.R.A.S.—Notes of Observations on η Argûs, p. 28. Hydrometric Observations of the Water of the Mediterranean, p. 28. Ocean Swell, p. 45. Magnitude, &c., of Waves, p. 108. Fog in the Mediterranean, p. 169. Meteor Seen by Daylight, p. 193.

**Heys** W. H.—Cotton Fibre, p. 224.

**Hopkins Thomas**, M.B.M.S.—Influence of the Earth's Rotation on Winds, p. 26.

**Hull Edward**, B.A., F.G.S.—New Red Sandstone and Permian Formations as Sources of Water for Towns, p. 32. New South Wales Coalfield, p. 38. Drift Deposits and Gravels Round Manchester, p. 212.

**Joule** J. P., LL.D., F.R.S., V.P.—Notice of a Compressing Air Pump, p. 5. Steam Boiler Explosions, p. 25. Mirage at Douglas, p. 39. Sensitive Barometer, p. 47. Sensitive Thermometer, p. 73. Phenomena of Meteors, p. 203. Portrait of, 213.

**Kirkman** Rev. T. P., M.A., F.R.S.—Maximum Groups, p. 59. Complete Theory of Groups, p. 133, p. 161.

**Latham** A. G.—Metallic Lustre on the Wings of Moths, p. 198.

**Leigh John**, F.C.S.—Mildew in Cotton Goods, p. 41. Dialysis in Microscopical Investigations, p. 53.

Lowe G. C.—Mercurial Seconds Pendulum, p. 104. Luminous Meteor, p. 242.

Lynde J. G., F.G.S.—Colouring of Tissue by Magenta, p. 41. Action of Magenta on Vegetable Tissue, p. 87.

Mackereth Thos.—Rainfall in 1862, p. 112. Rainfall in 1863, p. 228.

Mitchell Capt. J.—Form of Cotton Hair, p. 209. Examination of Cotton in Various Stages of Growth, p. 261.

Molesworth Rev. W. N., M.A.—M. Gerard's Visit to Manchester, p. 4.

Mosley George.—Mildew on Cotton Goods, p. 42. Cotton Fibre, p. 55. Obituary Notice of, p. 274.

Nasmyth James.—On the Planet Mars, p. 97.

Nevill T. H.—Report on Shells in the Montreal Deposit, p. 100.

O'Neill Chas., F.C.S.—Cotton Fibre During Solution, p. 123. Apparatus for Measuring Tensile Strengths, p. 186. Experiments on Cotton, p. 188. Structure of Cotton Fibre, p. 197. Tensile Strength of Cotton, p. 235.

Platt W. W.—Rock Salt from Middlesborough, p. 158.

Rankine Prof. W. J. M., LL.D., F.R.S.—Reply to Mr. Dyer's Paper on Heat, p. 93. History of Steam Navigation, p. 105, p. 131.

Rawson Robert, Hon. Mem.—Memoir of the late Professor Hodgkinson, Part 2nd, p. 1.

Roberts Richard, Hon. Mem.—Obituary Notice of, p. 274.

Roberts W., M.D.—Action of Solution of Magenta on Blood, p. 9. Use of Magenta in the Examination of Tissues, p. 80. Blood Corpuscles, p. 101.

Roscoe H. E., F.R.S., Hon. Sec.—Rubidium, p. 37. Spectrum Analysis in Manufacture of Steel, p. 57. Carbon and Sulphur in a Meteorite, p. 57. Amount of Carbonic Acid in Air of Manchester, p. 219. Light Emitted by Burning Magnesium, p. 283.

Russell W. H. L., A.B.—Solution of the Differential Resolvent, p. 72.

Schorlemmer C.—American Rock Oil, p. 81.

Seguin M.—History of the Dynamical Theory of Heat, p. 21.

Sidebotham J.—Photo-Lithography, p. 97. Spurious Gold Slides, p. 210. Source of the Nile, p. 211. Method of Mounting Microscopic Objects, p. 232.

Smith R. Angus, F.R.S., V.P.—Optical Phenomenon, p. 39. Crystallised Amalgam of Sodium, p. 106. Decomposition of Phosphate of Lime, p. 107. Ancient Maps of Africa, p. 229.

# PROCEEDINGS

OF

# THE LITERARY AND PHILOSOPHICAL SOCIETY.

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Ordinary Meeting, October 7th, 1862.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

A Paper, by ROBERT RAWSON, Esq., Hon. Mem., was read, entitled, "Memoir of the late Professor E. Hodgkinson. Part 2nd."

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Ordinary Meeting, October 21st, 1862.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

The Rev. W. N. MOLESWORTH introduced to the Society M. Jules Gérard, the celebrated African traveller and lion hunter, officer of the French Army in Africa.

M. GERARD delivered an address on Africa, in reference to its future relations with Europe, and on the journey of exploration which he proposes to undertake in the former continent.

The following is a translation by the Rev. W. N. Molesworth, from a summary made by M. Gérard, who spoke in the French language.

M. Gérard discussed—1st, the question of climate; 2nd, that of the races of men by whom the African continent is peopled; 3rd, the means by which he hopes to avoid the difficulties and the dangers which have led to the failure or the death of many previous explorers. M. Gérard observed that Africa is unhealthy only on the southern, eastern, and western coasts, in the neighbourhood of the equator; but he maintained that in other latitudes, and especially in the interior, the country is healthy and very habitable for Europeans; and in proof of this he referred to the English colony at the Cape of Good Hope, at the southern extremity of the continent; to Algeria, at the northern extremity; and to all the countries in the centre which are situated at a distance from the great lakes and from the rivers which overflow their banks, that is to say, all the countries which are at a sufficient elevation above the level of the sea, and remote from the miasma produced by stagnant water.

With regard to races, M. Gérard has found that there exist various peoples who are both morally and physically degraded; but he has also found that Africa contains populations which are deserving of interest, and which are susceptible of progress and civilisation. With regard to the former, he expressed his regret at the utter neglect with which they are treated, inasmuch as the more weak and ignorant a being is, the greater is its claim on the indulgence and support of the strong; and he called the attention of the Society to the people of Dahomey, stating that their barbarous customs have not originated with themselves, but from the sovereign and his favorites. As a proof of this assertion M. Gérard said that the victims in their human sacrifices are sometimes prisoners of war, but more frequently men, women, and children belonging to the nation of Dahomey. It is incredible that it can be agreeable or even indifferent to the people of Dahomey to sacrifice their families to the pleasure of the king. M. Gérard is of opinion that the people endure this calamity because imposed on them by despotic violence; but that, however brutalised, they would bless the hands that would bring them deliverance. As a means of promoting a result so desirable in the interests of humanity, M. Gérard proposes to arm the peoples who dwell on the northern borders of the country of Dahomey. They are the natural enemies of the abominable government of Dahomey for many reasons, of which the principal are that the troops of the king make continual incursions into their territories to procure slaves, and that he prevents them from trading with the European establishments on the coast. The king of Dahomey closes all the routes in order to prevent these peoples from obtaining any other arms than the primitive ones which they use at present, viz., the lance and the arrow, thus enabling him to preserve the advantages he possesses over them in consequence of the supply of fire-arms which he can command. M. Gérard pointed out that arms and ammunition might be distributed among these peoples by two channels over which the king of Dahomey has no command. With these arms we might introduce men to instruct and direct the natives. This done, the interests of the people of Dahomey would have to be separated from the interests of the dynasty, by means of agents recruited in the country, and thus the axe would be laid to the very root of this dynasty. Then the abominations which have made us groan with horror will be succeeded by a natural state of things; the labours of peace will take the place of war, carnage and fire; and Europe, as well as Africa, will profit by the happy change. In the journey of exploration which he is going to undertake, M. Gérard proposes to study *on the spot* this great question, in the solution of which he will be happy to participate actively. His point of departure will be Sierra Leone, or some other English possession a little further to the west. He will endeavour first to discover the *sources of the Niger*, which are still unknown. Afterwards he will visit the peoples who dwell at the northern extremity of the republic of Liberia and the country of Dahomey. He will then return towards the upper basin of the great river, where, according to the information he has obtained, he expects to find a vast agricultural country in which the natives produce cotton and indigo; and in which he hopes that an establishment may be founded for encouraging the growth of these productions, and extending commercial relations with Europe. After having studied these countries and their inhabitants, M. Gérard will visit Timbuctoo, in order to obtain the papers of the unfortunate Major Laing, who was murdered near this town in 1822. From Timbuctoo he will return to Europe by Ainselah, Goleah, and Algiers. The length of this journey is about three thousand miles; for the purpose of accomplishing it M. Gérard reckons on the destruction which he will make of leopards, lions, and panthers, which depopulate whole villages in these countries; and on the relations he has formed with the Arabs. M. Gérard concluded by thanking the president and the members of the Society for the great attention with which he had been heard, although addressing them in a foreign language.

A vote of thanks to M. Gérard having been passed, and some discussion having taken place on his Paper,

The Rev. W. N. Molesworth remarked that M. Gérard had visited Manchester in the hope of obtaining pecuniary assistance towards carrying out his proposed explorations. His object was of great importance both in a scientific and commercial point of view. He was opening up to them new sources whence they might be supplied with good and cheap cotton, and he also expected to make geographical, ethnological, and zoological discoveries. It was to be hoped, therefore, that gentlemen present who felt an interest in either of these two objects would make his purpose as widely known as possible, and render to him, or obtain for him, all the assistance they could.

Some remarks having been made respecting the sudden fall of the barometer, which occurred on Sunday the 19th inst., Mr. Baxendell read the following extract of a letter from Mr. Vernon, F.R.A.S.:—"At 8h. a.m. on Sunday, the reading of the barometer was 29·483 inches, and at 9h. 50m. p.m. 28·502 inches, a fall of 0·981 inches in a little over twelve hours: the entire fall in ten days was 1·8 inches, a very unusual amount. The depression accompanying the Royal Charter storm was only about one-half that accompanying the past storm."

Mr. ALFRED FRYER, who observed the movements of an aneroid barometer under the microscope during the storm, stated that the minimum pressure occurred at 10h. 10m. p.m., and that for some minutes before and after this time the index of the instrument was in a state of rapid vibration.

A Paper was read by Dr. J. P. JOULE, entitled, "Notice of a Compressing Air Pump."

The Author referred to the difficulties of realising in practice the theoretical advantages of the air, or the superheated steam engine. The abrasion which takes place when metal rubs against metal, without an intermediate lubricator, speedily destroys the cylinder. He believed that the necessity of using elastic packing would not exist if the length of the channel along which the elastic fluid must pass, in order to arrive at the opposite side of the cylinder, were sufficiently increased. This might be accomplished by increasing the depth of the piston, or by placing on the rim of the piston concentric rings to enter, at the beginning and end of each stroke, corresponding concentric grooves in the covers of the cylinder.

The principle of great depth of piston, as a substitute for packing, had been successfully carried out in the pump which was the subject of this communication. The cylinders, two in number, are twenty inches long and two inches in diameter. The pistons are solid cylinders of iron, ten inches long, fitting as accurately to the cylinders as is consistent with freedom of motion. The depth of each piston, as compared with its diameter, renders the usual guide or parallel motion unnecessary, so that the connecting rod is simply jointed to the top of the piston. Air is readily compressed to sixteen atmospheres, the quantity passing the sides of the cylinders being very trifling.

## MICROSCOPICAL SECTION.

### Fourth Annual Meeting, 19th May, 1862.

E. W. BINNEY, F.R.S., F.G.S., V.P. of the Section, in the Chair.

Mr. David Joy, and Mr. John Leigh, M.R.C.S., were elected members of the Section.

The SECRETARY then read the Annual Report of the Council, which was agreed to and ordered to be printed for circulation amongst the members.

The following gentlemen were then elected officers for the ensuing Session:—

**President.**
WILLIAM C. WILLIAMSON, F.R.S.

**Vice-Presidents.**
EDWARD W. BINNEY, F.R.S., F.G.S.
JOSEPH SIDEBOTHAM.
ARTHUR G. LATHAM.

**Secretary.**
GEORGE MOSLEY.

**Treasurer.**
JAMES G. LYNDE, M. INST. C.E., F.G.S.

**Of the Council.**
THOMAS ALCOCK, M.D.
JOSEPH BAXENDELL, F.R.A.S.
JOHN DALE, F.C.S.
J. W. MACLURE, F.R.G.S.
THOMAS H. NEVILL.
JOHN PARRY.
WILLIAM ROBERTS, M.D.
JOHN WATSON.

It was proposed by Mr. LYNDE, seconded by Mr. NEVILL, and carried unanimously, that the following alterations be made in the Rules:—

No person shall be eligible for the office of President for more than two years in succession.

The Vice-President who has attended the least number of meetings shall be ineligible for re-election as Vice-President for at least one year.

The two members of the Council who have attended the least number of meetings shall be ineligible for re-election for at least one year.

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October 20th, 1862.

Professor Williamson, F.R.S., President of the Section, in the Chair.

Mr. George Venables Vernon, F.R.A.S., was elected a member of the Section.

Mrs. Bury, of Croft Lodge, Ambleside, presented, through Professor Williamson, a series of twelve photographic plates, with seventy-two figures of *Polycistins* from drawings by that lady; also seventeen mounted slides of Barbadoes earth.

Mr. Horatio J. Frembly, of Gibraltar, presented, through Mr. H. A. Hurst, thirty-four slides of tongues of mollusca, collected and mounted by himself. An elaborate report upon their scientific classification was read by Dr. Thomas Alcock.

Mr. H. A. Hurst presented a collection of tongues of mollusca, from Bengal, made by him during his residence in India. They have been placed in the hands of Dr. Alcock, who has kindly undertaken to examine and report thereupon.

Captain J. C. Gales, of the ship "Quito," presented soundings taken off the coast of Chili and the Falkland Islands; two specimens of anchor mud, from the Falkland Islands; and some of the *Fucus Natans* from the Sargossa Sea, with specimens of its inhabitants, dried in the sun. Captain Curling, of the P. and O. S. S. "China," presented four soundings from the coasts of Malabar, Yemen, Malacca, &c.; Captain Vickers, of the "Rosina Claypole," anchor mud from Port Royal and Black River, Jamaica, and a sounding taken off the south coast of Ireland; and Captain Samuel Flood, of the ship "Pantoleon," anchor mud from Sumatra and Malacca. Two deep Atlantic soundings in 1,730 and 2,220 fathoms, were also thankfully acknowledged.

Mr. **Thomas Heelis** presented many interesting specimens, collected during his late voyage, amongst which may be named two soundings from the Agulhas Bank, two specimens of Hoogly mud, scales of flying fish, and gulf weed, with minute crustacea and other animals preserved in spirits.

Mr. **Joseph Sidebotham** presented to every member of the Section a photographed finder for high powers, in case inscribed with the member's name.

Mr. **John Dale** presented a quantity of desiccated balsam dissolved in chloroform, to be divided amongst the members.

Mr. **A. G. Latham** presented a mounted spiracle of a mole cricket from Africa, and pointed out the difference from that found in this country.

Mr. **E. W. Binney** presented a copy of his Paper "On some Fossil Plants showing structure, from the Lower Coal Measures of Lancashire."

A Letter was read from Mr. **Thomas D. Toase**, of Jamaica, with reference to the animalcule previously described.

Mr. **Brothers** exhibited some fine specimens of *Stephanoceros*.

Mr. **Parry** exhibited some photographs of magnified sections of wood.

Mr. **W. H. Heys** exhibited the peculiar oil glands on the leaf of the *Procranthera Violacea;* stellate hairs on the calyx of the *Deutzia Scabra,* which differ from those on the leaves by having a greater number of rays and a central disc; spines of the *Loasa Coccinea;* and two kinds of spangles upon oak leaves from Beddgelert.

Dr. **William Roberts** exhibited a mounted specimen of crystals of *Cystin.*

## Ordinary Meeting, November 4th, 1862.

J. P. Joule, LL.D., F.R.S., Vice-President, in the Chair.

Mr. Peter Hart was elected an Ordinary Member of the Society.

Dr. **Roberts** exhibited some microscopic preparations illustrating the effect of a solution of magenta on the blood. The red blood-disks were tinted of a faint rose; and one portion of their outline, in a majority of the corpuscles, appeared more deeply tinted than the remainder. The pale corpuscles were more strongly tinted than the red; and their nuclei were displayed with great clearness, dyed of a magnificent carbuncle-red. A number of the nuclei were seen in the process of division, more or less advanced, and in some cells the partition had issued in the production of two, three, or four distinct secondary nuclei. There was evidence that these secondary nuclei were set free in the blood, and, by subsequent enlargement and change of form and chemical constitution, developed into red blood disks, which would therefore appear to be, as Mr. Wharton Jones first conceived, free cellæform nuclei, and not, according to the current belief, enlarged and altered pale corpuscles.

Mr. **Dyer**, Vice-President, exhibited a broken screw bolt, 1⅛in. square (used to fasten a cart body to the axle). The fracture, near the head end, appeared very much like one of cast iron; imbedded in the centre of the bar was a smooth egg-shaped mass about ½in. diameter, crossing the fracture, and leaving a cavity as its mould in the metal on one side. He assumed that faults like this were probably owing to the rapid processes in use for reducing masses from the puddle into bars of wrought iron, whilst the metal was only partially converted to the malleable state, as appeared in this sample of bad iron. The iron in a semi-fluid state, is passed from the furnace through a succession of rollers, without re-heating or faggoting, as were formerly practised, and at once reduced to the sizes required. The improved rolling mills could not, it seemed, insure improved qualities of wrought iron, whilst they afforded temptations to make it far inferior to any that could have been made fifty years ago. Considering the many hazards to which life and property are exposed in travelling by railway and otherwise, from the iron "shuffled off in haste," and found in use in engineering constructions, it becomes important that previous tests should be employed to ascertain the real nature of the iron, so as to leave no question of its being in a safe condition for the purpose intended, and not like this specimen, and like much now-a-days made by pressing the half-converted puddle into marketable shapes.

In connection with the subject of the slow changes which iron undergoes, M. BREGUET, of Paris, stated that in their furnace for preparing soft iron, he had observed a remarkable case of crystallisation of wrought iron. One of the furnace bars became brittle, and on breaking a portion of it, he found it to contain a large cubical crystal of iron, each of whose sides measured five millimetres in length. This singular specimen is now in the possession of M. Balard.

A Paper was read "On a certain class of Linear Differential Equations," by the Rev. ROBERT HARLEY, F.R.A.S.

In the *Philosophical Magazine* for May of last year, Mr. Cockle showed, in a Paper entitled, "On Transcendental and Algebraic Solution," that from any algebraic equation of the degree $n$, whereof the coefficients are functions of a variable, there may be derived a linear differential equation of the order $n-1$, which will be satisfied by any one of the roots of the given algebraic equation. The connexion of this theorem with a certain general process for the solution of algebraic equations led me to consider its application to the form

$$y^n - ny + (n-1)x = 0 \ldots \text{(I)}$$

to which it is known that any equation of the $n$th degree, when $n$ is not greater than 5, can, by the aid of equations of inferior degrees, be reduced.

In the course of my investigations I was conducted to the conclusion that for all integral values of $n$ between the limits

$$n = 2,\ n = 5,$$

both inclusive, the linear differential equation, or, as it is proposed to call it, the "differential resolvent," is of the form

$$\left\{a_0 + a_1 x \frac{d}{dx} + a_2 x^2 \left(\frac{d}{dx}\right)^2 \cdots + a_{n-1} x^{n-1} \left(\frac{d}{dx}\right)^{n-1}\right\} y$$

$$= a_{n-1} \left(\frac{d}{dx}\right)^{n-1} y,$$

and I completely determined the constants $a_0, a_1, \ldots a_{n-1}$ for all the cases up to and including $n = 5$.

I found, moreover, that this result, in itself sufficiently remarkable, might be put under a still more simple and striking form by following a process of transformation proposed by Professor Boole in his Memoir on a General Method in Analysis, which appeared in the *Philosophical Transactions* for 1844, part II. I found in fact that writing $\varepsilon^{\theta}$ for $x$, and D for $x\frac{d}{dx}$ or $\frac{d}{d\theta}$, the differential resolvent of the trinomial equation (I) may be made to take the form

$$\mathrm{D}(\mathrm{D}-1)(\mathrm{D}-2)\ldots(\mathrm{D}-n+2)y-\left(\mathrm{D}-\frac{2n-1}{n}\right)\left(\mathrm{D}-\frac{3n-2}{n}\right)$$

$$\left(\mathrm{D}-\frac{4n-3}{n}\right)\ldots\left(\mathrm{D}-\frac{n^2-n+1}{n}\right)\varepsilon^{(n-1)\theta}y=0\ldots(\mathrm{A})$$

the only exception being the case $n=2$, in which the resolvent contains a term independent of $y$.

Using the ordinary factorial notation, that is to say, representing

$$(u)(u-1)(u-2)\ldots(u-r+1)$$

by $[u]^r$, the form (A) may be written

$$n^{n-1}\left[x\frac{d}{dx}\right]^{n-1}y-(n-1)^{n-1}\left[\frac{n}{n-1}x\frac{d}{dx}-\frac{2n-1}{n-1}\right]^{n-1}x^{n-1}y=0\ldots(\mathrm{B})$$

In the *Proceedings* of this Society (vol. II., pp. 181—184) for the 4th of February last, I gave, without the details of calculation, the several differential resolvents for the successive cases $n=2$, 3, 4, 5; and these results Mr. Rawson, of Portsmouth, has kindly verified. I gave, also, in the same Paper, the Boolian (symbolical) form of the resolvent for the biquadratic, and this seems to have suggested to Mr. Cayley an investigation, in which he showed, by the aid of Lagrange's theorem, that the equation (B) holds for all values of $n$. I had the honour of communicating Mr. Cayley's investigation to the Society on the ensuing 18th of February, and an abstract of it appeared at p. 198, vol. II., of the *Proceedings*. The Paper itself will be printed in the forthcoming volume of Memoirs. Before receiving Mr. Cayley's remarkable analysis, I had calculated, and I believe I had also communicated to Mr. Cockle, the Boolian forms of the resolvents for the cases $n=2$ to $n=5$, both inclusive; and these suggested to me the general form (A). (See *Proceedings*, vol. II., pp. 199—201, and pp. 237—241.)

The singular simplicity of these results for the form (I) had an effect inducing me to consider the corresponding form.

$$y^n-ny^{n-1}+(n-1)x=0\ldots\text{(II)}$$

to which also any algebraic equation of the $n$th degree, $n$ being not greater than 5, can, as Mr. Jerrard has shown, be reduced by means of equations of inferior degrees; and by induction I was led to the following general expression for its resolvent, viz.

$$n^{n-1}[(n-1)\text{D}]^{n-1}y-(n-1)(n\text{D}-n-1)[n\text{D}-2]^{n-2}\epsilon^{\theta}y$$
$$=[n-1]^{n-1}\epsilon^{\theta}\ldots\text{(C.)}$$

or, what is the same thing,

$$n^{n-1}\left[(n-1)x\frac{d}{dx}\right]^{n-1}y-(n-1)(nx\frac{d}{dx}-n-1)\left[nx\frac{d}{dx}-2\right]^{n-2}xy$$
$$=[n-1]^{n-1}x\ldots\text{(D)}$$

The particular cases on which these inductions were founded are given in the present Memoir.

Every differential resolvent may be regarded under two distinct aspects. It may be considered either, first, as giving in its complete integration the solution of the algebraic equation from which it has been derived; or, secondly, as itself solvable by means of that equation. In the first aspect I have considered the differential equation (A) in a Paper entitled "On the Theory of the Transcendental Solution of Algebraic Equations," just published in the *Quarterly Journal of Pure and Applied Mathematics*, No. 20. In the second aspect every differential resolvent of an order higher than the second gives us, at least when the dexter of its defining equation vanishes, a new primary form, that is to say, a form not recognised as primary in Professor Boole's theory. And in certain cases in which the dexter does not vanish, a comparatively easy transformation will rid the equation of the dexter term, and the resulting differential equation will be of a new primary form.

A Paper was read, entitled, "Note on Differential Resolvents," by William Spottiswoode, Esq., M.A., F.R.S., &c. Communicated by the Rev. ROBERT HARLEY, F.R.A.S.

The object of this note is, first, to show how the differential resolvent of an equation of any degree may be found by elimination from systems of linear equations. The method is exemplified in the cases of the complete quadratic and the complete cubic; and in the latter case the results of some investigations by Mr. Harley are introduced, whereby the resolvent is freed from an extraneous factor, and reduced to its proper degree (the 10th) in the coefficients of the original cubic.

## Ordinary Meeting, November 18th, 1862.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

It was proposed by Mr. DAVID MORRIS, and seconded by Mr. JOSEPH SIDEBOTHAM:—That the words "three-fourths" in Rule 6, Section iv., be altered to "two thirds." The motion being put to the meeting, there were 36 for it and 33 against: it was therefore declared to be carried.

## Ordinary Meeting, December 2nd, 1862.

J. P. JOULE, LL.D., F.R.S., Vice-President, in the Chair.

E. W. BINNEY, F.R.S., the President, said that in a paper published in Vol. X. (Second Series) of the Society's Memoirs, "On the Drift Deposits found near Blackpool," he had stated, in a note at page 122, that since the paper was written Mr. J. F. Bateman, C.E., F.G.S., had informed him that in making the Hollingworth reservoir near Mottram-in-Longdendale, he had met with the common cockspur shell (*Turritella terebra*) in considerable abundance. During the past summer he had visited the locality alluded to by Mr. Bateman, in company with Mr. Prestwich, F.R.S. After going up to the uppermost part of the reservoir, which is one of those belonging to the Corporation of the City of Manchester, to the point where the goit conveys the water on the east side of the valley, we saw a deposit of brown sandy clay, or till, which had been cut through to the depth of between three and four feet for the purpose of forming the goit. This deposit contained small granite and greenstone pebbles, some rounded and others angular. In it he found a considerable number of shells, some quite entire and others in fragments. He procured and showed to the meeting specimens of *Turritella terebra*, *Fusus Bamfius*, *Purpura* *lapillus*, two specimens of *Tellina* and *Cardium edule*. The City Engineer, Mr. J. G. Lynde, F.G.S., had given him the exact height of the spot where the fossils were found at as 568 feet above the level of the Irish sea.

Shells, identical with recent sea shells, have been found at much greater elevations on the mountains of North Wales, but very few so far inland; for the locality where the specimens were met with is full fifty miles in a straight line from the Irish sea, and a greater distance if the water courses of the Etherow and Mersey are followed. Mr. John Taylor has found recent marine shells in the sands of Bredbury and Hyde, which he has described in the Transactions of the Manchester Geological Society, and Mr. Prestwich informed him that he has found similar fossils on the Buxton Road, about three miles from Macclesfield, but the specimens herein described are the first that have been noticed in the deep vallies running up into the sides of the pennine chain.

He further stated that he had found a large mass of greenstone, evidently a travelled rock of the drift period, at the extreme end of one of the tributary vallies of the Tame, in Saddleworth, as high up as New Year's Bridge, near Denshaw Vale. All these facts prove the former presence of the sea (in some cases containing inhabitants similar to those found on our present coasts) high up on the sides of the Cheshire, Yorkshire, and Derbyshire hills at a recent period geologically speaking, and show that many of our deep vallies have not been formed by the streams of water now traversing them, but are chiefly due to the more powerful action of the waters of the ocean, most probably assisted by ice.

A communication "On certain Linear Differential Equations," by James Cockle, Esq., M.A., F.R.A.S., F.C.P.S., &c., was read by the Rev. ROBERT HARLEY, F.R.A.S.

At pages 11 to 14 of the Society's *Proceedings* for the 4th of November last, Mr. Harley has made an important contribution to the subject of linear differential equations. I am not in a condition to make a substantial addition to Mr. Harley's formulæ, but the following, derived by a mere change of notation from his, seem to me to give a new interest to his valuable results. Write

$$y^n-ny^{n-r}+rx=0\ \ldots\ \text{(III)}$$

$$n^{n-1}\,[(n-r)\mathrm{D}]^{n-1}\,y$$
$$-r^{n-1}[n\mathrm{D}-r-1]^{n-r-1}\left[\frac{n}{r}\mathrm{D}-\frac{n+r}{r}\right]^{r}e^{r\theta}y=\mathrm{X}\ \ldots\ldots\ \text{(E)}$$

or, which is the same thing,

$$n^{n-1}\left[(n-r)\,x\frac{d}{dx}\right]^{n-1}y-r^{n-1}\left[nx\frac{d}{dx}-r-1\right]^{n-r-1}\left[\frac{n}{r}\,x\frac{d}{dx}-\right.$$
$$\left.\frac{n+r}{r}\right]^{r}\cdot x^{r}\,y=\mathrm{X}\ \ldots\ldots\ \text{(F)}$$

and, further, suppose that X is equal to $\rho x^r$, where

$$\rho=\frac{1}{n-r}\,\frac{d^{n-1}}{dx^{n-1}}\,x^{n-r},\ \text{or}\ \rho=(1-1)^{r-1}[n-2]^{n-2},\ \text{or}\ \rho=\frac{1-1}{r-1}[n-2]^{n-2}$$

or any other discontinuous function of the like nature. Then (E) or (F) is, in the cases $r=n-1$ or $r=1$, the differential resolvent of (III).

Mr. HARLEY added the following remarks:—

Mr. Cockle's formula, (E) or (F), is interesting and useful, as comprehending the differential resolvents of both the trinomial algebraic equations

$$y^n-ny+(n-1)x=0$$

and

$$y^n-ny^{n-1}+x=0$$

under one and the same general expression. For these cases, ($r=1$ and $r=n-1$,) to which the author confines it by express statement, the formula holds good; but I do not find that it holds for other cases. If (*ex. gr.*) we assume

$$n=4\ \text{and}\ r=2,$$

the equation which in Mr. Cockle's communication is marked (III), becomes

$$y^4-4y^2+2x=0\ \ldots\ (a)$$

and the formula (E), after some slight reduction, gives

$$2^4(D)(2D-1)(D-1)y=(4D-3)(2D-3)(D-2)\epsilon^{2\theta}y$$
$$=\epsilon^{2\theta}(4D+5)(2D+1)(D)y;$$

whence

$$2^3(2^2-x^2)x\frac{d^3y}{dx^3}+2(2^3.3-19x^3)\frac{d^2y}{dx^2}-3^3x\frac{dy}{dx}=0,$$

an equation which I find, on trial, is not satisfied by the roots of ($a$). In fact, the differential resolvent of ($a$), obtained by direct calculation, is

$$2^4(2-x)x\frac{d^3y}{dx^3}+2^4.3(1-x)\frac{d^2y}{dx^2}-3.5\frac{dy}{dx}=0\ldots(\beta)$$

and the Boolian or symbolical form is

$$2^4[2D]^3y-[4D-3]^3\epsilon^{\theta}y=0,$$

which, expunging a common symbolical factor, may be reduced to the form

$$2^4D(2D-1)y-(4D-3)(4D-5)\epsilon^{\theta}y=0,$$

and returning to the ordinary form of a differential equation, the last result may be written thus:

$$2^4(2-x)x\frac{d^2y}{dx^2}+2^4(1-x)\frac{dy}{dx}+y=0\ldots(\gamma)$$

This equation I have also obtained by direct calculation. It is worth while noticing that ($\beta$) and ($\gamma$) may be combined in one equation

$$2^4(2-x)x\frac{d^3y}{dx^3}+2^4\left\{3(1-x)+\mu(2-x)x\right\}\frac{d^2y}{dx^2}$$
$$-\left\{3.5-2^4\mu(1-x)\right\}\frac{dy}{dx}+\mu y=0\ldots(\delta)$$

in which $\mu$ is a perfectly arbitrary function, either constant or otherwise.

I content myself at present with simply recording these results, without attempting a discussion of them; but I believe they will be considered, by cultivators of the calculus, as not wanting in interest.

In my third communication to the Society, entitled "On the Theory of the Transcendental Solution of Algebraic Equations" (see pp. 237—241 of Vol. II. of the *Proceedings*), I gave Mr. W. H. L. Russell's solution by definite integrals of a certain quartic resolvent, and in connexion therewith I remarked that Professor Boole had pointed out to me a method of solution which did not differ essentially from Mr. Russell's. I have since found that there is an essential difference, inasmuch as Professor Boole's method gives the solution in forms involving single, not (as in Mr. Russell's solution) triple integrals.

Soon after the publication of the Paper above quoted, I received from Mr. Cayley a letter (dated April 29, 1862,) containing some remarks on the subject, which I have pleasure in appending to this communication.

Mr. Cayley to Mr. Harley. "The series for the solution of your resolvent equation should I think be assimilated to the form of the hypergeometric series. If instead of the ordinary notation

$$F(\alpha,\beta,\gamma,x)=1+\frac{\alpha.\beta}{1.\gamma}x+\frac{\alpha.\alpha+1.\beta.\beta+1}{1.2.\gamma.\gamma+1}x^2+\text{\&c.},$$

we write

$$F\left(\begin{matrix}\alpha,\ \beta\\ 1,\ \gamma\end{matrix};x\right)$$

then naturally

$$F\left(\begin{matrix}\alpha,\ \beta,\ \gamma\\ 1,\ \delta,\ \epsilon\end{matrix};x\right)$$

will denote

$$1+\frac{\alpha.\beta.\gamma}{1.\delta.\epsilon}x+\frac{\alpha.\alpha+1.\beta.\beta+1.\gamma.\gamma+1}{1.2.\delta.\delta+1.\epsilon.\epsilon+1}x^2+\text{\&c.}$$

and Mr. Russell's series, writing them under the forms

$$1+\frac{\frac{5}{12}\cdot\frac{2}{12}\cdot\frac{-1}{12}}{1\cdot\frac{2}{3}\cdot\frac{1}{3}}x^3+\frac{\left(\frac{17}{12}\cdot\frac{5}{12}\right)\left(\frac{14}{12}\cdot\frac{2}{12}\right)\left(\frac{11}{12}\cdot\frac{-1}{12}\right)}{1\cdot 2\cdot\frac{2}{3}\cdot\frac{5}{3}\cdot\frac{4}{3}\cdot\frac{1}{3}}x^6+\text{\&c.}$$

will be

$$F\left[\begin{matrix}-\frac{1}{12}, \frac{2}{12}, \frac{5}{12} \\ \frac{1}{3}, \frac{2}{3}, 1\end{matrix} ; x^3\right] \qquad xF\left[\begin{matrix}\frac{3}{12}, \frac{6}{12}, \frac{9}{12} \\ \frac{2}{3}, 1, \frac{4}{3}\end{matrix} ; x^3\right]$$

$$x^2F\left[\begin{matrix}\frac{7}{12}, \frac{10}{12}, \frac{13}{12} \\ 1, \frac{4}{3}, \frac{5}{3}\end{matrix} ; x^3\right]$$

There is a little (incidentally) on these ultra-hypergeometric series in Krunmer's valuable Memoir on the hypergeometric series, Crelle, t. xv. I attach *no value whatever* to the transformation of the series into definite integrals. As to your own remark 'what is wanted is the solution without the aid of definite integrals'—it seems to me that the *series* is *the solution*, and that what is wanted is a study of these series, so as that they may be considered as known transcendents; which, considering the great extent of the theory of the simplest case, that of the hypergeometric series, is likely to remain a desideratum."

Ordinary Meeting, December 16th, 1862.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Dr. Joule read the following letter addressed to himself:—

"Varagnes, near Annonay, Ardèche, France,
"November 25, 1862.

"Dear Sir,

"I am much obliged to you for the disinterestedness you have maintained in reference to the discovery of the great principle that my uncle, the celebrated Montgolfier, revealed to me some sixty years ago.. Since that time I have passed my whole life in studying this question.

"Much merit is due to you, sir, for your defence of another's rights, especially after the compositions which you have published, and are, without doubt, the best explanations yet written of the great principle so continually occupying you, myself, M. Grove, M. Mayer, and many others. Among the latter there are many who endeavour to persuade the public (every one in his interest) that the world owes the great advancement of the dynamical theory of heat solely to their works; but the public do not like being led in this kind of logic, and look with indifference upon the pretended authors of these discoveries, at the same time raising the merit of those, like yourself, who take a pleasure in rendering justice to those who wait patiently until just and impartial men judge them by their works and not by their words.

"For more than sixty years I have studied the question of identity of caloric and movement, and yet have perceived no advancement of this subject; nothing that either has been said or written has helped me towards the conviction I have arrived at. Indeed, almost all the experiments since then have proved this a fact (which formerly no one endeavoured to contest), viz., that immediately movement disappears, heat is produced, reciprocally. We cannot, however, praise too much your studies, and those of the physicists who have followed you, having witnessed the details of this great principle and calculated the different circumstances with the utmost accuracy. All the attempts in giving a plausible explanation of the phenomenon in question have generally conveyed this opinion, that the molecules of bodies, as well as those of ordinary matter, are liable to Newton's attraction, and, consequently, must be separated from each other by spaces infinitely larger than they occupied before; then, they must keep their distances from each other by virtue of laws, if not identical, at least analogous with those which govern the celestial bodies and maintain in fixed boundaries the distances between the planets and the stars, which determine their course.

"Such were the ideas I had, when addressing a letter on this subject to the celebrated and most learned Sir John Herschel, when President of the Royal Astronomical Society, the most distinguished man of our time for his philosophical genius and his wise and judicious manner of treating sciences, well known to me through the connection I have had with him.

"We are indebted, sir, there is not a doubt, to the solid and enlightened spirit that characterises the English learned, for the development made in your country on this interesting question, which occupies the most learned men in the world. I have endeavoured to engage, by numerous papers, the attention of the members of the French Academy of Sciences, my colleagues, upon this important subject, but without avail. But I take the liberty of asking you to communicate this letter to the members of the Institution of which you are a member, and, should I obtain its publication, I hope my studies will be known by all your learned colleagues, and contribute to enlighten the opinion of those who pursue this branch of science.

"The letter I addressed, on the 2nd of April, 1824, to Sir John Herschel has been translated into English, and was sent to Sir David Brewster, who inserted it, on the 24th of

April, 1824, in the 20th number of the *Edinburgh Review* (page 280), in which I expressed an opinion that the cohesion of the bodies constituted on the surface of the earth may be liable to attraction, which will tend to join in their centre of gravity the composed molecules. I also explained the great difficulty I had in attaining this result. Indeed, if the particles of invisible and invaluable bodies could maintain their distances by the same laws as the celestial bodies, the former would necessarily be perceived, for they would be obliged to collect themselves in a symmetrical and regular manner about their centres of gravity, and make their movements apparent to our senses. I have understood since that a law exists, unknown to me, determinating the organised parts of bodies to keep a distance and to preserve the permanent forms they exhibit. Therefore I studied incessantly to solve a question of so great an interest, and in 1836, after twelve years passed in study and meditation, I was fortunate enough to succeed. Indeed, an attentive examination of the manner of action exercised reciprocally by the particles and generally by all the bodies brought this result: If systems of two or several molecules, being comparatively in repose, traverse other systems comparatively in movement, the former separate the latter from another, losing a part of their own movement. Consequently, the question of cohesion was for me implicitly resolved; and, after eleven years' further reflection, I began to publish (in 1848) the series of papers I have read in the Institute, and in which I proved, under the name of Distension, the new result of Newton's attraction applied by myself.

"Then, making myself better acquainted with this law, I published, in 1855, a large volume upon cohesion, and in 1859 another upon the origin and propagation of force, in which I have perfectly explained these principles, trusting to my own proofs and experience. Moreover, I published in 1861, in the form of a letter addressed to M. Tramblay, director of the *Cosmos*, a third paper in reference to the same subject. But yourself, sir, a physicist of the greatest reputation, and who have taken quite a different direction in science not attempted by any other, must know very well how difficult it is, even for those who sincerely wish the triumph of truth, to leave ideas and old practices in which they have grown up. To attain this result we must have confidence in ourselves. New ideas, however clear and judicious they may be, are never adopted immediately, but by degrees, so that those who are occupied with the same question, already explained by their predecessors, fancy themselves the discoverers and authors. After several generations, however, when the tomb is closed above all, historians and commentators will re-establish the truth of the facts and render them due justice.

"Mr. Faraday, your fellow-countryman, gave me that great satisfaction, and his powerful testimony has paid me more than I ever could hope, for the oblivion with which several of my contemporaries wished to cover me, who endeavoured to uphold one another without attaining anything but ridicule, the public always being just and free from prejudice, even if the question is above their intelligence.

"Let us console ourselves, sir, should our efforts rest in obscurity and not be valued as we suppose just; let us be assured that those who will follow us will know how to sift the true from the false, that they will adopt the one and reject the other; and that, if the opinions we have exhibited are just and exact, they will be duly valued one day. But we shall not complain for the present to see our reputation injured by errors we have incurred, perhaps at the first moment, for these errors will have the definitive result of classing us according to merit in the history of science.

"I have the honour to be, with the greatest respect,

"Sir,

"Your most obedient humble Servant,

"SEGUIN, Aîné."

In connection with a conversation on the solutions of different salts with which almost all the waters in England are contaminated, and the practical evils resulting from these impurities to steam-boilers, Mr. Spence stated that various means had been adopted, with only partial success, for preventing incrustation on boilers caused by these impurities. For some years he had adopted a simple plan, which is perfectly successful, both at Goole, where the Aire and Calder Canal has a considerable amount of lime salts, and also at his Newton works, where the water from the Rochdale Canal is not only impregnated with lime salts, but apparently also with sulphate or chloride of iron. The plan is as follows: On the suction part of the water-pipe by which the boiler is supplied, that is, between the force-pump and the canal, a small vessel is placed, capable of containing about two gallons of water; a pipe of half an inch diameter, with a stop cock, communicates from the water-pipe with this vessel; every day the boiler man puts into this vessel 1lb. or 1½lb., as found sufficient, of soda ash, and dissolves it in water in the vessel, and then, when pumping into the boiler, turns the small cock, and in three or four minutes all the solution is taken up and passed through the force-pump into the boiler, and this is daily repeated; the consequence is that not the slightest crust forms on the boilers and no chipping is ever required, the salts being all decomposed and the earthy and metallic bases thrown down as mud, which may be blown off or cleaned out periodically, if the boiler is a flued one. These two boilers are cleaned monthly, and a broom and shovel are the only tools required. The plan is easily adopted, perfectly efficient, and very generally applicable.

Dr. Joule drew attention to the great sacrifice of life by steam-boiler explosions. He believed that, in nearly every instance, rupture took place simply because the iron, by wear or otherwise, had become unable to withstand the ordinary working pressure. Various hypotheses set up to account for explosions were worse than useless, because they diverted attention from the real source of danger. He believed that one of these hypotheses—that which attributed explosions to the introduction of water into a boiler the plates of which are heated in consequence of deficiency of water, was quite inadequate to account for the facts; although weak boilers might be exploded at the moment of starting the engine, in consequence of the swelling of the water through renewed ebullition throwing hot water over the heated plates. The absolute necessity of employing the hydraulic test periodically had been pointed out so frequently that he considered that the neglect of it was highly criminal.

A Paper was read by Mr. Thomas Hopkins, M.B.M.S., "On the Influence of the Earth's Rotation on Winds."

In this Paper the Author treated of the opinion generally entertained by writers on the influence of the rotation of the earth on winds that are passing over its surface, and proposed to show that the atmosphere, in passing from polar towards tropical latitudes, is not left behind by the more rapidly revolving parts, as has been generally maintained by meteorologists. In doing this he showed that air, going from certain slowly revolving polar localities to others that revolve more swiftly, is not left behind in the way that has been generally supposed.

Extracts were given from Kaemtz and Herschel's Meteorology, to show that these writers attribute the western direction of the trade winds within the tropics to the increasing rapidity of the rotating surface of the globe over which they successively pass; and the Author maintained that it was a mere assumption, unsupported by evidence, and was proved by numerous facts to be erroneous.

On the eastern side of the tropical Atlantic, in the Gulf of Guinea, air passes towards the east, not only without being left behind by the great velocity of the surface, but with additional speed, as it constitutes a wind blowing briskly towards the east. In like manner western winds blow on the eastern side of the Pacific ocean within both the northern and southern tropics, and must therefore rotate eastward more swiftly than the surface on which the air presses. It was contended that the cause which determined the air constituting these winds to rotate faster than the surface of the globe that was beneath them, ought to be considered that which makes other winds pass over the surface when they do not move with equal rotatory velocity, and are therefore said to be left behind. We cannot presume that there are two separate causes in operation to create these winds — one making them move faster than the surface which supports them, and the other slower. In the Indian ocean also, as well as among the islands of the great East Indian Archipelago, strong evidence on the subject is to be found. Thus the north-western monsoon, and other similar winds, are not left behind by the more rapidly rotating equatorial surface; and there does not appear to be any reason to doubt that they are made to rotate faster than the surface over which they are passing by the same cause which produces all other winds, including those which are said to be produced by being left behind. It was also pointed out that in both the Pacific and Indian oceans, regular season winds blow; in one season eastward, making the air pass in that direction more rapidly than the surface of the globe, and at another season westward, when the air must move less rapidly than the surface revolves, and is therefore left behind; but the rotation of the surface can no more be supposed to retard one of these winds than that it can accelerate the speed of the other. It was contended that the cause of all such winds is to be found in atmospheric vacua, created by condensation of aqueous vapours and consequent heating and expansion of the gases where the heating takes place. This expansion creates a partial vacuum in the atmosphere, into which neighbouring air rushes to restore the disturbed equilibrium of pressure; and it is this rush of air that constitutes all winds, whether they move in a pirection contrary to the earth's rotation and appear to be left behind, or go with it and rotate faster. The atmosphere appears to press on the surface of the globe with a force that causes the air to move with the surface in its rotation, unless its own uniformity of pressure is disturbed, when gravitation comes into action to restore the equilibrium.

## PHYSICAL AND MATHEMATICAL SECTION.

### November 13th, 1862.

ROBERT WORTHINGTON, Esq., F.R.A.S., President of the Section, in the Chair.

A Paper by Mr. THOMAS HEELIS, F.R.A.S., entitled "Notes of Observations on $\eta$ Argûs," was read.

In this the Author states that from a series of comparisons of this interesting star with others near it, carefully made with the naked eye and with a first-rate opera glass, during a voyage in the South Atlantic and Indian oceans, he constructed the following table:—

| DATE. | SHIP'S PLACE. | | MAG. | COMPARISON. |
|---|---|---|---|---|
| 1861. | | | | |
| Decr. 7, 4 a.m. | 2°45′N | 15°10′W | 3 | Equal to δ Crucis. |
| Decr. 9, 4 a.m. | 0°44′N | 19°58′W | 3·5 | |
| Decr. 24, 9.30 p.m. | 35°54′S | 4°27′W | 3.5 | |
| 1862. | | | | |
| Mar. 21, 9.30 p.m. | 19°15′N | 112°15′E | 3 | Equal to δ but less than γ Crucis. |
| June 18, 7 p.m. | 7° 1′S | 79°20′E | Less than 3 | Less than δ Crucis. |
| June 22 | 14°48′S | 70° 5′E | 3 | |
| June 29, 7.30 p.m. | 24°47′S | 53°56′E | Greater than 3 | |
| July 11, 9 p.m. | 35° 3′S | 25°14′E | Less than 3 | Less than δ Crucis. |
| July 20, 6.37 p.m. | 28°48′S | 9°54′E | 3 | |
| July 21. 7 p.m. | 27°14′S | 8°26′E | Less than 3 | Less than δ Crucis. |
| July 24, 8 p.m. | 22°21′S | 3°17′E | 2·5 | |
| July 27, 8 p.m. | 19°44′S | 0°18′W | 3 | |

A Paper by Mr. THOMAS HEELIS, "On Hydrometric Observations of the Water of the Mediterranean," was read.

From a series of observations, (affected by the observed temperature of the water) made by himself in the Mediterranean, the Author concluded—

That, from the gut of Gibraltar the specific gravity of the water decreases towards the area lying between Sicily and the Morea.

From Cape Malea the specific gravity increases as one proceeds up the Archipelago, the influence of the current from the Black Sea being hardly felt at all to the south of Tenedos.

The creeks and bays of the Greek Islands give water of very high specific gravity, such gravity increasing with the distance from open water and being independent of depth.

In the Mediterranean, water taken from a point 16 feet below the surface always, except in one instance, gave higher hydrometric readings than surface water, and the temperature of water drawn from the lower station was generally higher than that at the surface. In the Atlantic no difference in specific gravity was observed in water taken from the surface and the lower station.

Mr. Baxendell, F.R.A.S., mentioned the following observations made by himself and Mr. Richard Dale, on October 24th, 1862, at Mr. Worthington's Observatory:—

The night was remarkably clear; there was no moon, nor any decided indication of aurora in the north; but there was so much light in the atmosphere that the country around could be distinctly seen, and houses and hedgerows at a distance clearly perceived. This apparent luminosity of the atmosphere continued for two hours, and the night then became very dark, but still remained very clear. Mr. Baxendell afterwards noticed that objects at a distance were seen much more distinctly on this occasion than when the moon was seven days old.

## MICROSCOPICAL SECTION.

November 17th, 1862.

J. G. LYNDE, F.G.S., M. Inst. C.E., in the Chair.

Capt. **RANDALL**, late of the barque "Brazil," forwarded eight soundings, taken on the north coast of the Brazils.

Mr. **THOS. HEELIS** presented a specimen of the *Echeneis Remora*, or sucking fish.

Mr. **J. PARRY** presented a number of cells and rings in cardboard; they were very smooth and sharply cut, without the bur usually produced by punching out cells. Mr. Parry explained they were cut in a lathe, twenty to thirty together, the outside cuttings only presenting an appreciable bur.

Dr. **ROBERTS** called attention to the aid that might be received in the examination of the structure of animal and vegetable tissue by the use of colouring materials. Magenta is peculiarly adapted for this purpose, in consequence of its solubility in simple water and its inert chemical character. The nuclear structures of animal cells are deeply tinted by magenta, and by its use the nuclei of the pale blood corpuscles, of pus globules, of the renal and hepatic cells, and of all epithelial structures, are brought out in great beauty, tinted of a bright carbuncle red. The *red* blood discs are tinted of a faint rose colour, and a darker red speck, not hitherto noticed, is to be observed on the periphery of the corpuscle; it undergoes some changes when treated with tannin and subsequently with caustic potash, but this point is still under investigation.

Dr. Roberts exhibited drawings and mounted specimens to illustrate his views.

Mr. **JOHN LEIGH**, M.R.C.S., exhibited a case of microscopical dissecting instruments, by Messrs. Wood, of Manchester, which were highly approved of for completeness and finish.

Mr. **THOS. H. NEVILL** exhibited, with dark ground illumination, some fine specimens of *Conochilus volvox*.

## Ordinary Meeting, December 30th, 1862.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Mr. Samuel Ogden was elected an Ordinary Member of the Society.

Mr. T. T. WILKINSON, F.R.A.S., said that, in a Paper "On the Burnley Coal Field," prepared by Mr. Joseph Whitaker, of Burnley, and himself, and read before the Geological Section of the British Association, at Manchester, they gave a sketch of the principal mines in this district, but they purposely omitted all mention of those whose thickness did not exceed one foot. There are, however, one or two of these thinner mines which may hereafter become worthy of notice, and hence he offered the present note as an addition to Mr. Hull's valuable synopsis in pp. 133-4 of his "Coal Fields of Great Britain."

### SECTION OF STRATA NEAR WORSTHORNE, BURNLEY.

| | | | |
|---|---|---|---|
| | Strata, composed principally of blue clay, followed by light metals | 102 | feet. |
| 1. | *Coal*, the "China Bed" | 2 | ,, |
| | Strata, consisting chiefly of grey rag and metals | 39 | ,, |
| 2. | *Coal*, the bed not named, and overlaid by about *three inches* of cannel, together | 1⅓ | ,, |
| | Strata, composed mainly of dark rag and metal seating | 73 | ,, |
| 3. | *Coal*, the "Danby Bed" | 2¾ | ,, |
| | Strata, consisting of rag, *light blue rock*, metals, and black shales | 126 | ,, |
| 4. | *Coal*, the Arley, or Habergham mine | 4½ | ,, |

The bed (2) is the one which has been hitherto omitted; and, if of no other value at present, it may be useful for coördination and identification with the seams of coal in other localities.

A Paper was read by EDWARD HULL, B.A., F.G.S., "On the New Red Sandstone and Permian Formations as Sources of Water Supply for Towns."

The Paper commenced by pointing to the advantage enjoyed by most of the large towns of the central counties from their geological position, when built on the new red sandstone. First, from being in proximity to coal; second, from having a dry foundation; third, from having easy access to building stone; and, fourth, from the fact of their resting upon natural reservoirs of water stored up in the sandstone itself. The Author considered that many of these towns had not taken full advantage of this last-named source of water supply, partly from a distrust of the resources of the rock, and partly from the failure sometimes sustained in consequence of the positions for the wells having been selected without a proper regard to the geological structure of the country.

The excellence of the new red sandstone and the lower permian sandstone as sources of water supply, was shown to depend upon three qualities. 1. Their porousness; 2. Homogeneity, or uniformity of structure and composition; 3. Filtering powers. Each of these were treated of in detail, and with examples from several wells in Lancashire and Cheshire. The Author then referred to the failure of the attempts to obtain fresh water at Rugby, and a sufficient supply at Wolverhampton; and he maintained that an abundant supply might have been found at the latter town had the position for the well been selected with due regard to the geological structure of the country.

The Author then proceeded to lay down certain rules of general application for the selection of proper sites for wells, and illustrated the subject by reference to a well now being sunk under his direction at Whitmore, for the supply of the railway works and town of Crewe. The position of the well is in a *trough*, both geographically and topographically, and on a four-inch bore hole being sunk to a depth of 148 feet the water ascended to the surface with a head of four feet, and has continued to flow without diminution for the last six months. The well, which is being made within 100 yards of the bore hole, has only reached a depth of 60 feet, but already yields 250,000 gallons per day.

In conclusion, the Author expressed his conviction that the question of water supply from the strata was becoming every day more a geological one, just as was the discovery of coal beneath the formations which overlie the coal-measures.

## Ordinary Meeting, January 13, 1863.

E. W. Binney, F.R.S., F.G.S., President, in the chair.

The President said:—In a very valuable work lately published by that eminent geologist Dr. Geinitz, of Dresden, entitled, "Dyas, or the Magnesian Limestone Formation and the Lower New Red Sandstone," the Author, whom I have had the pleasure of accompanying over some of the permian deposits of South Lancashire, has done me the honour to allude to two Papers of mine, "On the Permian Deposits of the North-West of England," printed in Vols. XII. and XIV., Second Series, of the Society's Memoirs.

At page 313 of the above work the Author says, "Through Mr. Binney we have become acquainted with true Rothliegende, and indeed of its upper portion in the region of the lower red sandstone of the north-west of England. That accurate observer mentioned to me that the reddish gray sandstone underlying it, and which is very similar to the lower red sandstone of the north-east of England, contains plants of the coal measures, and that it occupies even a lower position than certain limestones of the coal measures which are rich in ichthyolites. I have myself seen from the reddish sandstone of Astley, near Manchester, *Calamites approximatus* Schl., and *Calamites Suckowi* Brongn.; from the red shales at Ardwick, Manchester, *Sagenaria dichotoma* Sternb., *Calamites Suckowi* Brmg., *Sphenopteris irregularis* Sternb., *Sphenopteris coralloides* Gutb., *Dictyopteris neuropteroides* Gutb., *Cyatheites villosus* Brmg., *Cyatheites oreopteroides* Göpp., and *Alethopteris lonchitides* Sternb., all true coal plants."*

No one more than the learned Author, I am sure, would like any mistake to be corrected. Now, in the statement that "the reddish gray sandstone underlying it (the true Rothliegende), and which is very similar to the lower red sandstone of the north-east of England, contains plants of the coal measures," the Doctor is quite correct; but when he proceeds to state "that it (the true Rothliegende) occupies even a lower position than certain limestones of the coal measures which are rich in ichthyolites," the Author has misunderstood me by apparently confounding the ribbon beds of limestone in red marls lying above the pebbly beds at Astley with the upper carboniferous limestones at Ardwick, and then stating that such pebble beds occupy an inferior position to the latter. It is certain that the Astley pebble beds containing coal plants occupy a higher geological position than the red shales which the Doctor truly states also contain true coal plants. How much higher it is impossible to say, as it is most probable there are higher carboniferous strata than those yet seen at Ardwick, and there may be permian strata lower than those up to this time met with at Astley — at present we cannot tell.

As the permian group of strata is now occupying the attention of geologists both in England and on the Continent, the following table of the beds in the north-west of England, with their approximate thicknesses, as seen at Shawk, west of Carlisle, Westhouse, south of Kirkby Lonsdale, and Manchester, in the descending order, may be acceptable.

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* For the translation of this extract I am indebted to the kindness of Mr. Ekman.

| | SHAWK. | WESTHOUSE. | MANCHESTER. |
|---|---|---|---|
| | Feet. | Feet. | Feet. |
| 1.* Laminated and fine grained red sandstones | 300 | not seen. | not seen. |
| 2. Red and variegated marls containing sometimes, but not always, beds of limestone and gypsum, with fossil shells of the genera *Schizodus*, *Bakevellia*, &c. | 150 | traces of them seen. | 300 |
| 3. Conglomerate | 4 | 300 | 50 |
| 4. Lower new red sandstone, generally soft and incoherent | 7 | 500 | 500 |
| 5. Red shaly clays | not seen | 250 | not seen. |
| 6. Astley pebble beds, containing common coal plants, termed by me lower permian | not seen. | not seen. | 60 |

Mr. SPENCE brought under the notice of the society a specimen of the metal magnesium. This metal has hitherto been produced only in small quantities, by means of sodium. The specimen shown derived its interest from the fact that it is stated to have been produced by direct reduction, either from the oxide or some of its salts.

If this information, which has been obtained through a friend of the inventor, proves correct, it is a matter of considerable importance, as the metal has most valuable qualities.

Professor ROSCOE read the following extract from a letter which he had just received from Professor Bunsen, respecting the properties of metallic rubidium:—"I have prepared metallic rubidium by reduction with carbon in an iron vessel; from 75 grammes of the bitartrate of rubidium I obtained 5 grammes of the metal in coherent masses, one of which weighed 3 grammes. Rubidium closely resembles potassium in its properties, its vapour possesses a greenish blue colour, the specific gravity of the metal is 1·65, it is more electro-positive than potassium, and it melts at the very low temperature of 38·5° C."

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* The first four strata of the above series, Professor Harkness, F.R.S., in a fine natural section seen at Hilton Beck, north of Brough, estimates to be of 3000 feet in thickness.—*Quarterly Journal of the Geological Society* for August, 1862.

In reference to the age of the New South Wales coalfield, Mr. **Edward Hull** stated that he had received letters from the Rev. W. B. Clarke, who has for many years been engaged in its exploration, and from Mr. John Mackenzie, who has had considerable experience as a mining surveyor in Wigan and North Wales. It is well known that Mr. Clarke maintains the Palæozoic age of the carbonaceous deposits of New South Wales, in opposition to Professor M^c·Coy, who holds that they are of more recent formation (Mesozoic). As Professor M^c·Coy has never actually visited the New South Wales coalfield, and derives his information from cabinet specimens, men of science will probably prefer the evidence of one who has spent years in personally exploring and collecting from the beds themselves. In Mr. Clarke's Memoir on the "Recent Geological Discoveries in Australasia" (2nd edit.), the author defends his view of the Palæozoic age of the coal-bearing strata; and in the letter from Mr. Mackenzie, the writer gives the following series of fossiliferous strata *overlying* the coal and cannel belonging to the Hon. B. Russell, which, if correct (as there is every reason for supposing), ought to set the question at rest in favour of the true Carboniferous age of the coal-measures. He states—"In a pit above this coal are strata with *Fenestella, Stenopora, Orthonota costata, Spirifer, Producta, Terebratula,* &c. In a pit about 100 feet below the same coal occur *Spirifer, Producta, Conularia,* and vegetable impressions; about 60 feet lower, *Spirifer, Terebratula, Pleurotomaria, Stenopera*; and similar shells, accompanied by vegetable remains, are stated to occur still lower. Mr. Mackenzie promises to send specimens of *Lepidodendron* and *Sigillaria* from the same beds.

Dr. **Joule** described a peculiar kind of mirage which he had witnessed from the northern extremity of Douglas Bay, Isle of Man, and at an elevation of 20 feet above the surface of the water. At about 9 a.m. a number of chimneys in the town were fired, and the products of combustion were driven out to sea by a very gentle breeze. Presently the wind changed and drove the smoke at right angles to its former track. The steamer for Liverpool had in the meantime attained a distance of about three miles, and, although somewhat obscured, was distinctly visible through the smoke. Noticing something extraordinary in her appearance, he viewed her through a telescope, and then observed that nearly the whole of the hull was obscured by the horizon, the uppermost part of the paddle-boxes, the bowsprit, and tafferel being alone visible, whilst at the same time the masts and funnel appeared considerably elongated. Quarter of an hour afterwards, when the smoke had cleared away and the steamer was about seven miles distant, the hull was seen as usual, quite unobscured by the horizon. Dr. Joule attributed the phenomenon to a stratum of highly refractive gases, lying about 20 feet above the level of the sea and gradually diluted and diminished in extent on its upper and lower sides, so as to produce the effect of a convex lens.

Dr. **Angus Smith** said: I saw once a very remarkable instance of the diversion of the rays of light from the straight line. Although fogs are common on our hills, I have met no instance of similar exaggeration of effect among us. I went with some friends up Skiddaw, and near the top entered a cloud which prevented us from seeing many yards before us, although it was not extremely dark. When moving over those loose stones which form a highway to the summit for a considerable distance, we observed a building which appeared to us about fourteen feet high. The side towards us seemed to be a wall nearly square, and we took it for granted that the top of the hill was attained and that the foremost of the party would be found there; but as we approached the building sank, and in the course of a few steps it went downwards until we found that there were only three layers of stones not very thick, and the whole under two feet high. The disappearance must have occurred within the space of twenty feet; my present impression is that it was less. Further on we attained the man at the summit, and the cloud became denser; we could not see many feet, and I did not think it safe to move from the spot for some time, not knowing the locality. Sitting there, I saw four perpendicular lines on the cloud; I looked up and found that they terminated in the body of an animal which was moving slowly towards us. The distance could not be measured, except by the number of steps taken by the animal, and from these I concluded that when first seen it was from twelve to twenty feet from us; I looked on twelve as being most probable. I do not exaggerate when I say that the height was in appearance thirty feet. Wondering what was to be the end of this strange vision, I called the attention of all, but the animal diminished in size so rapidly that only one or two, who instantly attended to my call, could perceive the monstrousness of the exaggeration of form now presented by a moderately-sized pointer. On coming up to the more advanced portion of our party, they were enlarged and distorted, as we often hear described; but such effects are comparatively common, and they are not to be compared with the two instances mentioned. The first was seen with my face towards the sun nearly. It was about midday. The second phenomenon was seen twenty minutes later, when looking nearly north.

Although I have lived much in sight of Morven, where one might expect to see the "spirit of the mist on the hills," I had not seen anything similar before, nor have I gained from the shepherds that such things are common. Perhaps if they were common, Ossian's ghosts would be less imposing.

## MICROSCOPICAL SECTION.

15th December, 1862.

Mr. J. G. Lynde, F.G.S., M. Inst. C. E., in the chair.

Mr. Alfred Fryer was elected a member of the Section.

Captain Moodie, of the R.M.S.S. "Canada," presented two soundings taken off Nova Scotia. Captain Thomas Millard, of the barque "First of May," presented specimens of anchor mud from Montego Bay and the harbours of Kingston and Port Royal, Jamaica; also a sounding from the banks of Newfoundland.

The Chairman stated that he was led to pursue Dr. Roberts's suggestions on the use of magenta dye in examining tissues. From experiments made since the last meeting of the Section, he finds that the dye has no power to colour living tissue, whether animal or vegetable, but that as soon as life is extinct the action of the dye commences. He is continuing the experiments, which are of a most interesting character, and he hopes to lay the result before the next meeting of the Section.

Mr. Leigh considered it probable that so long as vital action continued, ordinary endosmosis could not take place.

Mr. Mosley said that Mr. Hepworth had frequently tried magenta for injections, and the results were not satisfactory, as the colouring matter diffuses itself through the whole of the tissues, giving an appearance of dyed flesh rather than that of injected preparations. This appears to confirm the preceding observations, and to account for the accumulation of colour where the integument is thickest, by means of which Dr. Roberts discovered the spot on the red blood discs, as announced at the previous meeting.

Mr. Leigh drew the attention of the Section to the adulteration of size as a cause of mildew in cotton goods.

Mr. Watson named the investigations made by Mr. Thompson about twenty-five years ago, as to the cause of mildew in madder purple-printed cottons shipped to hot climates. It was attributed to the starch employed in finishing the goods, which, acted upon by moisture, heat, and pressure, had given rise to an organic acid which discharged the colour.

Mr. HURST described his experience of mildew on printed cottons and upon dyed fustians, at Gibraltar and Calcutta. In most cases it appeared in spots and round patches, which affected the colours. On the fustians, he had no doubt it was caused by the growth of a fungus, as the surface of the spots was sensibly raised.

Mr. MOSLEY considered there might be several kinds of mildew; that upon the fustians might be attributed to the bone size with which those goods were generally finished, and known by the characteristic smell. Mr. Mosley also exhibited a pattern of grey calico, which had become discoloured and quite rotten in irregular patches, from mildew; it had lain for some time in a damp place, under pressure; there was this peculiarity about it, that the coloured patches whilst damp were quite tender, but on exposure to the air and drying, the cloth had recovered its strength.

Mr. HEYS remarked that twenty years ago he was engaged in the manufacture of fine muslins; it was usual to soak the weft in soap suds to facilitate the weaving; and it was found the cloth was most liable to mildew during hot, close, summer weather, and the greater the quantity of goods heaped together the more rapidly would mildew set in. The flour from which the size was made was always the best that could be purchased.

Mr. HEYS exhibited mounted specimens of the fibres of the Zôstera Marina, and stated that, as the fibre is considerably finer than the finest Sea Island cotton, it might probably be of use in the manufacture of fine muslins for ladies' dresses, if possible to obtain a supply, and separate the fibre from the plant by machinery. Mr. Heys also exhibited mounted specimens of Queensland cotton, lately sold at five shillings per pound; the fibre is very regular in size and much more cylindrical than other cottons; also several specimens from ripe and unripe pods, and Mr. Heys expressed the opinion that great advantage would arise from a regular and careful examination of cotton fibre taken fresh from the plant through every stage of growth.

## Ordinary Meeting, January 27th, 1863.

E. W. **Binney**, F.R.S., F.G.S., President, in the Chair.

The **President** said that during the last few years the MS. papers of the late Mr. Sturgeon, the electrician, and Professor Eaton Hodgkinson, F.R.S., had been presented to the Society, and were now deposited in its archives. The Council had resolved to collect, as far as possible, all MSS, by the late illustrious President of the Society, Dr. Dalton. He thought that this Society was the most proper depository for the scientific papers or correspondence of any of its deceased members. As it was probable that members and other parties might possess papers and correspondence of the late Dr. Dalton, he hoped that they would be induced to present them to the Society. He was sure that their excellent Librarian would devote great care in cataloguing and arranging the papers, and that they could not be in better custody than in the Society with which that illustrious philosopher had been connected for so long a period of his life.

Mr. **Baxendell**, referring to the nebula discovered by Mr. Hind, in 1852, in the constellation *Taurus*, but which has since disappeared; and to the variable star near its north following edge, stated that he had lately found that another star, distant only about nineteen minutes of arc from the place of the nebula, was also variable. His observations of the brightness of this star were as follow:—

1862.—March 19. = 10·4 magnitude.
October 18. = 10·1 ———
November 15. = 9·9 ———
December 26. = 9·6 ———
1863.—January 23. = 9·2 ———

These numbers show an apparently progressive increase of brightness; but it is not improbable that a minimum occurred during the summer months, at which time no observations could be made, owing to the star's proximity to the sun; and therefore the actual range of variation may have been greater than that indicated by these observations. This star is No. 705, Zone + 19°, of Professor Argelander's *Bonner Sternverzeichniss*, where its magnitude is stated to be 9·4. Its approximate mean place for 1860·0 is

R.A. 4h. 13m. 40s. Declination north, 19° 28′·9.

According to Professor Argelander's system of nomenclature this variable will be called U *Tauri*.

Mr. Baxendell also communicated the following observations of the variable star near the edge of the nebula (T *Tauri*):—

1862.—March 19. T = 10·3 magnitude.
October 18. below 12·5 ———
November 15. = 12·6 ———
„ 16. = 12·6 ———
December 26. = 12·7 ———
1863.—January 23. = 12·1 ———

From the last observation it would appear that T *Tauri*, after being at a minimum for some time, is now brightening up again; and judging from the course of its changes in the early part of last year, a maximum may be expected to occur within the next two months.

A Paper, by Mr. Thos. Heelis, "On Swell Observed at Sea, particularly in the Regions of the South-east Trade Wind," was then read, in which the Author, after examining the causes of swell and giving instances and especially alluding to the rollers at Ascension, St. Helena, and Tristan d'Acunba, gave some instances of the observed succession and order of the undulations in altitude and of the speed of translation of the crests, which in one instance he had found to attain twenty-two miles per hour; and, after setting forth a table of different instances of swell chiefly in the Atlantic and Indian Oceans, and Bay of Bengal, addressed himself to the consideration of the swell which is frequently observed in tropical latitudes in the southern hemisphere to roll up from the southward. This he attributed to the action of currents setting especially in the Indian Ocean from the neighbourhood of the southern tropic, towards the great connecting current which extends from the Cape of Good Hope to Australia. The Author gave instances of these currents experienced in

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|---|---|---|---|---|---|---|
| 35°47′ S. | 79°10′ E. | running | south | 27 | miles | in 24 hours. |
| 28°22′ S. | 81°13′ E. | „ | „ | 27 | „ | „ |
| 26°42′ S. | 81°45′ E. | „ | „ | 23 | „ | „ |
| 23°38′ S. | 83°10′ E. | „ | S. 50° E. | 42 | „ | „ |

and called attention to the difference between them as actually experienced, and the set of the water as laid down upon general current charts, the peculiar circulation which they shewed to exist at times in the Indian Ocean, and the meteorological disturbances which the causes which set such currents in motion cannot fail to originate.

## Ordinary Meeting, February 10th, 1863.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Mr. G. Stanley Darbishire was elected an Ordinary Member of the Society.

Among other donations which received the thanks of the Society, Mr. Parry presented a volume of MS. Lectures on Chemistry, by Mr. Thomas Henry, one of the founders of the Society, and the father of the late Dr. William Henry, the celebrated chemist.

Dr. Joule described a barometer for measuring small atmospheric disturbances. It consists of a large carboy connected by a glass tube with a miniature gasometer formed by inverting a small platinum crucible over a small vessel of water. The crucible is attached to the short end of a finely suspended lever multiplying its motion six times. When the apparatus was raised two feet the index moved through one inch; hence he was able in serene weather to observe the effect corresponding to the elevation of less than one inch. The barometer is placed in a building, the slated roof of which affords, without perceptible draught, free communication with the external atmosphere. In this situation it was found that the slightest wind caused the index to oscillate, a gale occasioning oscillations of two inches, an increase of pressure being generally observed when the gusts took place.

Dr. Clay exhibited a specimen of the Snake Nut, the fruit of the *Ophiocaryon paradoxum (Schomb.)*. Nat. ord.

Sapindaceæ, from British Guiana. For a description of the Snake-Nut Tree, see Ann. Nat. Hist., vol. v., p. 202 (1840).

Mr. JOSEPH SIDEBOTHAM exhibited a cornelian pebble with a cavity containing a globule of water, brought from the coast of Tuscany. These pebbles are often picked up on the shore, and when broken exhibit a crystalline cavity about one-third filled with water.

Mr. DYER read a Paper entitled "Notes on the Introduction of Steam Navigation."

Mr. Dyer stated that this subject, being of great importance, had engaged many able pens in tracing the origin of the several inventions and experiments that preceded the final triumph of steam power over that of wind for navigating ships; each writer claiming the honor of priority for his own country. It may be useful to state the order in which, and the parties by whom the principal attempts were made to realise that object. Several letters lately appeared in the *Times*, and were thence transferred to the pages of the *Engineer*, giving a graphic account of the "first steamer in English waters," the "*Margery*, built at Dumbarton, by the late William Denny, for William Anderson, of Glasgow, and passed through the canal to the Forth and thence to the Thames, where she arrived on the 23rd January, 1815." On the authority of Mr. Anderson, then, this date is fixed when the first steamboat was seen on English waters. The first steamboat, the *Claremont*, was started as a regular packet on the Hudson River, in the spring of 1807; so that the first steamer seen on the American waters was fifty-five years ago, a lapse of time that should now insure a calm view of the steps that led to this first actual success in steam navigation. It will be shown that, by a long course of persevering labours, the honor of that success must be conceded to Robert Fulton, by whom it was achieved.

Whilst admitting the merits of other ingenious men long engaged in the same pursuit, it is clearly proved that, either from good fortune, or by the exercise of superior judgment and skill, the race was won by eight years' priority of steam navigation, by Fulton, on the Hudson River.

In 1793, Mr. Fulton sent his plan for a steamboat to Lord Stanhope, who approved of, and thanked him for the communication. Shortly after, Fulton went to Paris, and made experiments on the French waters, with the chain floats, the duck's-foot paddles, the screw or smoke-jack propellers, and with the paddle wheels, to which latter he gave the preference, and constructed a boat with them in 1803, which was the model adopted in building the *Claremont* in 1806.

Mr. Dyer had sailed in the *Claremont*, and remembers the sensation created by her appearance, and the high admiration bestowed on the author of so great an enterprise. That sensation in 1807 was precisely the same as the *Margery* created among the vessels on the Thames in 1815.

All attempts at steam navigation were fruitless before the invention of Mr. Watt's steam-engine, his engine being the *first* that could be usefully applied to rotative machines on *land*, and therefore for propelling ships. The principal claims put forth by other inventors of steamboats, are the following:—

In France, the Marquis de Jauffroy constructed a steamboat at Lyons, in 1782, "with paddle wheels," but that this boat did not succeed is obvious, because she was not heard of until 1816, when the first Fulton boat was started to run on the Seine.

In 1783, Daniel Bernoulli proposed a plan which consisted of forcing water through a tube, out at the stern of the boat. This scheme has been tried many times since, but fails on account of the defective principle of applying the force. *Endless chains*, with float propellers, have been many times tried and have failed on the same ground.

In 1795, Lord Stanhope made experiments with a boat on the Thames, using the reciprocating or "duck's foot" paddles, which also failed, from the loss of time and power by the return stroke.

In 1785, James Rumsey, of Virginia, tried a boat on the Potomac, and afterwards in London, both without success; and about the same time Mr. Fitch, of Philadelphia, tried one with paddle wheels, on the Delaware, but this boat also did not succeed and was given up as a failure. J. C. Stephens, of New York, made experiments in 1804, with a "boat 25 feet long and 5 feet wide," which of course did no good and was stopped as a failure, though again brought to notice as preceding Mr. Fulton's.

In 1788 and 1789, William Symington, in conjunction with Patrick Millar and James Taylor, made experiments with their patents for navigating by steam, and in 1802 commenced running a boat on the canal at Glasgow which made three miles an hour, but after many changes of her propellers and trials, the scheme was given up and no more was heard of the steamboat of Mr. Symington until long after those of Fulton were widely spread over the American waters.

In 1816, the Marquis de Jauffroy complained that the Fulton steamboat on the Seine had taken the "paddle wheels" invented by him and used at Lyons thirty-four years before, but also abandoned by him. To this charge Mons. Royou replied in the *Journal des Debats*, thus:—"It is not concerning an invention, but the means of applying a power already known. Fulton never pretended to be an inventor in regard to steamboats in any other sense. The application of steam to navigation had been thought of by all artists, but the means of applying it were wanting, and Fulton furnished them."

The first ocean steamer was *The Fulton*, of 327 tons, built in 1813, and the first steamer for harbour defence was built under Fulton's direction, 2,470 tons, launched in 1814. This became the model ship for the iron-clad batteries and rams since constructed with many changes. It will be seen by the drawings of Fulton's plans, that he had tried the several other kinds of propellers—the chain float, duck's foot, and the screw fan—before adopting the paddle wheel, for though the *screw* was good in principle, it was many years before it could be constructed to act efficiently. The *James Watt*, was the first boat with the screw running between London and Havre, about ten years after the advent of *The Margery*.

In 1811 I endeavoured to introduce steam navigation into England, but I found a strong conviction that it would not answer in this country, our most eminent engineers saying, "We don't doubt the success of steam-boats in the wide rivers and harbours of America, but in our comparatively small rivers and crowded harbours they will never answer." Even such scientific engineers as the late John Rennie, sen., and Peter Ewart, a Vice-President of this Society, both advised me to relinquish the attempt to introduce steamboats, as sure to prove a waste of time and money to no purpose. However, when conviction came over the public mind that steam navigation would answer here—but not until after more than 5,000 tons of steamboats had been launched on the Hudson in 1816, did it so come—then began the spread of steam navigation, since extended with such marvellous rapidity and perfection as to atone for the sluggish beginning. Since nations are indebted to the genius of Watt for success in using steam power, to that of Fulton for its successful application to navigation, to Stephenson for the like success on railways, the meed of praise due to each of their names should be cheerfully awarded by all who are so largely benefited by the result of their labours. In doing this we should bear in mind, that *inventions* do not spring into existence perfect from their birth, like Pallas from the brain of Jupiter, but they come from the prior labours of many brains, and he is the true inventor who first collects the essence of and gives the stamp of vitality to those labours. In this sense the invention of steam navigation will for ever illustrate the name of Robert Fulton.

## MICROSCOPICAL SECTION.

19th January, 1863.

Mr. JOSEPH SIDEBOTHAM, Vice-President of the Section, in the Chair.

Captain ISAAC TESSYMAN, of the ship "Ann Mary," presented four soundings off the coasts of Patagonia, Singin, Malay Peninsula, and at Algoa Bay, taken during his late voyage to San Francisco, Singapore, &c.

Captain J. B. HUSBAND, of the ship "Mutlah," presented three soundings taken off Orissa, and the Black Pagoda on the coast of Bengal.

MR. LATHAM presented mounted slides of the skin of the Muræna Guttata, cocoon of silk worm, and cuticle of Gladiolus.

Mr. JOHN SLAGG, jun., presented mounted specimens of floats and ovaries of Ianthina; shells of ditto; berry of Fucus natans covered with Membranifera; and Cresia aciculata, a Pteropod.

Mr. JOHN HEPWORTH referred to the mildew mentioned in the proceedings of the previous meeting, which had been observed at Gibraltar on fustians stiffened with bone size, and stated that he had noticed a peculiar fungoid growth upon mounted sections of bone, and that it would be desirable if possible to compare them.

Mr. JOHN LEIGH, M.R.C.S., read a Paper "On the Use of Dialysis in Microscopical Investigations."

After describing the researches of Professor Graham, the present Master of the Mint, and explaining the nature of the division into two classes of all natural bodies, namely, into crystalloids and colloids, their affinities and means of separation, the Author proceeded to describe some curious bodies found in cleaning out a steam boiler, which have almost exactly the external form and internal structure of the concretions formed by Mr. Rainey, and figured in Dr. Carpenter's work on the Microscope, third edition, page 769. They are composed chiefly of carbonate of lime and organic matter aggregated in the presence of the aluminous colloidal mud in the boiler, and are, on a large scale, a singular illustration of Mr. Rainey's experiments. The Author then enlarged upon the advantages of this method of investigation to the microscopist and chemist, who may go hand in hand in the examination of the crystalloidal constituents of organic bodies. "The microscopist (says the Author) will often be able to direct his fellow worker into new channels of research. A careful study of minute crystals, with accurate measurements of their angles and observations on the effects of polarised light, may, to speak medically, lead to an accurate diagnosis of them, as is afforded to the tests of the chemist, to whose larger operations they may be referred for further analysis. Dialysis affords us the means of separating the saline constituents of the juices of plants from the salts fixed in their tissues, and similarly in regard to animal bodies. By one or more dialytic operations on a limited scale the crystalloids of any vegetable juice may be obtained in solution of great limpidity, and by careful evaporation over a water bath they will crystallise out in a state fit for examination." Mr. Leigh concluded his Paper by the quotation of some apposite remarks by the late Professor Johnstone, of Durham, and exhibited the small trays he uses in his experiments, consisting of a double rim of gutta percha securing a disc of parchment paper in the form of a sieve; also specimens of the mulberry-shaped nodules found in a steam boiler as before named.

The **Chairman** said that for minute experiments he had used the parchment paper in the form of a filter.

Mr. **Dancer** stated that porous earthenware could be advantageously used as a dialyser.

Professor **Williamson** indicated a number of subjects upon which dialysis would probably throw light, both in vegetable and animal physiology. He especially dwelt upon the phenomena of calcification and silification, illustrating his remarks by reference to what occurs in the formation of calcareous and silicious growths in the colloid sarcode of sponges and polypifera, in the development of the dental plates of the teeth of Echinus, in the calcification of the derms of the crustacea, the shells of mollusca, the scales of fishes, and in the chondriform and membraniform bones and teeth of the vertebrate animals. The Professor suggested that a natural process of dialysis probably underlay all these formations. He specially called attention to the close resemblance subsisting between the primary spherical and concentric granules seen in the derms of the crustacea, in the scales of cycloid and ctenoid fishes, in the outermost layers of many teeth, and the artificial concretions produced by Mr. Rainey, to which Mr. Leigh alluded in his Paper. Professor Williamson further suggested for inquiry, how far the structureless basement membrane seen underlying the calcareous layer of many calcified structures (*e.g.* the pulp membrane of the tooth) played some part equivalent to the parchment dialyser of the Master of the Mint.

Mr. **Mosley** read extracts from a Report to the Cotton Supply Association, of a microscopical examination of a sample of cotton supposed to have some peculiarities. On comparison with good American cotton it was found to contain a greater proportion of round and partially flattened filaments, all more or less twisted, but full and well developed; the polarised colours were more bright and vivid, all indicative, he considered, of strong and vigorous growth in a congenial soil, and careful gathering when the pod was at its highest stage of development. The fibres varied in size, from flattened ribbons of $\frac{1}{80}$ of an inch broad to cylindrical fibres of $\frac{1}{1500}$ of an inch in diameter; the variation being due mainly to the amount of compression of the cylinder rather than to actual difference in bulk. The staple measured from 1 inch to $1\frac{1}{4}$ inch in length. The contrast with some inferior cottons was strongly marked, as regards their twisted, flat, tape-like appearance, and faint polariscopic colouring, which he attributed either to weakly growth or to having been picked from over-ripe pods, when the fibre had become dry and sapless. Too little is, however, known to form an exact opinion; dissection of buds and pods in all stages of growth would be necessary for a full and exhaustive investigation of the subject.

In reply to a question from Dr. Roberts, Professor **Williamson** stated that, like all vegetable hairs, the cotton fibre in its early stage is unquestionably cylindrical.

Mr. **Sidebotham** exhibited a convenient and effective form of binocular microscope, by Mr. Dancer, suitable for naturalists and others.

Mr. **Brothers** exhibited a mounted slide of Foraminifera, and a drawing of Colochœte Scutata, a minute fresh-water alga.

Mr. **Whalley** exhibited Trichoda lynceus, marine infusoria; also an objective, $\frac{1}{25}$ of an inch focus, by Messrs. Powell and Lealand.

## Ordinary Meeting, February 24th, 1863.

E. W. **Binney**, F.R.S., F.G.S., President, in the Chair.

Among other donations for which the thanks of the Society were voted were an extensive collection of Dr. Dalton's manuscript correspondence, presented by Dr. W. C. Henry; and a framed photograph of Professors Bunsen and Kirchhoff, presented by Mr. H. Petschler.

Professor **Roscoe** stated that he had been for some little time, and is still, engaged in an interesting examination of the spectrum produced by the flame evolved in the manufacture of cast steel by the Bessemer process, on the works of Messrs. John Brown and Co., of Sheffield.

The spectrum of this highly luminous and peculiar flame exhibits during a certain phase of its existence, a complicated but most characteristic series of bright lines and dark absorption bands. Amongst the former the sodium, lithium, and potassium lines are most conspicuous; but these are accompanied by a number of other, and as yet undetermined bright lines; whilst among the absorption bands those formed by sodium vapour and carbonic oxide can be readily distinguished. Professor Roscoe expressed his belief that this first practical application of the Spectrum Analysis will prove of the highest importance in the manufacture of cast steel by the Bessemer process, and he hoped on a future occasion to be in a position to bring the subject before the Society in a more extended form than he was at present able to do.

A communication was likewise made by Professor **Roscoe**, concerning "The Existence of a Crystallizable Carbon Compound and Free Sulphur in the Alais Meteorite."

Through the kindness of R. P. Greg, Esq., of Manchester, I was placed in possession of about a gramme and a half of this peculiar meteorite, which fell near Alais, in France, on the 15th March, 1806, and was examined by Berzelius in

1834. This distinguished chemist states* that the Alais meteorite is remarkable as containing an organic carbon compound, soluble in water, which turns brown on heating, deposits a black carbonaceous mass, and burns without residue. In the year 1860, Wöhler† discovered the presence of small traces of a crystallizable hydrocarbon, soluble in alcohol and ether, in two meteorites, one of which fell at Kaba, in Hungary, on the 15th April, 1857, and the other at Bokkevelde, in South Africa, on October 13, 1838. The fact thus undoubtedly proved of the existence in these two meteorites of crystallizable carbon compounds, which in terrestrial matter are solely the results of vital action, rendered a further confirmation of the existence of organic matter in the Alais meteorite of special interest.

In general appearance the small fragments of the meteorite experimented upon coincided exactly with the minute description of the substance given by Berzelius; the white efflorescence which covers the surface of the mineral was found to consist mainly of small crystals of sulphate of magnesium; the only other bodies which could be detected by spectrum analysis were soda and lime. Iron was not contained in the soluble salts. On extracting 1·0583 gramme of the meteorite with water, 0·1155 gramme of soluble salts was dissolved, corresponding to 10·91 per cent, and thus closely agreeing with Berzelius's estimation of 10·3 per cent.

Ether was found to dissolve from the residue 1·94 per cent on the original meteorite, a substance which on evaporation was deposited in distinct crystals. The crystals possessed a peculiar aromatic odour, and melted at 114° C., subliming on heating, and leaving a slight carbonaceous residue. Under the microscope the crystals were seen to be of two forms, one acicular, the other rhombic. The acicular crystals were difficultly soluble in absolute alcohol, but easily soluble in ether, bisulphide of carbon, turpentine, and cold nitric acid, and dissolved in sulphuric acid with formation of a brown colouring matter; the rhombic crystals likewise dissolved in ether and bisulphide of carbon, but were unaltered by cold nitric and sulphuric acids, or turpentine. The etherial extract gave no reaction for sulphuric acid, but, after boiling with nitric acid, a copious precipitate of sulphate of barium was deposited. When burnt in a stream of dry oxygen gas, 0·0078 gramme of the extract, dried at 100° C., yielded 0·010 gramme of sulphurous acid, 0·008 gramme of carbonic acid, and 0·003 gramme of water. Hence the meteorite contained 1·24 per cent of free sulphur, 0·54 per cent of carbon, and 0·1 per cent of hydrogen, in a form soluble in ether. The meteorite contains a considerable quantity of carbon (probably as graphite) which is insoluble in ether. The total percentage of carbon found on igniting the meteorite in oxygen amounted to 3·36 per cent; this closely corresponds with the amount found by Berzelius, viz., 3·05 per cent.

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* Pogg. Ann. xxxiii. p. 113.

† Wien. Acad. Ber. xxxv. 5., ditto xli. 565.

From the above it is evident that the Alais meteorite contains at least a half per cent of a hydrocarbon, which is deposited in acicular crystals when the mass is treated with ether, together with considerable quantities (more than one per cent) of free sulphur, crystallizing from the ethereal solution in rhombic octohedra. To judge by the melting point, the hydrocarbon may be analogous to a mineral wax called Könlite, discovered by Kraus in the lignite of Uznach, which contains an equal number of atoms of carbon and hydrogen, and melts at 114° C.

The Rev. T. P. Kirkman, M.A., F.R.S., read a Paper "On Maximum Groups."

A substitution is positive or negative, as it is made by an even or by an odd number of transpositions of two letters.

To every group G of positive substitutions (a *positive* group G), derangements GP, GQ, .... can be added, so as to complete the entire group of $\frac{1}{2}\Pi N$ substitutions, N being the number of letters, which is always a positive group.

A positive group G is a *maximum positive group,* when no derangements of G or of any factor of G can be added to it so as to complete a positive group of an order inferior to $\frac{1}{2}\Pi N$.

To every *mixed* group J, *i.e.* a group containing positive and negative substitutions, derangements JP, JQ, . . . can be added so as to complete the entire group of the order $\Pi N$.

A mixed group J is a *maximum mixed group* when no derangements of J or of any factor of J can be added to it to complete a group of an order inferior to $\Pi N$.

To every group H, whether mixed or positive, derangements can be added so as to complete the entire group of $\Pi N$, which is always a mixed group if $N > 2$.

The suspicion expressed in the final paragraph of my memoir *On the Theory of Groups* (Memoirs of the Literary and Philosophical Society of Manchester, 1861), that the effect of variation of exponents in the auxiliary group $g$ (Art. 34) on the number of equivalent groups constructible, would have to be reconsidered, turns out to be correct. The groups enumerated in theorem H (Art. 37) always exist, but they are not always the whole of the equivalents. If the partition of N is

$$N = A\cdot a = A\cdot Aa',$$

A being a prime number, my estimate of the effects of variation of exponents is correct for $N = A\cdot A = A^2$, whatever prime A may be, and also for $N = A\cdot A^2 = 2\cdot 2^2 = 8$; but it is defective for higher values of N. I took it for granted that if the auxiliary be

$$\begin{array}{cccccccc}
1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 \\
2 & 1 & 4 & 3 & 6 & 5 & 8 & 7 \\
3 & 4 & 1 & 2 & 7 & 8 & 5 & 6 \\
4 & 3 & 2 & 1 & 8 & 7 & 6 & 5 \\
5 & 6 & 7 & 8 & 1 & 2 & 3 & 4 \\
6 & 5 & 8 & 7 & 2 & 1 & 4 & 3 \\
7 & 8 & 5 & 6 & 3 & 4 & 1 & 2 \\
8 & 7 & 6 & 5 & 4 & 3 & 2 & 1,
\end{array} \qquad (g)$$

no variation of exponents would give different equivalent groups of H, by the method of Art. 39, except by taking for unity in $(g)$ one of the seven forms following,

$$1\ 2^2 3\ 4\ 5\ 6\ 7\ 8,\quad 1\ 2\ 3\ 4^2 5\ 6\ 7\ 8,\quad 1\ 2\ 3\ 4\ 5\ 6^2 7\ 8,$$
$$1\ 2\ 3\ 4\ 5\ 6\ 7\ 8^2,\quad 1\ 2^2 3\ 4^2 5\ 6\ 7\ 8,\quad 1\ 2^2 3\ 4\ 5\ 6^2 7\ 8,$$
$$1\ 2^2 3\ 4\ 5\ 6\ 7\ 8^2,$$

and then affecting every element in $(g)$ with the exponent read over it in unity. The truth is that $(g)$ is an effective auxiliary if we write it thus:

$$\begin{array}{cccccccc}
1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 \\
2 & 1 & 4 & 3 & 6 & 5 & 8 & 7 \\
3 & 4 & 1 & 2 & 7^2 & 8^2 & 5^2 & 6^2 \\
4 & 3 & 2 & 1 & 8^2 & 7^2 & 6^2 & 5^2 \\
5 & 6 & 7^2 & 8^2 & 1 & 2 & 3^2 & 4^2 \\
6 & 5 & 8^2 & 7^2 & 2 & 1 & 4^2 & 3^2 \\
7^2 & 8^2 & 5 & 6 & 3 & 4 & 1^2 & 2^2 \\
8^2 & 7^2 & 6 & 5 & 4 & 3 & 2^2 & 1^2,
\end{array} \qquad (g')$$

which can be proved, as in Art. 39, to be a group equivalent to $(g)$, and different as an auxiliary from the preceding eight. Seven more auxiliaries can be formed as above from $(g')$, and thus we can construct on $(g)$, not eight only, but sixteen equivalent groups K of the 16th order with 16 letters, all having 15 principal substitutions of the second order, and all beginning with

$$\begin{array}{l}
1\ 2\ 3\ 4\ 5\ 6\ 7\ 8\ 9\ 0\ ab\ cd\ e\ f \\
2\ 1\ 4\ 3\ 6\ 5\ 8\ 7\ 0\ 9\ ba\ dc\ f\ e = \theta,
\end{array}$$

by the method of Art. 39. There are in all $15{\cdot}13{\cdot}11{\cdot}9{\cdot}7{\cdot}5{\cdot}3{\cdot}1$ equivalent substitutions $\theta$: if we employ them all, and with them 16 variations of each of the 30 equivalents of $(g)$, we shall construct

$$\frac{1}{15}(16{\cdot}30{\cdot}15{\cdot}13{\cdot}11{\cdot}9{\cdot}7{\cdot}5{\cdot}3) = Q$$

different equivalent groups K of 16. Dividing $\Pi$ (16) by this number of equivalents, we obtain for the order of the maximum modular, made by adding to K all its derived derangements (Theorem A, Cor.),

$$16{\cdot}15{\cdot}14{\cdot}12{\cdot}8,$$

and this maximum modular has Q equivalents.

This corrects the result of my article 96, and it may be of interest as affording when generalised, as it easily can be, a tactical verification of the very important theorem deduced by M. Jordan from a theorem of Galois about a certain determinant, that if $n^r$ be any power of a prime number $n$, there is a group made with $n^r$ letters of

$$n(n-1)n^2(n^2-1)n^3(n^3-1)\ldots n^r(n^r-1)$$

linear substitutions. Vide M. Jordan's *Thèse sur le nombre des valeurs des fonctions* (Journal de l'Ecole Polytechnique, 1861), chap. v. It also shows that M. Jordan's group of linear substitutions is a maximum modular on the model ($j$) of the order $n^r$, having $n^r-1$ principal substitutions, *i.e.* consisting of ($j$) and all its derived derangements.

As this treatise of M. Jordan is the only one, so far as I can learn, that has appeared in a revised form in the French language since the days of Cauchy, on this difficult subject, it may be worth the while to give a little account of it. The memoir, as may be expected from the nature of the question, is neither short nor simple, but the aim is well defined, and the result is easy both to understand and to evaluate. The principal object of the work is to determine the order of a transitive group T made with M letters, which contains a set of similar substitutions $P_1P_2P_3\ldots$, such that the whole of them with their powers and products form an intransitive group $\Sigma$. M. Jordan shows that $\Sigma$ will contain an intransitive group made up of $\frac{M}{n}$ transitive groups of $n$ letters, and, therefore, $n$ being prime, of the $n$th order; and if there be more than one such group $\Sigma$ in T, he selects the *sous-groupe minimum* so constructible, and takes its prime order $n$ as the modulus of his substitutions. His final result is, that $M = k\cdot n^{rp} = k\cdot m^p$, and that the order of T will always of necessity divide

$$1\cdot2\cdot3\ldots k\left\{ n^{\frac{r(r+1)}{2}}\cdot(n-1)(n^2-1)\ldots(n^r-1)\right\}^{kp};$$

and thus, although he has not determined the order of T, nor assigned the form of his substitutions when $k > 1$ (for he has not at all inquired into the degree of his unknown functions of $i$, the $(r+1)^{th}$ variable which has $k$ values), he conceives that he has exactly given the superior multiple of any group T, which has a set of similar substitutions $P_1 P_2 \dots$ the whole of which form, with their derivatives, an intransitive group $\Sigma$.

He has, indeed, assigned the superior limit of many such *woven grouped groups* T; but there is a vast number of them whose order is out of his limit. Take, for example, the woven grouped group of my theorem L (Art. 51), which is made by writing under 12345 the long-known group of 60 made with those elements, parallel to the same group of 60 made with 67890. Weaving the two sub-groups of 60, and then transposing them, or, which is the same thing, multiplying by 6789012345, we have a transitive group T of $60 \cdot 60 \cdot 2$. If we collect the 48 substitutions similar to 1234578906, *i.e.* to 2345167890, of the fifth order, these determine an intransitive $\Sigma$, containing the five powers of 2345178906, and no regular substitutions but of the fifth order. No other intransitive $\Sigma$ is thus constructible. Therefore $n = 5$ is the order of the *sous-groupe minimum*, $M = 2 \cdot 5^{1 \cdot 1} = kn^r$, and the order of T ought, by M. Jordan's result, to divide $1 \cdot 2 \cdot \{5 \cdot 4 \cdot\}^2 = 800$. But $60 \cdot 120$ is no divisor of this number. We can also construct such a group T of the order $120 \cdot 240$, which is no divisor of 800.

For another example, write under 123456 the group of 60 positive substitutions, selected from the group of 120 of my article 65, parallel to the same group made with 7890*ab*. We have by weaving and grouping, as before, a transitive group T of the order $60 \cdot 120$, in which, if we collect all the substitutions similar to 2653417890*ab* of the third order, these determine a derivative intransitive $\Sigma$, the only one constructible, which has regular substitutions of the third order, and of no other. Then $n = 3$ is the order of the *sous-*

*groupe minimum*, $M = 4 \cdot 3^{1 \cdot 1} = k \cdot n^{rp}$, and the order of T should divide

$$1 \cdot 2 \cdot 3 \cdot 4 \{3 \cdot 2\}^4,$$

but 60·120 is no divisor of the number. In like manner we can construct transitive groups T of the orders, 120·240, 360·720, and 720·1440.

We thus easily show that M. Jordan's limit is defective for such transitive groups T made with $2n^r = M$ letters, $(k = 2)$ whatever $n^r$ may be $> 4$; and also for higher values of $k$.

All the groups T considered by M. Jordan are portions of the transitive maximum woven grouped group of my theorem L made with

$$kn^{rp} = n^r l = Aa$$

elements, of which the order is

$$\left(n^r \cdot (n^r - 1) \ldots 3 \cdot 2 \cdot 1\right)^l 1 \cdot 2 \cdot 3 \ldots l.$$

When $M = 2 \cdot 3^{1 \cdot 1} = k \cdot n^{rp}$, or $= 3 \cdot 2^{1 \cdot 1} = k \cdot n^{rp}$, or $= 2 \cdot 2^{2 \cdot 1} = k \cdot n^{rp}$, M. Jordan's limit coincides with mine. The tactical construction of this group is very simple, and requires no aid of analytic formulæ, or numerical computation. It is very desirable that this maximum group should be broken into its factors, but the key to this division is yet to be found. I shall show, that the required division can be in a great measure effected by selecting the positive groups and the mixed groups composing it.

If we treat M. Jordan's group of linear substitutions made with $n^r$ elements, $n$ being prime, by the method of my article (84) (*Theory of Groups*), we easily establish the theorem following, which is probably new:—

Theorem.—If $n^r$ be any power $> 2$ of a prime number $n > 2$, there is a transitive non-modular group formed with $n^r - 1$ elements of $(n^r - 1) n^{r-1} . (n^{r-1} - 1) n^{r-2} . (n^{r-2} - 1) \ldots n^2 \cdot (n^2 - 1) n (n - 1)$ substitutions; also a transitive group made with $n^r - 2$ elements of $n^{r-1} . (n^{r-1} - 1) n^{r-2} . (n^{r-2} . (n^{r-2} - 1) \ldots n^2 (n^2 - 1) n (n - 1)$ substitutions, which is generally non-modular.

This theorem might be extended. When $n^r = 2^4 = 16$, there is a non-modular group made with 15 of the order 15·14·12·8, a non-modular made with 14 of 14·12·8, and a non-modular made with 12 of 12·8, which has a derived derangement, making it a modular group of 12·8·2. These are all transitive groups and easily constructed, either with or without the aid of the notation of linear substitutions made with 16 letters. We have here the true generalisation of the group of 7·4·3·2·1 made with 7 letters, about which, with the 30-valued functions given by it, so much labour, far more learned than lucrative, has been spent by MM. Betti, Kroneker, Hermite, and myself.

It is curious, that the true analytical expression of these transitive groups of $n^r - 1$ and $n^r - 2$ letters is obtained by considering them as intransitive groups of $n^r$ letters.

## MATHEMATICAL AND PHYSICAL SECTION.

### February 12th, 1863.

**Robert Worthington**, F.R.A.S., President of the Section, in the Chair.

Mr. W. L. **Dickinson** read a Paper "On the Eclipse of the Sun, May 17, 1863."

This eclipse, which is a partial one, will not be of great magnitude to any part of the United Kingdom. In this neighbourhood about three-tenths of the sun's diameter will be obscured by the moon. The following particulars, which have been computed from the numerical equations in the *Nautical Almanac*, are offered to the members, to enable them to compare the calculated results with their own observations.

*Eclipse of the Sun, on Sunday afternoon, May* 17, 1863.

Computation for Manchester (Royal Infirmary), lat. N. 53° 29′, long. W. 2° 14′: —

| | h | m | s |
|---|---|---|---|
| Begins.............. | 5 | 37 | 12 |
| Greatest Phase ....... | 6 | 25 | 32 |
| Ends ............... | 7 | 11 | 4 |

Greenwich mean time.

Magnitude of the Eclipse (Sun's diameter = 1) 0·307.

Angle, from North Pole, of
first contact, 49° towards the West,
last contact, 45° towards the East,

Angle, from Vertex, of
first contact, 88° towards the West,
last contact, 9° towards the East,
for *direct* image.

The position of the points of contact may be familiarly illustrated in the following manner. If we suppose a Victoria shilling to represent the sun, the moon will appear first to touch it on the right side, at the last *r* in the word *Britanniar:* and to leave it on the left side near the vertex, at the letter *i* in the word *Gratia*. A line drawn between these letters will indicate the direction of the moon's path.

There will not be another Eclipse of the Sun visible in England until October 19, 1865.

The following returns of the rainfall for 1862 were communicated to the Section:—

## THE FLOSH, CLEATOR, near WHITEHAVEN.

BY THOS. AINSWORTH, ESQ.

| 1862. | Amount in Inches. | No. of Days on which Rain fell. |
|---|---|---|
| January | 5·075 | 22 |
| February | 1·122 | 12 |
| March | 3·345 | 18 |
| April | 5·410 | 22 |
| May | 5·230 | 17 |
| June | 4·197 | 23 |
| July | 3·850 | 19 |
| August | 5·150 | 18 |
| September | 2·417 | 17 |
| October | 8·780 | 27 |
| November | 1·762 | 19 |
| December | 6·965 | 26 |
| | 53·303 | 240 |

## OLD TRAFFORD, MANCHESTER.

BY G. V. VERNON, F.R.A.S.

Gauge 3 feet above the ground, and 106 feet above the sea level.

| 1862. | Fall in Inches. | Days. | Mean of 5 years, 1858—1862. | Mean of 69 years, 1794—1862. |
|---|---|---|---|---|
| January | 1·896 | 17 | 1·826 | 2·443 |
| February | 0·958 | 10 | 1·427 | 2·347 |
| March | 3·669 | 18 | 3·335 | 2·336 |
| April | 2·717 | 19 | 2·461 | 2·054 |
| May | 4·470 | 20 | 2·091 | 2·354 |
| June | 3·072 | 22 | 3·399 | 2·888 |
| July | 4·527 | 21 | 2·693 | 3·648 |
| August | 2·350 | 12 | 3·815 | 3·589 |
| September | 4·908 | 18 | 3·916 | 3·191 |
| October | 5·035 | 23 | 3·422 | 3·810 |
| November | 1·685 | 14 | 2·423 | 3·485 |
| December | 3·221 | 24 | 2·938 | 3·282 |
| | 38·598 | 218 | 33·746 | 35·427 |

The fall in 1862 was 4·852 inches above the average for the last five years, and 3·171 inches above the average for sixty-nine years.

The means for 69 years have been found by including the values for the years 1858—1862 in addition to the values determined by Mr. John Curtis for 64 years, in Vol. XV. of the Memoirs of the Society.

## THELWALL, near WARRINGTON.

### BY JOHN ATKINSON, F.G.S.

The height of the station above the mean sea-level is 96 feet. The gauge is about 12 or 14 inches above the ground.

| 1862. | | Inches. | Days on which it fell. |
|---|---|---|---|
| 6·821 | January | 2·142 | 18 |
| | February | 0·744 | 10 |
| | March | 3·935 | 18 |
| 9·139 | April | 2·335 | 18 |
| | May | 4·141 | 20 |
| | June | 2·663 | 22 |
| 10·328 | July | 3·970 | 19 |
| | August | 2·958 | 19 |
| | September | 3·400 | 16 |
| 9·552 | October | 4·915 | 24 |
| | November | 1·673 | 13 |
| | December | 2·964 | 22 |
| | | 35·840 | 219 |

## Ordinary Meeting, March 11th, 1863.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

The Rev. ROBERT HARLEY, F.R.A.S., read the following communication "On Bring's Reduction of the Equation of the Fifth Degree to a Trinomial Form."

Mr. Ekman, the Honorary Librarian of this Society, has called my attention to a remarkable investigation on the theory of algebraic equations, bearing date 14th December, 1786, by Erland Sam. Bring, who for some time was Professor of History in the University of Lund. That portion of Bring's investigation which relates to equations of the fourth and fifth degrees has recently been republished by the Royal Swedish Academy of Sciences, in their *Proceedings* for 1861, along with a Paper on the subject by Professor C. Hill. (See "Ofversigt af Kongl. Vetenskaps-Akademiens Förhandlingar. Adertonde Årgången." 1861, pp. 317—355.)

Bring's investigation was originally published as a "Disputation," but, sharing the fate of many other University disputations, it was suffered to drop into oblivion, and it remained there until about a year and a half ago, when Professor Hill discovered it and dragged it to light. The only complete copy of Bring's Paper out of Sweden which my friend, Mr. Ekman, has been able to trace, exists, he informs me, in the library of the Observatory of Pulkova, in Russia.

Bring succeeds in extending Tschirnhausen's method of transformation so as to reduce the general equation of the fifth degree to a trinomial form by means of equations of inferior degrees. It is commonly supposed, I believe, that this reduction was first effected by our own countryman, Mr. Jerrard, whose "Mathematical Researches," published about thirty years ago, gave a new impulse and direction to the efforts of those analysts who were labouring to conquer the difficulties connected with the higher equations. Mr. Jerrard has certainly taken a large and comprehensive view of the subject, and by his original researches has laid all cultivators of algebra under great obligations to him. But in his reduction of the general quintic equation to the trinomial form, by taking away its second, third, and fourth terms at once, he was anticipated, it seems, by Bring.

In a Paper entitled "A Contribution to the History of the Problem of the Reduction of the General Equation of the Fifth Degree to a Trinomial Form," about to appear in No. 21 of the *Quarterly Journal of Pure and Applied Mathematics*, I have briefly indicated what has been done in this problem by different investigators, and I have particularly noticed the labours of Mr. Jerrard, Sir W. R. Hamilton, Mr. Cockle, Professor Sylvester, M. Serret, and Mr. Cayley, as well as of E. S. Bring, whose reduction, written in Latin, I have given *in extenso*.

My object in this communication is simply to draw attention to Bring's investigation; and I shall now content myself with giving a brief sketch of the process which he employs.

Starting with the quadrinomial form,

$$z^5+pz^2+qz+r=0\ldots(\mathrm{A})$$

to which, as is well known, Tschirnhausen's quadratic transformation enables us to reduce the general equation of the fifth degree, and assuming

$$z^4+dz^3+cz+bz+a+y=0\ldots(\mathrm{B}),$$

Bring, by the elimination of $z$ between (A) and (B), arrives at an equation in $y$ of the form

$$y^5+\mathrm{a}y^4+\mathrm{b}y^3+\mathrm{c}y^2+\&\mathrm{c}.=0\ldots(\mathrm{C})$$

where a, b, c, &c. are rational functions of the given coefficients $p, q, r$, and of the disposable coefficients $a, b, c, d$. He next proceeds to inquire how the three conditions

$$\begin{aligned} \mathrm{a}&=0\ldots(\mathrm{D})\\ \mathrm{b}&=0\ldots(\mathrm{E})\\ \mathrm{c}&=0\ldots(\mathrm{F}) \end{aligned}$$

can be satisfied simultaneously; and he finds that the first (D) is satisfied by making

$$a=\frac{3pd+4q}{5}$$

This value being substituted in the second and third equations, viz. (E) and (F), and $b$, $c$, or $d$ being eliminated, there results an equation of the sixth degree. But this elevation of degree is avoided by a very simple artifice. Writing

$$b=ad+\zeta, \text{ and } c=d+\gamma,$$

the equation (E) takes the form of a quadratic in $d$, which quadratic is made to vanish identically. The evanescence of the first coefficient determines $a$, by the solution of a linear equation, as a rational function of the known quantities $p, q, r$. The evanescence of the second coefficient, $a$ being now treated as known, determines $\zeta$ as a linear function of $\gamma$. And the evanescence of the third coefficient, when the value of $\zeta$ in terms of $\gamma$ is introduced, determines $\gamma$, by the solution of a quadratic equation, as a function of $p, q, r$. Hence $a, \zeta, \gamma$ are now severally known, and are so determined as to satisfy the second equation of condition (E). But it still remains to satisfy the third condition (F). Substituting for $a, b, c$ their respective values

$$\frac{3pd+4q}{5},\ ad+\zeta, d+\gamma,$$

the resulting equation will contain only one unknown, viz. $d$, and a glance at the explicit form of c, given by Bring, shows that $d$ cannot rise above the third degree; so that, by the resolution of a cubic equation, $d$ may be determined and the last condition (F) satisfied.

A Paper was read, "On the Solution of the Differential Resolvent," by W. H. L. Russell, A.B., communicated by the Rev. ROBERT HARLEY, F.R.A.S.

"In the course of last year I was induced, at the request of Mr. Harley, to consider the very interesting differential equation which he has denominated the 'Differential Resolvent.' I obtained the solution of the quartic resolvent by series, which I summed by means of a triple integral. But Professor Boole intimated that he had discovered a process of transformation by which the quartic could be resolved by a single integral. This led me to examine my own series, and I find that the series representing the solution of the quartic could be summed by means of a single integral. I have since discovered that the general resolvent can be solved by means of a single integral. To effect this is the object of the present Paper."

The Author first deals with the resolvent of the quartic equation, viz.:—

$$y^4-4y+3x,$$

$$y-\frac{\left(D-\frac{7}{4}\right)\left(D-\frac{10}{4}\right)\left(D-\frac{13}{4}\right)}{D(D-1)(D-2)}\,\varepsilon^{3\theta}\,y=0$$

where $\varepsilon^{\theta}=x$; and he obtains the solution

$$\frac{C}{x}\int dx\,x\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\theta\,\frac{\cos^{\frac{1}{2}}\theta\left\{\cos\frac{5\theta}{3}-\cos^4\theta\cos\frac{\theta}{3}\cdot\frac{(3x)^3}{2^4}\right\}}{1-\cos^4\theta\cos 2\theta\left(\frac{3x}{2}\right)^3+\cos^8\theta\frac{(3x)^6}{2^8}},$$

C being a certain irrelevant function.

He next discusses the resolvent of the equation

$$y^n-ny+(n-1)x=0,$$

viz.:—

$$y-\phi(D)\,\varepsilon^{(n-1)\theta}\,y=0,$$

where

$$\phi(D)=\frac{\left(D-\frac{2n-1}{n}\right)\left(D-\frac{3n-2}{n}\right)\cdots\left(D-\frac{n^2-n+1}{n}\right)}{D(D-1)(D-2)\dots(D-n+2).}$$

The solution is expressed in a series, the sum of which is found to be as below:—

$$\frac{Cr}{x^r}\int dx\, x^{r-1}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\theta \cos\frac{nr-3n+1}{n-1}\theta$$

$$\frac{\cos\frac{nr-2r+1}{n-1}\theta-\frac{2^n(n-1)^{n-1}}{n^n}\cos^n\theta\cos\left\{n-2-\frac{nr-2r+1}{n-1}\right\}\theta x^{n-1}}{1-\frac{2^{n+1}(n-1)^{n-1}}{n^n}\cos^n\theta\cos(n-2)\theta x^{n-1}+\frac{2^{2n}(n-1)^{2(n-1)}}{n^{2n}}\cos^{2n}\theta x^{2n-2}}.$$

In a brief note the Author indicates the method by which the series employed were derived from the differential equations.

Mr. Crookes F.C.S., exhibited a specimen, weighing 450 grains, of the new metal, thallium, which he discovered by spectrum analysis. He stated that he had been fortunate enough to find this element in comparatively large quantities in the deposit from the flues of Mr. Spence's pyrites burners.

Dr. Joule made the following communication respecting a new and extremely sensitive thermometer:—"Some years ago I remarked the disturbing influence of currents of air on finely suspended magnetic needles, and suggested that it might be made use of as a delicate test of temperature. I have lately carried out the idea into practice, and have obtained results beyond my expectation. A glass vessel in the shape of a tube, two feet long and four inches in diameter, was divided longitudinally by a blackened pasteboard diaphragm, leaving spaces at the top and bottom, each a little over one inch. In the top space a bit of magnetised sewing needle, furnished with a glass index, is suspended by a single filament of silk. It is evident that the arrangement is similar to that of a 'bratticed' coal pit shaft, and that the slightest excess of temperature on one side over that on the other must occasion a circulation of air, which will ascend on the heated side, and, after passing across the fine glass index, descend on the other side. It is also evident that the sensibility of the instrument may be increased to any required extent, by diminishing the directive force of the magnetic needle. I purpose to make several improvements in my present instrument, but in its present condition the heat radiated by a small pan, containing a pint of water heated 30°, is quite perceptible at a distance of three yards. A further proof of the extreme sensibility of the instrument is obtained from the fact that it is able to detect the heat radiated by the moon. A beam of moonlight was admitted through a slit in a shutter. As the moon (nearly full) travelled from left to right the beam passed gradually across the instrument, causing the index to be deflected several degrees, first to the left and then to the right. The effect showed, according to a very rough estimate, that the air in the instrument must have been heated by the moon's rays a few ten-thousandths of a degree, or by a quantity no doubt the equivalent of the light absorbed by the blackened surface on which the rays fell."

E. W. **Binney**, F.R.S., the President, said that of late years considerable attention had been devoted to the examination of the beds of sand and gravel found in the valleys formed since the deposition of the till or boulder clay. Sir Charles Lyell, in his valuable work on the Geological Evidences of the Antiquity of Man, has given us many facts connected with these valley gravels, especially relating to the terraces of the higher and lower level gravels found in the valley of the Somme, in which the flint instruments have been met with. As these two deposits are seen in the neighbourhood of Manchester, he wished to direct attention to all excavations that were being made in them, in order that any remains or implements which might be met with should be preserved. Doubtless many interesting specimens have perished, owing to the parties finding them being ignorant of their value. Many years since a former member of this Society, the late Mr. F. Looney, F.G.S., in speaking of the superficial gravel found in this neighbourhood, at page 23*, says:—"Imbedded in the gravel near the river courses are occasionally found the stone celts of the ancients, from which it is presumed that the rivers, since the country was inhabited, have either sawn their beds deeper or much exceeded their present volume of water; several large trees have been dug up from the sand and gravel; part of one is now lying near the residence of the Rev. J. Clowes, at Kersal Moor, which was dug from the Show Field on his estate, at upwards of 20 feet elevation above the present level of the river. A case more illustrative of this was beautifully shown in the winter of 1820, during the cutting away part of the high ground at Castlefield, near the tunnel mouth; for sixteen feet below the level of the grass a wooden box was found. It was square and formed of four upright posts, driven into a bed of clay; the sides and bottom were closed in with logs of wood; the logs were rudely hewn, had been riven, not sawn, from five to six inches square; some greenstone boulders lay at the bottom, and the whole was covered with sixteen feet of sand and gravel; twelve feet of the lower part had never been disturbed, the continuity of the layers being unbroken.

SECTION.

Alluvium, four feet.

Layers of gravel, twelve
feet, unbroken.

Box.

Layer of clay.

Gravel."

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* List of Organic Remains, &c., and where found, to accompany Mr. Elias Hall's Introduction and Map, by Mr. Francis Looney, member of the Literary and Philosophical Society of Manchester, published in 1836.

Mr. Looney was well known to be a most accurate and intelligent observer, and his mention of stone celts having been found at levels above the present river courses, appears to afford us fair hopes of their being found in the lower level gravel if carefully looked for. But the most interesting fact is the finding of the wooden fabric, by the author termed a box, which, although smaller in size, bears some resemblance to the *crannoges* found sometimes in Ireland under the peat bogs. He (the President) had lately been to examine the place where it was found, which is to the south of and just outside of the old Roman Station of Mancunium, in company with a party who saw the fabric when it was first exposed, and stood in it afterwards. The locality of course is now much changed, but the person was able to point out to him the position where it was found, which is now covered by No. 6 arch of the Altrincham Railway, and said that it was about six feet square and four feet high. The four upright posts driven into the clay were about five inches thick, and roughly riven, as were also the slabs forming the sides and bottom. His informant did not notice any top to the fabric, or how the slabs had been fastened to the posts, but he remembers a layer of cobble stones, each about six inches in diameter, forming a kind of pavement, being in the bottom of it. Upon these stones were some bones, which he examined, but did not recognise any of them as human. All the wood appeared to be oak, but it was very rotten, and fell to pieces soon after it was exposed to the air. None of it was preserved. Under the clay the red rock (Trias) was found at a short distance. The geological position where the fabric was found is in the lower level gravel, about 29 feet above the water of the present river Irwell. For whatever purpose it was made, there can be little doubt as to its having been the work of man, and as we are assured that the overlying sand and gravel was quite undisturbed, there can be as little question of its great antiquity, reaching as far back as the age of the lower level gravels of the valley of the Somme in France, where the flint implements have been found. Doubts have been raised as to these flints having been fashioned by human hands, but as to the origin of the wooden fabric, if, as stated, it was covered by 12 feet of undisturbed sand and gravel,* no such doubts can rationally be raised.

A Paper was read, entitled, "Notes on the Action of Heat and Force upon Matter," by J. C. DYER, V.P.

Confining his observations to the "action of mechanical forces,"—apart from those depending on the electrical, magnetical, and vital forces—he held the former to depend on the inherent properties of matter, as gravity, inertia, and elasticity, each coextensive with the material universe.

That force cannot in any case be treated as an abstraction or as apart from the matter in which it resides, though several authors have had recourse to such an abstraction, to account for phenomena attending the operation of force. Mechanical action, depending on the *elastic force* generated by the agency of *carbon* and *water*, being of so great importance, it is desirable to obtain some settled knowledge as to how this arises. We all know that by combustion of the former the latter is converted into *steam*. That which passes from the burning coals through the boiler plates and into the water to form the elastic vapour, is called *heat*; but whether it be a union of heat with the water, or only a transmitted mechanical action on the water, causing its atoms to separate and assume the elastic state, remains a question for solution; and of late several able writers have offered explanations of this phenomenon founded on the supposed nonentity of heat and the agency of mechanical force to form steam, and thus rejecting the latent heat theory which before seemed to account for the heat absorbed to form steam. So far as the latent heat theory serves to explain phenomena, the mechanical creation of heat is uncalled for, since it is unphilosophical to assign new causes when those before accepted clearly meet the case. Yet we have seen two elaborate essays, by Dr. W. J. M. Rankine and Professor Tyndall, each based on the *force heat* assumption, but without affording any solid proofs of its application to the phenomena adduced by them.

Mr. Rankine states that "Heat produces an increase of temperature and of expansive force in bodies; that the changes from the solid and liquid states to the gaseous state is accompanied by an increase in volume; that heat which produces those effects is known by the name of sensible heat, as attaining the form of heat, and in short making the body hotter." Again, that "changes of volume are attended with changes of molecular arrangement, perceptible or imperceptible, and that the latent heat of expansion or evaporation consists of heat that disappears in overcoming pressure and the attraction of the particles of the body." In reference to the non-production of heat by the moving force exerted in waterfalls, the following solution is offered, viz.: "It is natural to suppose that the *motion* during this phenomenon (the falling of a mass of water) has not been destroyed, but has been converted into revolutions of the particles in vortices or eddies too small to be perceptible by any of our means of observation, and that the centrifugal force of such eddies is the *cause* of the tendency of hot bodies to expand, melt, and evaporate."

The first class of phenomena above cited are plainly accounted for by the doctrine of latent heat, but the latter explanation about waterfalls passes Mr. Dyer's comprehension. He submits that what is said to be *natural to suppose* concerning the revolutions of particles in vortices and eddies too small to be perceived is too obscure and questionable to be accepted in explanation of anything. If Mr. Rankine has no better mode of accounting for the nonappearance of heat from the force expended in waterfalls than the above, his defence of the generation of heat by force fails, and the balancing the force by the resistance at the foot of the falls is just as open to view as the falling force itself; so that the motions, too small to be perceived, are supererogatory inventions. The dynamical origin of heat is also set forth by Professor Tyndall, in a paper "On the Forms and Action of Water." Examples are therein adduced of vast *mechanical forces* exerted by the formation of water, and by its changes from the states of a solid, liquid, and vapour. He says that "on the combination of oxygen and hydrogen to form water weighing 10lb. an energy was expended, the atoms clashing together with a force equal to that of a ton weight let fall from a height of 23,757 feet. In falling from the state of vapour to that of water an energy was expended equal to that of a ton falling from a height of 3,700 feet, the moving force of the stone avalanches of the Alps was but as that of snowflakes, compared with the energy involved in the formation of a cloud. In passing finally from the liquid to the solid state, the atoms of 10lb. of water exercised an energy equal to that of a ton weight falling down a precipice 550 feet high."

Now, the union of the gases to form water, the evaporation of it to form steam, and freezing it into ice, are chemical changes, and the forces called into action by them are chemical forces, which cannot be measured by that of gravity in falling tons or even ounces; but the mutations of heat attending those changes are measured by degrees, when passing from the latent to the sensible state, and *vice versa.* Of vapour in the air, the Professor says, that "though forming only about 0·5 per cent of the atmosphere, for every ray of terrestrial heat struck down by the air, fifty, sixty, or seventy rays were destroyed by the aqueous vapour. This vapour permitted the solar rays to reach the earth, but intercepted the terrestrial rays in their escape towards space," and he adds that "in the presence of such experiments, it was easy to see how the snow of the Alpine mountains, and how the ice, should be squeezed through the moulds formed by the valleys, and then that these mountain glaciers were as much a proof of heat as of cold, and to produce the vapour to condense and form glaciers required as much heat as would raise five times the weight of the glaciers of cast iron to its melting point." The heat required to convert water into steam as it passes from the sensible into the latent state is a known quantity, so also is the amount required to convert ice into water; but that required to melt a pound of ice and evaporate a pound of water would fall far short of melting five pounds of cast iron.

That no mechanical energy is exerted in the conversion of water into steam is obvious to our senses, since evaporation is the most gentle, imperceptible, as well as the most universal action in Nature's laboratory; and although the elasticity of the steam itself exerts mechanical force, equal to the atmospheric pressure, chemical force alone is concerned in evaporation, and therefore cannot be compared with falling bodies, so that the alleged energy and *clashing force of atoms*, in the actions of water cited, are wholly imaginary; nor can philosophy assert any fact relating to mechanical action, which cannot be proved by experiment or established by plain induction; and in the cases cited, no one can know anything about the occult forces and energies of the motions of atoms attending the mutations of water. Great respect is due to both of the gentlemen whose essays have invited the above strictures; but the search after the truths of science "should not halt before high names."

Mr. Rankine, in offering demonstrations of the dynamical origin of heat, allows that heat disappears and becomes *latent in water*, in passing from the solid to the liquid state, and from this into vapour, as also that the same amount of heat is given out and reappears as sensible heat, upon the reversal of those changes; thus in substance admitting the latent heat doctrine, whilst denying the materiality of that which does so enter into a latent and reappear in a sensible state, and calling this heat the production of force, when no mechanical force whatever is called into action by those mutations of water. They are, however, plainly explained by the agency of heat, in its chemical union with water and its evolution from water. Although the evolution and absorption of heat from the compression and expansion of elastic fluids bears a constant relation to the amount of compression and to the compressing force, yet this is not the case, as Mr. Rankine supposes, in the resistance to compressing forces by liquids and solids; the amount of compression in these latter cases depends upon the relative mobility of the particles of the bodies, and we find forces of great energy, exerted against yielding solids, as in carriage and other springs and elastic bodies, are in continuous action without producing any sensible heat when no compression takes place. If it were true that acting forces always produced their equivalent in heat, then the powerful movements of railway springs, hydraulic presses, and the like, should raise intense heat in the steel springs and water exerting such force, and the absence of any heat in such cases, must be fatal to the dynamic creation of heat.*

The following Paper, "On the Chemical Constitution of American Rock Oil," by Mr. Schorlemmer, Assistant in the Laboratory of Owens College, was communicated by Professor Roscoe:—

In a Paper published in the October number of the Chemical Society's Journal, I showed that the products of the distillation of cannel coal at a low temperature contain a series of homologous hydrocarbons of the formula $C_n H_{n+2}$. I further showed that these are the hydrides of the alkohol radicles, as, upon treatment with chlorine, they yielded by substitution of one atom of hydrogen by one of chlorine the corresponding chlorides, from which other derivatives may be obtained.

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* I have experimentally determined the thermal effects of compressing or dilating metals and fluids, and have found these effects to be strictly in accordance with the dynamical theory of heat. See *Phil. Trans.*, 1859.—Ed.

In the portion of the oil boiling below 120° C, I found the following four hydrides:—

$C_{10} H_{12}$ hydride of amyl, boiling point 39° C.
$C_{12} H_{14}$ hydride of hexyl, ——— 68° C.
$C_{14} H_{16}$ hydride of heptyl, ——— 98° C.
$C_{16} H_{18}$ hydride of octyl, ——— 119° C.

Of these the hydride of heptyl or oenanthyl is the most interesting, as it was previously unknown, and I therefore undertook the investigation of its derivatives, concerning which likewise our knowledge is very limited and contradictory. Thus, for instance, many chemists state that the alkohol obtained by the distillation of castor oil with potash is heptylic alkohol, whilst others regard it as octylic alkohol, and it is only by the most recent experiments of Bouis[*] that we learn with certainty that this substance is octylic alkohol, inasmuch as he obtained the true heptylic alkohol by the action of nascent hydrogen on oenanthol. For the purpose of this investigation I endeavoured to obtain the hydride of heptyl from the American petroleum, as the yield of this substance from the cannel oils is but small, and the labour of purification tedious and disagreeable.

The existence of this hydride in the petroleum was rendered probable by the fact of the discovery of hydride of hexyl by Pelouze and Cahours.[†]

The oils which I examined are those known by the name of turpentine substitute, and obtained as the first products in the rectification of the crude oil. Different samples of the commercial articles possess very different properties, the spec. gr. lies between 0·70—0·75. One sample began to boil at 30° C, and the greatest portion distilled over below 100° C, whilst others between 80°—150° C, and others between 100°—200° C.

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[*] Compt rend. 55. 140.

[†] Compt. rend. 54, 1241.

When these oils are subjected to fractional distillation no product of constant boiling point is obtained, the oils requiring for this purpose a preliminary purification with concentrated nitric acid.

The greatest portion of the oils remains unattacked, and the acid solution contains nitrobenzol (from which aniline was prepared), nitrotoluol and binitrotoluol, and small quantities of fatty acids produced from traces of olefines which are probably contained in the crude oils.

I tried to separate these olefines by adding bromine to the crude oil until the colour of the latter no longer disappeared; a few drops, however, are sufficient for a large quantity of the oil, and when the whole is subjected to distillation, a very few drops of bromine compounds of a high boiling point remained behind, the quantity of which was too small for further examination.

The oil after this treatment was well washed, dried over potash, and rectified repeatedly over sodium. By fractional distillation, the following four hydrides were obtained, and found to be identical with the hydrides from the cannel tar:—

$C_{10}\,H_{12}$ hydride of amyl, boiling point 34° C.
$C_{12}\,H_{14}$ hydride of hexyl, ——— 68° C.
$C_{14}\,H_{16}$ hydride of heptyl, ——— 98° C.
$C_{16}\,H_{18}$ hydride of octyl, ——— 119° C.

In addition to these I obtained a small quantity (about one gramme) of a liquid boiling between 20°—30° C, and hence we may infer that also hydride of butyl is present in small quantities.

I stated in the paper above alluded to that hydride of amyl boils at 39° C. The same compound from petroleum boils at 34° C. I find, however, that the presence of traces of foreign substances alter the boiling point of this body very considerably. Thus, for instance, I obtained from the crude oil about one ounce of a liquid which boiled from 15°—20° C;

after treatment with nitric acid, the volume of the liquid remained almost unchanged, showing that only a very small quantity of substance had been removed; but, after drying with potash and rectifying over sodium, it was found that a mere trace of the liquid boiled below 30° C; nearly the whole distilled at 34° C, and consisted of hydride of amyl. Of the four hydrides which I isolated, I have only prepared the hydride of heptyl in quantity; four gallons of turpentine substitute boiling between 80°—150° C, yielded three pounds of the pure compound.

In order to obtain from this other heptyl compounds, it was transformed into the chloride according to the excellent method described by Hugo Müller,* which consists in the addition of a small quantity of iodine to the substance which is to be treated with chlorine. The substitution occurs much more rapidly in this case than when chlorine alone is employed, and goes on in absence of the daylight, so that a rapid current of chlorine gas can be led into the liquid without any chlorine escaping with the hydrochloric acid vapours.

Hence it is seen that the constitution of American petroleum, at least that portion boiling below 120° C, is quite analogous to that of the oil from cannel tar.

Petroleum consists mainly of the hydrides of the alkohol radicles, it contains very small quantities of benzol and toluol, and probably traces of olefines, whilst in the cannel coal oil the hydrides are found in smaller quantities, and benzol and toluol in proportionally larger amounts. In the oil obtained by distillation of boghead coal, Greville Williams has discovered a series of hydrocarbons possessing the composition and physical characters of the hydrides $C_n H_{n+2}$, also benzol and its homologues and olefines.†

The rock oils obtained in other countries appear to possess a somewhat similar constitution.

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* Journ. Chem. Soc., 15, 41.

† Phil. Trans., 1857.

Thus, for instance, Warren de la Rue and Hugo Müller[*] found in the Rangoon tar, benzol, toluol, xylol, and cumol and hydrocarbons of the formula $C_n\ H_{n+2}$. They were, however, unable to isolate from these a compound of definite composition and boiling point. The rock oil from Sehnde, in Hanover, consists, according to the investigation of Busenius, Eisenstuck,[†] and Uellsmann,[‡] of hydrocarbons of the same general formula, but they likewise failed to obtain definite products.

Pebal and Freund[§] found in the rock oil from Gallicia benzol and homologues, carbolic acid and homologues, and hydrocarbons which are not attacked by the strongest acids, and probably identical with those previously mentioned.

## MICROSCOPICAL SECTION.

16th February, 1863.

Mr. Joseph Sidebotham, Vice-President of the Section, in the Chair.

Captain **Fletcher**, of the ship "Tigris," presented a portion of harbour mud from Singapore, and five soundings from the coasts of Java and Sumatra.

Mr. R. D. **Darbishire** presented specimens of mud and fossil shells (received through Dr. P. P. Carpenter) from the post-pliocene or latest tertiary deposits at Logan's Farm, Mile-end Quarries, near Montreal, Canada, described by Sir C. Lyell ("First Travels in North America," Vol. II., p. 135), and in Papers by Dr. J. W. Dawson in the "Canadian Naturalist," 1858 and in 1859. Mr. Darbishire, in a note to the Secretary, stated that one of the peculiarities of the deposit is that it seems to have been formed in a quiet hollow.

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[*] Jahresbericht, 9, 606.

[†] Liebig's Annalen, 113—115.

[‡] *Ibid*, 114—279.

[§] Liebig's Annalen, 115, 19.

Spiculæ of sponges are found in position as if the sponge had grown and been quietly buried on the spot. Amongst other characteristic fossils are numerous Foraminiferæ, and a silicious and close textured sponge referred to Tethea, of the species Logani, which is now found in water from the tide line to 200 fathoms deep. Mr. Nevill undertook to examine and report upon the specimens.

Mr. H. A. HURST presented a copy of Part IV., Vol. XII, of the "Journal of the Agricultural and Horticultural Society of India," published at Calcutta, containing the Prize Essay on Cotton Cultivation in India from foreign seed, by Dr. J. Shortt, F.L.S., Zillah Surgeon, Chingleputt, for which the prize of 1,000 rupees and the gold medal of the Manchester Cotton Supply Association were awarded. Mr. Hurst read a paragraph from page 499 relating to the early stage of the cotton pod, which, bearing upon points lately in dispute and not before published in this country, is given entire. "On examining a cotton pod soon after the ovary has been impregnated (which is known by the change in colour and the fading of the petals, or flower leaves, or corolla), it is found to contain a number of seeds according to its particular variety. If a single seed be separated and examined by the naked eye nothing is visible; but when seen through the microscope, it is found covered with a villous coat, formed apparently of elongated cells joined end to end. These are filled with sap. The young seed itself is somewhat pear-shaped, and resembles in miniature, some of the China candied fruits with the frosted crystals of sugar covering it. On letting out the contents of a single cell, it is found to consist of granular cells containing a centro-lateral nucleus. On examining a pod between three and four weeks old, the seed still retains somewhat of its pyriform shape, and appears quite shaggy. The fibres, tapering to a point at their free end, resemble hollow cylindrical tubes filled with fluid, and vary in length; and on submitting a single fibre, compressed between pieces of glass, to the microscope, the flattened surfaces become distinctly visible. Again, on substituting a mature fibre before it gets dried, the filament is found to consist of tubular hairs, which are now quite cylindrical. After the dehiscence of the mature capsule by the contraction and separation of its valves, the wool becomes dry from exposure. A filament now placed under the microscope is found to resemble a flattened piece of tape, twisted upon itself, and apparently formed of an extremely thin and transparent membrane, interspersed with dark granular matter, which, after a certain time, disappears in some of the varieties."

Mr. J. G. LYNDE, F.G.S., M. Inst. C.E., read a Paper "On the Action of Magenta upon Vegetable Tissue," in which he described a series of experiments upon cuttings of Vallisneria immersed in a solution of that dye in cells under the microscope, and its effect upon the circulation in that plant. He found that so long as the vital action continued, the cell walls and the moving chlorophyll retained their green colour, but the injured cells were immediately deeply reddened, and their contents gradually acquired the same colour, the intensity of which was in proportion to the thickness or density of the tissue. Between the cell walls it would appear that there exists an intercellular membrane, devoid of vital action, which becomes rapidly coloured whilst the circulation continues active. On the inner surface of the cell wall, whilst rotation is going on, the author observed a luminous stratum suggesting the action of ciliæ, but in every observation as the dye permeated the tissue and the circulation ceased, the true cell wall became covered with irregular markings, either corrugated or having raised excrescences, scarcely alike in any two cells; in no case were the markings visible until the rotation had ceased, and they had the appearance which would be produced by ciliæ falling against the cell wall in all positions upon the suspension of vital action.*

The chlorophyll vesicles appear in three forms: in a gelatinous sac or mass rotating altogether in the cells; as independent vesicles apparently homogenous in their structure, rendered opaque by colouring matter; and lastly, as independent vesicles somewhat increased in size, of a pale green colour, almost transparent, containing nuclei, one, two, or three in number, which in reality appear to be immature vesicles within the parent, similar to Volvox globator, without rotatory motion. The chlorophyll vesicles appear to resist the action of the magenta for some time after their rotation has ceased, indicating a vitality, at least to a certain extent, independent of that of the cell. In some of the experiments a few of the cells assumed a purplish colour, whilst in the adjoining cells the circulation was active and the chlorophyll green; in those the chlorophyll appeared to be decomposed, and the cell to be nearly full of very minute dots, swarming like the granules in Closterium lunula. Upon this subject the author offered no opinion. The observations were made with $\frac{1}{6}$th and $\frac{1}{8}$th objectives, and the paper contained minutia of several experiments, with the hours of observation, temperature of the room, and other particulars.

Dr. **Roberts** observed that Mr. Lynde's remarks upon the separate vitality of the cell and cell contents were very suggestive; he had noticed that fresh blood discs (for instance from a pricked finger) were not immediately affected by Magenta, but that some little time was required for the dye to permeate the envelopes.

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* Eminent microscopists do not entertain the idea of the circulation in Vallisneria being due to ciliary action; "this appearance is decidedly affirmed by Mr. Wenham to be an optical illusion." (See Dr. Branson, in "Quarterly Journal of Microscopical Science" for 1855, Vol. III., p. 274, and Mr. Wenham, p. 277, quoted by Dr. Carpenter, "Microscope and its Revelations," 3rd ed., p. 408.) This opinion was no doubt formed solely upon observations made during vital action, and may be modified upon examination of the supposed dead and dying ciliæ rendered visible by the action of the magenta dye. (Sec. Micro. Section.)

Mr. **Nevill** exhibited a new form of cell cut out in cardboard, containing seven divisions for seven different objects; it is very suitable for Foraminferæ, Diatomaceæ, &c., and may contain seven species, which will economise space in cabinets and facilitate exhibition. The perforations are about ¼ of an inch in diameter, and are made with a saddler's hand-punch; the disc is then covered with black varnish and secured to the glass slide.

Mr. J. B. **Dancer** exhibited new cells for opaque objects of various sizes; they are made of composition, and are cast in a mould.

Mr. J. G. **Dale** exhibited with the polariscope, crystallised films of Santonine, and of Picrate of Aniline; they are very rich in colour, and some of the forms are believed to be new.

## MATHEMATICAL AND PHYSICAL SECTION.

### Annual Meeting, March 5th, 1863.

**Joseph Baxendell**, F.R.A.S., Vice-President of the Section, in the Chair.

On the motion of Mr. **Atkinson**, seconded by Mr. **Dickinson**, it was resolved that Rule 4 be altered by adding the following words: "No person shall hold the office of President for more than two years in succession."

The following gentlemen were elected officers of the Section for the ensuing year:—

**President.**

MR. JOSEPH BAXENDELL, F.R.A.S.

**Vice-Presidents.**

MR. ROBERT WORTHINGTON, F.R.A.S.
MR. E. W. BINNEY, F.R.S., F.G.S.

**Treasurer.**

MR. GEORGE MOSLEY.

**Secretary.**

PROFESSOR R. B. CLIFTON, M.A., F.R.A.S.

In illustration of the abnormal character of the season, two barometric curves, neatly laid down on engraved forms by Mr. Sidebotham, from observations made by him at Ashton-upon-Mersey during the months of January and February last, and a summary of meteorological observations taken at Thelwall by Mr. Atkinson, F.G.S., during the month of February, were communicated to the Section. The mean height of the barometer at Thelwall for February, reduced to 32° Fahr., and to mean sea level, had the unusually high value of 30·218 inches; the mean temperature was 42·5°; and the total fall of rain was only 0·811 inches.

Mr. Baxendell communicated a table which he had calculated, showing the monthly sums of the oscillations of mean daily temperature at Greenwich during the thirteen years 1848—60; and also the mean daily values for the different months. The mean monthly results were as follows:—

| | ° | | ° |
|---|---|---|---|
| January ...... | 102·8 | July.......... | 82·8 |
| February...... | 84·9 | August ...... | 78·4 |
| March ........ | 83·4 | September .... | 72·3 |
| April ........ | 90·6 | October ...... | 84·4 |
| May.......... | 83·9 | November .... | 94·5 |
| June ......... | 87·3 | December .... | 107·1 |

The oscillations of mean daily temperature have hitherto been strangely neglected by meteorologists, although they undoubtedly form an important element in the question of climate. The curve laid down from the above numbers differs considerably from that of any other element of temperature, the principal maximum of disturbance occurring in December, and the principal minimum in September; and it is perhaps worthy of remark that the course of this curve appears to bear a well marked relation to the annual progress of the growth and decay of plants, the period of maximum disturbance of mean daily temperature corresponding with that when the vital principle in plants is least active; and the time of minimum with that when the ripening of the more important cereals and fruits takes place.

Mr. Baxendell also drew attention to the attempt which is now being made to organise an association for the systematic observation of variable stars, and presented to the Section a chart of the vicinity of the variable star R *Canis minoris*, neatly executed by Mr. George Knott, F.R.A.S., of Woodcroft Observatory, Cuckfield, Sussex. This chart extends over one square degree: the central portion of half a degree square includes all the stars which are visible with a 7-inch object glass, and the outer portion all Argelander's stars to the 9·5 magnitude. A list of twelve comparison stars is given, the magnitudes of which have been photometrically determined by the method of limiting apertures. In reference to the objects of the Association, Mr. Baxendell remarked that the importance of a careful study of the phenomena of variable stars will be apparent when it is considered that all the so-called fixed stars—our own sun included—are supposed to have a general similarity of constitution; and as several eminent astronomers have doubted whether the emission of light from the sun is absolutely constant, it is not improbable that an extended knowledge of the phenomena of variable stars may ultimately assist us in obtaining a more intimate acquaintance with the constitution and phenomena of the great luminary of our own system, and lead to the detection of changes at present not recognised, but which in their influence upon the earth may have an important bearing upon many questions of considerable interest in magnetical and meteorological science, and also upon the economy of the various forms of vegetable and animal life. The recent results of spectral analysis tend to show that the light of the sun has a general resemblance to that of the class of stars to which nearly all the known variables belong, and thus give additional weight to the probability that it may also be slightly variable.

Professor CLIFTON made a communication, in which he endeavoured to show that the principles of the mechanical theory of heat afford an explanation of phenomena attending the production of light by bodies, either when this production of light is due to incandescence or to fluorescence. His remarks and explanations were illustrated by a series of highly interesting experiments.

## Ordinary Meeting, March 24th, 1863.

E. W. **Binney**, F.R.S., F.G.S., President, in the Chair.

Among other donations for which the thanks of the Society were voted, was a framed photograph of the Executive Committee of the British Association in Manchester, by Mr. Alfred Brothers.

The following letter was read:—

Sir,

1. I have to call attention to some errors in Mr. Dyer's representation of the nature and contents of a short Essay of mine, which was read to the British Association, in 1854, and reprinted, with a few abridgments and emendations, in the "Engineer" for the 23rd of January, 1863, and which Mr. Dyer criticises in his "Notes on the Action of Heat and Force upon Matter," read to the Literary and Philosophical Society of Manchester on the 11th of March.

2. Those errors consist, in the first place, of a complete misconception of the object of my Essay; and in the second place, of misquotations, in which, by the alteration of some words and the omission of others, the meaning of my statements is perverted or reversed.

3. I have no doubt that those errors arose merely from inadvertency on the part of Mr. Dyer; and I trust that, when they are pointed out, his own sense of justice will make him see the propriety of having them corrected before his Paper is published at length in the "Transactions."

4. With regard to the object of my Essay, Mr. Dyer says (at page 78 of the abstract of his Paper), "Yet we have seen two elaborate essays, by Dr. W. J. M. Rankine and Mr. Tyndall, each based on the *force-heat* assumption, but without affording any solid proofs of its application to the phenomena adduced by them."

5. This passage is calculated to convey the idea that my Essay *professes* to give proofs of the mechanical theory of heat. But that Essay does not profess to give a single proof of that theory. It is entitled, "*Outline* of the Theory of the Mechanical Action of Heat:" it gives a sketch of the general principles and results of that theory only; and for the *proofs*, it refers to published detailed accounts, by various authors, of experimental researches, and calculations founded on the results of those researches, the whole of which published accounts are in the Library of this Society.

6. The proofs of this, as of every other physical theory, consist in a mass of experiments and calculations, which cannot be condensed within the limits of a brief popular Essay; and in order to examine and test those proofs, it is necessary to go to the fountain-head of information; that is, to the detailed accounts of calculation and experiment.

7. As to defective and erroneous quotations, I have in the first place to point out the following. The words in the genuine passage, whose omission in Mr. Dyer's quotation has the effect of materially altering the meaning of the passage, are enclosed in square brackets, thus: [ ].

| THE GENUINE PASSAGE. | THE QUOTATION. |
|---|---|
| 5. *Threefold Effect of Heat.*—The communication of heat to a substance produces [in general, three kinds of effects (setting aside chemical, electrical, and magnetic phenomena, as being foreign to the subject of the present paper):—]<br>1st, An increase of temperature and expansive pressure; that is to say, an increased tendency to the communication of heat to | Mr. Rankine states "that heat produces an increase of temperature and of expansive force in bodies; that the change from the solid and liquid states to the gaseous state is accompanied by an increase in volume; that heat which produces those effects is known by the name of sensible heat, as retaining the form of heat, and in short making the body hotter." |

other bodies, and to the development of mechanical power by expansion.

[2dly, A change of volume; which, under a constant pressure, is an *increase* for every substance, except some liquids near their freezing points.]

3dly, A change of molecular condition; as from the solid to the liquid state, or from the liquid or solid to the gaseous state, or any imperceptible change of molecular arrangement; the change to the gaseous state being always accompanied by an increase of volume.

The heat which produces [the first of] those effects is known by the name of *sensible heat*, as retaining the form of heat, and, in short, *making the body hotter*.

[The heat which produces the second and third of those effects is called *latent heat*, as having disappeared in developing a mechanical effect, and being capable of reproduction by reversing the change which caused it to disappear.]

8. It is evident that the omission of the words in brackets has the effect of precisely reversing the meaning of the definition which I have given of sensible as distinguished from latent heat.

9. I have next to point out that Mr. Dyer, in criticising the 4th Article of my Essay, represents me as making a certain statement, commencing as follows—"It is natural to suppose that the motion, during this phenomenon, has not been really destroyed," &c.—in order to account for what he calls the "*non-production* of heat by the moving force exerted in waterfalls."

10. Now there does not occur in my Essay one word about any such "*non-production* of heat," nor one phrase or allusion which by any conceivable amount of ingenuity can be construed to bear any such meaning. The "phenomenon" really referred to in the passage above cited, and distinctly described in the paragraph preceding it, is the well established fact of the *production* of heat by the fall of water; and that not as a thing to be accounted for, but as a fact to be reasoned from. In this instance my meaning has again been exactly reversed in Mr. Dyer's account of my Essay.

11. I shall not enter into any controversy respecting the theory of heat; for it appears to me that physical theories are questions to be settled, not by argument, but by experiment and calculation; and I have nothing at present to add to the experiments and calculations already published.

12. My only object in writing this letter is that my brief sketch of the mechanical theory of heat, which Mr. Dyer has criticised, may be judged according to its real contents, and may not be erroneously quoted in a publication so important as the *Transactions of the Literary and Philosophical Society of Manchester*.*

I am, Sir,
Your most obedient Servant,
W. J. MACQUORN RANKINE.

GLASGOW, 21ST MARCH, 1863.

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* Mr. Dyer states that in making the quotations from the *Engineer* he had studied brevity, and had no thought of conveying any other than Mr. Rankine's meaning.—ED.

Mr. **Sidebotham** exhibited a large sheet of photo-lithographs, executed by Mr. Dean, of Douglas, Isle of Man. They consisted of copies of wood engravings, copper-plate, pen drawings, writing, music, and letter-press, all copied by the camera and then transferred to stone, and printed in the ordinary manner with printing ink. Mr. Sidebotham said he considered this by far the best specimen yet exhibited, from the extreme sharpness of the lines, and thought as this was a branch of art likely to be much used, it was of great interest to see its present state of perfection. He also called attention to the great facility this plan offered to the forger of Bank notes, &c.: hitherto, although there was no difficulty in getting a perfect *fac-simile* of a note or cheque, the fact of its being printed on paper by silver or iron salts made its detection easy; now that printing ink could be used a perfect *fac-simile* in every respect could be made, so far as the printing is concerned. Mr. Sidebotham thought some steps should be taken in the matter, and suggested as the most simple mode of defeating the photographer to use yellow paper and print in blue. So far as at present known, there would be no possibility of copying such a document by any process of photography.

Mr. **Sidebotham** read a Paper "On the Planet Mars," by James Nasmyth, Esq., and exhibited at the same time a large drawing of the planet, as seen by Mr. Nasmyth on the evenings of September 25th and October 11th, 1862, by the aid of his fine reflecting telescope of twenty inches diameter. Mr. Nasmyth called special attention to the patch of snow, as it was considered to be, at the south pole of the planet. This was unusually bright and round, like a white wafer on the pole of a globe; it had also a cliff-like edge. This supposed patch of snow does not coincide with the true pole of the planet, but, like the arctic poles of the earth, or poles of minimum temperature, is slightly eccentric.

Mr. Nasmyth also called attention to the different shades of the planet as exhibited in the drawing—which was a faithful representation of the appearance of the planet—and the probability of their being land and water. One ruddy spot in the blue band of the supposed sea he could not help thinking to be an island, and, as its first discoverer, he claims it to be called Nasmyth's Island. He also made some remarks on the superiority of the reflecting over the achromatic telescope, in giving the true colours of the heavenly bodies, and gave his explanation of the supposed cause.

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Mr. W. H. L. Russell, A.B., wishes it to be stated that in consequence of errors in the printing of the abstract of his Paper "On the Solution of the Differential Resolvent," in the last number of the Proceedings, he fears that the results given as the solution of the differential resolvent will be unintelligible to the reader.

## MICROSCOPICAL SECTION.

March 16th, 1863.

Mr. Joseph Sidebotham, Vice-President of the Section, in the Chair.

Mr. **Watson** presented specimens of Jungermannia tomentella and asplenoides, collected on Baguley Moor.

Mr. **Sidebotham** presented specimens of the following mosses, in fruit: Fissidens exilis, Fissidens adiantoides, Grimmia pulvinata, Weissia controversa, Bryum atropurpureum, &c., in a good state for microscopical examination.

Mr. J. G. **Dale**, F.C.S., presented a specimen of crystallised film of picrate of aniline; and in a note to the Secretary explained his method of preparation from picric acid and aniline. The equivalent of picric acid is 229; that of aniline is 93; and when dissolved in strong alcohol in those proportions by weight, mixed and set aside, the picrate of aniline will crystallise in yellow needles. The film for the microscope is formed from a solution of these needles in absolute alcohol, a drop of which being spread over a clean hot glass slide, the crystallised film is at once produced by the rapid evaporation of the alcohol, if the slide be at the proper degree of heat, which can only be found by repeated trials. If too hot, the salt will melt and become partially decomposed; if not hot enough, it will be crystallised in needles, or be deposited as an amorphous film. When properly crystallised, circular radiated discs will appear with more or less regularity, showing with the polariscope very brilliant colours, and a black cross in the centre. The crystallised films may be mounted in *new soft* balsam; but a mixture of chloroform and balsam dissolves them immediately.

The Natural History Society presented for distribution amongst the members a number of beetles not required for the Museum.

Mr. Nevill reported upon the fossil Foraminferous shells found in the Montreal deposit presented by Mr. R. D. Darbishire at the last meeting. They were mostly in a fine state of preservation, and many were as perfect as recent shells. He found—

Polystomella,
Nonionina umbilicatula,
Polymorphina lactea,
Miliolina seminulum,
Entoselenia squamosa, var: scalariformis,
Ditto of a peculiar form and rare
Entoselenia marginata,
Ditto globosa, very fine,
Patalina corrugata,
Textularia,
Dentalina,
Lagena vulgaris.

The Polystomella and Nonionina were in great profusion, the other kinds were scarce; but Mr. Nevill was of opinion that remarkably fine specimens might be found of all the various kinds, if there were a larger quantity of material to operate upon. Mr. Nevill was indebted to the worthy President of the Section, Professor Williamson, for verifying the names, and he presented to the Section mounted and named slides for the cabinet. No diatomaceæ were found amongst the material.

Dr. Alcock exhibited a young living salmon, about 14 days old, attached to part of the ovum. Dr. Alcock particularly called attention to the form of the vertebral column, which, whilst young, is similar to that of the lower grade of cartilaginous fishes when fully grown; the skeleton of the salmon, however, becomes gradually changed, until at maturity it is that of the higher class of osseous fishes.

Dr. Alcock also exhibited a lingual riband of the Patella athletica, from Bray, in Ireland; he compared it with that of the common limpet, Patella vulgata, and pointed out the differences in the form of the teeth.

Dr. Roberts exhibited some mounted specimens of blood-corpuscles from an albuminous urine, which showed an appearance as if the contents of the cells had separated from the cell-wall, and become aggregated round the centre like a nucleus. When these corpuscles were treated with magenta, the central portion was either not coloured at all, or only faintly so, whereas the circumferential portions became deeply tinted. By treating fresh blood with an excess of a solution of carbolic acid this appearance could be produced at will. In the blood-corpuscles of the fowl a similar effect was produced by the carbolic acid solution; the cell contents appeared to detach themselves from the cell-wall and to collect round the nucleus. The appearances presented, strongly suggested the idea that the cell envelope of the blood-disc was a double membrane; that the inner separated under certain circumstances from the outer membrane and shrank in toward the centre. Dr. Hensen, of Kiel,* seems to have convinced himself that such is the case in the blood-disc of the frog, and he compares the inner membrame to the primordial utricle of the vegetable cell. Of the prolongations described by Dr. Hensen as stretching radially between the shrunk inner membrane and the outer one, Dr. Roberts saw nothing. If the said view of the structure of the blood-cells were substantiated, it would greatly facilitate the explanation produced on these cells by magenta and tannin.

Mr. Charles O'Neill, F.C.S., exhibited a mounted fibre of Orleans cotton, torn by a gradually increasing weight suspended to its extremity. It had sustained a weight (gradually increased) of 162 grains for many minutes. Mr. O'Neill stated that there were 143 such fibres in ·01 grains of cotton, each fibre therefore weighing less than the ten thousandth part of a grain. The strongest fibres were capable of supporting more than two million times their own weight. He is engaged in making experiments upon the tensile strengths of various fibres by a special apparatus, but they are not yet completed.

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* Siebold and Kölliker's Zeitschrift, for 1861, p. 263.

Mr. Brothers exhibited a number of fresh water insects, larva, &c.

Mr. Parry exhibited the transverse section of a Fossil Palm, from the Island of Antigua.

## Ordinary Meeting, April 7th, 1863.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Mr. Edmund Salis Schwabe, B.A., was elected an Ordinary Member of the Society.

Messrs. Robert Worthington and G. V. Vernon were appointed Auditors of the Society's Accounts for the present Session.

Mr. Brothers exhibited and described an apparatus for determining the magnitudes of stars. (See Proceedings of Physical Section for April 2nd.)

Professor Roscoe communicated the following extract from a letter he had just received from Professor Bunsen, respecting the atomic weight of cæsium. Professor Bunsen formerly found the atomic weight of this metal to be 123; since then, Messrs. Johnson and Allen, Yale College, United States, found, by operating on large quantities, the number to be 133. He writes, "The atomic weight Cs.=133 is quite right. The difference is caused by an error of about one per cent on the chlorine contained in the chloride of cæsium, owing to an amount of impurity which cannot be separated according to the method I first used for purification. I have employed all the three grammes of Cl Cs, with which I was obliged to make the whole of the investigation of the cæsium compounds for another determination of the atomic weight of the metal, and I find the same number (133) as the Americans found, but by quite a different method. The alteration from 123 to 133 makes a difference in the analysis of most of the salts, of from a quarter to one per cent in the percentage composition."

Mr. G. C. Lowe exhibited part of a mercurial seconds pendulum, which, from its mode of construction, bears a strong resemblance to a horse-shoe magnet, and possesses considerable magnetic power. It is what is called the stirrup, and has attached to its lower extremities a circular brass disc, upon which the jar of mercury is placed. The stirrup is attached to the lower end of the pendulum rod, and has two steel rods, about ten inches in length, connected at their upper ends by a short steel bar, so that when magnetised the lower ends form one a north and the other a south pole. It had been suggested by Mr. Baxendell that some irregularities which are observed in the rates of clocks may be explained by referring them to variations in the intensity of the earth's magnetism influencing a pendulum which has thus become magnetised. He had noticed in two clocks which have mercurial pendulums sudden changes of rate amounting to nearly or in some instances quite one second per day. There were no considerable changes of temperature or of the atmospheric pressure at the time, and the compensation is so nearly perfect that so great a deviation from the rate could not be due to change of temperature. A very brilliant display of aurora borealis was observed by Mr. Baxendell on the evening when the last remarkable change of rate took place, which favours the view now suggested, viz., that the lower part of the pendulum having become magnetised, an increase or diminution of the intensity of the earth's magnetism may have the effect of accelerating or retarding the motion of the pendulum. Mr. Lowe suggested that if the rods forming the stirrup were connected with brass at the top instead of steel, they would have much less force as magnets. In a pendulum also under Mr. Baxendell's observation, the disturbance of rate was much smaller in amount, scarcely exceeding one-third of the change that was found in the two clocks already mentioned. This pendulum consists of a long steel rod and two shorter ones, and has a very correct compensation effected by a zinc tube. This pendulum has another advantage, viz., that the steel rods and the zinc tube are so nearly the same in size that sudden changes of temperature affect all the parts nearly simultaneously, whereas in the mercurial pendulum the steel rod is much more quickly affected than the large mass of mercury; the compensation therefore does not take place soon enough to prevent a small error in the clock's rate.

A letter by Mr. DYER was read, relative to Dr. Rankine's reply, inserted in the Proceedings of March 24.

Mr. Dyer disavows all intention of unfairness towards Dr. Rankine, and states that although the brevity which it was necessary to observe in drawing up his abstract compelled him to give only detached passages, he conceived that the sense could not be considered to be perverted by reason of the omissions. He had always felt and expressed the highest regard for Dr. Rankine's genius and learned researches.

A Paper was read entitled "Note as to two Events in the History of Steam Navigation," by W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S., Hon. Member of the Society.

(1.) An interesting Paper was lately read to this Society by Mr. Dyer, containing a history of a series of important events in the progress of Steam Navigation.

(2.) It is to be regretted, however, that the author has noted either very slightly or not at all, what appears to have been an event of paramount importance in the first adaptation of the double-acting cranked steam engine to drive a paddle wheel. Before that adaptation was made, the success of all attempts at steam navigation, such as those of Jouffray, Rumsey, Titch, Miller, and Taylor, &c., had been only temporary, because of the rudeness of the machinery for communicating motion from the piston to the shaft.

(3.) That first adaptation was unquestionably accomplished by William Symington in 1801, as is proved by authentic documents which have been published by Mr. Woodcroft in his "Origin and Progress of Steam Navigation." Symington, instructed by the failure of the ratchet-work engine which he had made for Miller's boat, fitted up the "Charlotte Dundas" in 1801, with a double-acting horizontal cranked engine, and this made her what Mr. Woodcroft has justly called "the first practical steamboat." Her speed, when running alone, and not towing other boats, was six miles an hour.

(4.) The use of this vessel was abandoned, not from any fault in her construction or working, but because the Directors of the Forth and Clyde Canal feared that she would damage its banks. Yet the man in all Britain who possessed, at that time, the greatest practical experience of the working of canals—the Duke of Bridgewater—was not deterred by any such apprehension from ordering, in 1802, *eight* similar vessels, from Symington, to be used on his canal.

(5.) The death of the Duke of Bridgewater, early in the following year, prevented the execution of that order. But Symington had evidently done all that lay in his power, and all that was necessary, to convert the steamboat from an awkward piece of experimental apparatus to a practically useful machine; and the honour paid to his memory ought not to be lessened because the career of his invention was cut short by a misfortune.

(6.) There is nothing in this to detract from the honour which is justly paid to Fulton, as having been the first to practise Steam Navigation on a great scale, as a commercially profitable art.

(7.) Another event passed over in the Paper to which I have referred, is the first introduction of commercial Steam Navigation into Europe, which was effected on the river Clyde, in 1812, by Henry Bell, as is proved by documents cited in Mr. Woodcroft's work, already referred to.

Dr. R. ANGUS SMITH said that he had been using an amalgam of sodium for the production of hydrogen. When doing this it was found that the amalgam had become one mass of crystals. These crystals are fine needles or prisms of considerable length; some may be traced in the tangled mass for about half an inch; they present hollow ends, although it was not possible to trace this hollowness through the whole length of the crystals. They are hexagons, and contain 9·47 per cent of sodium; an excess of mercury remains; 10·3 per cent would be equal to 2 at. of mercury and 1 of sodium. He had not seen any notice of an amalgam of similar composition. It can be examined only through glass, as it rapidly absorbs oxygen from the moisture of the air, and cannot be retained in the air many seconds without change being perceived. If this change be examined under a microscope, bubbles of hydrogen are seen to rise from every corner, extremely minute certainly, so that many millions would be required to cover a square inch, but affording one of the most convincing and direct proofs of the existence of moisture. This property may in many cases be found to be of value. The inclination to combine with oxygen when dry seems comparatively slight.

Dr. SMITH mentioned also a method of separating lime from phosphoric acid, which, as far as he was aware, was new. Many very elaborate methods had been given. After adding tartaric acid to a solution of phosphate of lime in muriatic acid, the lime may be precipitated by oxalate of ammonia and ammonia as in other solutions. Carbonate of lime has been obtained in this way free from every trace of phosphate, and the solution has been freed entirely from lime. Tartaric acid prevents the precipitation of phosphate of lime in weak solutions by ammonia, but has no influence over the oxalate of lime. The true action of tartaric and similar acids in preventing precipitation deserves a more careful inquiry than has yet been made.

## PHYSICAL AND MATHEMATICAL SECTION.

April 2nd, 1863.

Joseph Baxendell, F.R.A.S., President of the Section, in the Chair.

A Paper was read entitled "Notes of Observations of the Speed and Order of Succession in Magnitude of Waves observed in Gales of Wind off the Cape of Good Hope," by Mr. Thomas Heelis, F.R.A.S.

The highest waves measured were about 35 feet in height from the trough, no broken crests having been measured. Their speed varied (the force of the wind being 8 according to the Board of Trade scale, and equal to weather in which a ship on a wind can just carry treble-reefed topsails) from twenty to twenty-three miles per hour, the breadth of trough being 300 to 350 feet. The observations show that usually the succession of magnitudes (or heights) returns in series of twelve waves, the first and second of each series being very large, the sixth or seventh being also large but inferior in magnitude to the first and second, and the intermediate ones being small. The observations show that waves are limited in length, measured along their bases, the crest being apparently at the middle point of the base, and the length varying with the altitude of the crest, and that the order of succession of magnitudes depends upon their being arranged so that the crest of one wave follows on the same line as the lower flanks of a preceding wave. The speeds also of waves appear to vary, so that a following wave often coalesces with and is increased in size by absorbing one immediately preceding. When a wave is first formed it is small, and increases in size in its progress, until the crest topples over in foam, after which the height decreases rapidly, and there seems reason for thinking that if ordinates were drawn so as to represent the height of any wave at different periods of its existence, its height would be found to coincide with Mr.

Scott Russell's wave-line curve. The length of a wave in open water, measured along its base, seems also to depend upon and bear a definite relation to the width of the trough between two successive waves. The speed of the waves is not so much affected as would naturally be imagined by the force of the wind. In a moderate gale they run as fast as in a heavy one. It is otherwise with their height.

Mr. ALFRED BROTHERS read the following Paper "On an Improved Diaphragm for determining the Magnitudes of Stars."

At a recent Meeting of this Society, when Mr. Baxendell made a communication respecting one of the variable stars of small magnitude, it occurred to me that the system used by him for determining the variability of the smaller stars, must be not only tedious but often unsatisfactory, owing to the necessity for leaving the position of observation for the purpose of changing the diaphragms.

When apertures of various diameters are used as diaphragms, placed at the end of the dew-cap of large telescopes, and particularly refractors, it must at all times be difficult to make these changes, even when the advantage of clock-work motion is available; but, with telescopes whose motions must be controlled by hand, the possibility of determining with accuracy such results as the one-tenth of a magnitude must be very uncertain; as, unless an assistant be at hand to change the diaphragms during the observation, the observer must be constantly liable to lose the star at the moment it becomes necessary that it should be in the centre of the field of view, and the eye in position to note the effect produced by the decrease of light on the change of diaphgram being effected. Several contrivances have been adopted to contract the aperture of the telescope, such as square and hexagonal forms, opened and closed mechanically; but hitherto, it is believed, no attempt has been made to retain the *circular* form of aperture.

In order to facilitate observations of this character, and to enable the observer to dispense with an assistant, I have recently adapted to my telescope (of 5in. aperture) an apparatus which may be termed the "Iris Diaphragm," as its action resembles to some extent the expansion and contraction of the pupil of the eye. The invention was, I believe, patented by Mr. Jennings, of London, as a capsule for closing jars, bottles, &c., but has been superseded by a more simple method for the same object. If found to be practically successful for the purpose I have now applied it to, it will, I think, be extremely useful in the Observatory, and form a desideratum long required by Astronomers. A great saving of time will be effected, and the results will be regarded with greater confidence.

The following is a description of the invention, and the method I propose for adapting it for astronomical purposes.

The *invention* consists of two rings of metal, one working within the other, of nearly the same diameter, but differing in width, to the outer edges of which, is attached a piece of sheet india-rubber joined at the ends, and so placed that when the rings are moved in opposite directions the india-rubber is stretched, and closes the aperture of the rings when they have made about one half of a revolution, the aperture remaining circular or nearly so, its perfect circularity depending on the proper attachment of the india-rubber.

The *adaptation* I have effected in the following manner. A flat ring of tin with a flange is made to fit accurately the end of the dew-cap of the telescope. To the flange the diaphragm is soldered; to the broader ring of the diaphragm is fixed a circle of brass, having teeth cut rather more than one-half round its circumference. Fixed to the ring, which is attached to the dew-cap, is a bearing for a small pinion, which gives motion to the half wheel by means of a slender rod (of brass tubing) the length of the telescope, having at the eye-piece end, a handle of convenient form, and worked by the left hand. The rod is supported at the eye-piece end of the telescope, on a bearing attached to a metal collar made to clip the telescope by a screw, and attached to this bearing is a ratchet wheel, which prevents the too rapid reverse motion of the india-rubber diaphragm. The ratchet is not absolutely necessary with the present apparatus, owing to the friction of the pinion against the wheel, preventing the india-rubber contracting too rapidly.

It is not considered necessary to give the dimensions of the parts of the apparatus, as in every case these must vary with the diameter of the object-glass of the telescope. The principal point to be attended to, is the width of the broader ring, which is covered inside by the india-rubber, and this must be sufficiently wide to close the aperture when in use. In order to prevent the over-winding of the diaphragm, stops are placed at each end of the teeth on the half wheel.

An index of some kind will be necessary to make the apparatus complete. A pointer might be fixed on the end of the dew-cap, and a scale might project from the wheel. The diameter of the various openings could also be determined by the ear, the number of teeth in the wheel and pinion being known, the ratchet wheel could be so adapted, that a certain number of *clicks* would indicate that a certain aperture is then in use. This method, however, might be tedious to the observer, owing to the necessity for counting. The index and scale will perhaps be more convenient, and if the light from the sky should not be sufficient to show the reading of the scale, the micrometer lamp could be placed so as to illuminate the scale and index only.

The additional weight at the end of the telescope renders a counterpoise necessary, and this is effected on my telescope by a slight addition to the counterpoise already attached.

It may perhaps be necessary to say, that I have as yet tried the apparatus merely for the mechanical working, but am very sanguine that it will answer the purpose intended more effectively, than any other method I am acquainted with.

Mr. G. V. Vernon, F.R.A.S., communicated the following returns of the Rainfall for 1862.

## Royton, near Oldham.

### By John Heap, Esq.

The rain-gauge is 10 inches diameter; and is 3 feet above the ground, and 500 feet above sea level.

| 1862. | Fall in Inches. | Days. | Mean of 27 Years, 1836—1862. |
|---|---|---|---|
| January | 2·622 | 17 | 2·168 |
| February | 0·736 | 11 | 2·092 |
| March | 3·073 | 17 | 2·235 |
| April | 2·709 | 24 | 2·142 |
| May | 4·636 | 23 | 2·360 |
| June | 3·332 | 20 | 3·080 |
| July | 4·332 | 24 | 3·399 |
| August | 2·545 | 16 | 3·563 |
| September | 4·064 | 16 | 2·905 |
| October | 6·361 | 19 | 3·620 |
| November | 1·737 | 12 | 2·980 |
| December | 5·091 | 21 | 2·607 |
|  | 41·238 | 220 | 33·151 |

## Eccles, near Manchester.

### By Thomas Mackereth, M.B.M.S.

Gauge 2 feet above the ground, and 105 feet above the sea.

|  | 1862. | Fall in Inches. | Days it Fell. |
|---|---|---|---|
| 8·005 | January | 2·820 | 22 |
|  | February | 0·799 | 9 |
|  | March | 4·386 | 17 |
| 9·533 | April | 2·460 | 18 |
|  | May | 3·820 | 21 |
|  | June | 3·253 | 25 |
| 10·986 | July | 4·217 | 21 |
|  | August | 2·224 | 16 |
|  | September | 4·545 | 17 |
| 9·140 | October | 4·589 | 24 |
|  | November | 1·675 | 13 |
|  | December | 2·876 | 22 |
|  | Total | 37·664 | 225 |

## Annual Meeting, April 21, 1862.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Mr. Leopold Hartley Grindon was elected an Ordinary Member of the Society.

The following reply to Dr. Rankine's Paper, read at the last meeting, was communicated by Mr. Dyer: —

In Dr. Rankine's Paper, entitled, "Note on two Events in the History of Steam Navigation," he calls attention to my Paper, "On the Introduction of Steam Navigation," and expresses regret that "the Author has noted either very slightly, or not at all, an event of paramount importance:— the first adaptation of the double acting cranked steam engine, to drive a paddle-wheel." Now, I own to have taken no notice at all of this double cranked motion, simply because I did *not* consider it of much importance in attaining success in *steam navigation.* I had before me Mr. Woodcroft's interesting treatise, and I fully appreciated his advocacy of the claims of Mr. Symington for having "fitted up the 'Charlotte Dundas,' as the first practical steam boat, in 1801." This *fact* is fairly stated in my Paper.

I aimed to place before the Society the several inventions and discoveries relating to the use of *steam power* to supersede that of wind to navigate vessels, and to prove that the final *success* was due to the invention of *Watt's steam* engine. No great stress need be laid on the double cranked action, or on any of the other methods used for *transmitting* the power to the paddle wheels. The principles involved in overcoming the resisting forces by steam power in navigation, are entirely apart from those relating to the mechanical means of converting rectilinear into circular motion. It seems strange that such able engineers as Dr. Rankine and Mr. Woodcroft should have given this prominence to the said double crank action. Whatever may have been the degree of success in the case of the "Charlotte Dundas" in 1801, and whatever may be said about the bar to further progress of Symington's inventions by the *Duke's death*, the fact remains clear that his schemes died out with the Duke, as no more was heard of them after the "Charlotte Dundas," and from some cause, she was discontinued; and it was in fact fifteen years after her advent before the "Margery" came out from the same waters to enter the Thames, in 1816. What shall be said of the "enlightened" of Glasgow, if "Symington's practical steam boat" was suffered to rot and be forgotten, leaving no successor, and the inventor himself to remain unrewarded and unnoticed for fifteen long years?

Through the limited space allowed for the abstract of my paper, Dr. Rankine was misled respecting the other *event*, also cited by him from Mr. Woodcroft's book, namely,—that I had "passed over the first introduction of steam navigation into Europe, by Henry Bell, in 1812;" for in my paper due notice is taken of this well known experiment of Mr. Bell. Although his trial boat proved a failure, on account of its being a very small one, and of his want of pecuniary means for continuing or extending the experiment, and the lack of any aid or encouragement afforded him by others, yet his trial boat served as the model for constructing the "Margery," three years after, which, as I have before said, was a success, and the first steam boat on English waters. I therefore think that due honour should be paid to the name of Henry Bell, for his spirit and enterprise, and a due stigma cast upon the capitalists who allowed him to sink under pecuniary pressure, from which he ought to have been relieved by them in so important an enterprise.

## REPORT OF THE COUNCIL.

The following Report of the Council was read by one of the Secretaries:—

At the commencement of the present session, the number of ordinary members was 204; during the course of the session 18 members have resigned; 2 have been struck off the list as defaulters; one member, Mr. G. D. Fleming, has died; and 4 new members have been elected. The present number of ordinary members is therefore 187. No alteration has taken place in the list of honorary or corresponding members.

Your Council are glad again to be able to refer to the very satisfactory Financial Statement of the Treasurer, from which it appears that the balance in hand this year amounts to £320. 18s. 7d., against £248. 6s. 7d., in hand last year. Your Council are likewise glad to remark the increasing estimation in which the Proceedings of the Society are held by the scientific and reading public, as evidenced by their being regularly printed in full in several literary and scientific journals.

The valuable library of literary and scientific reference, which the Society now possesses, has during the past year been enriched by the additions of the memoirs and transactions of the numerous academies and scientific societies throughout the world, with which the Society is now connected. These additions are noted in the Librarian's Report annexed.

Amongst numerous gifts of literary and scientific value which have been made to the Society during the past year, your Council desire to mention the donation by Dr. W. C. Henry, of an extensive collection of letters, papers, and other documents belonging to the late Dr. Dalton; and also several manuscripts, diagrams, &c., belonging to the late Professor Hodgkinson, and presented by his widow. These papers, many of which have a high scientific value, are preserved in the archives of the Society.

The following is a list of the papers and communications which have been brought before the Society during the present session:—

*October 7th*, 1862.—"Memoir of the late Professor E. Hodgkinson, Part 2nd," by Robert Rawson, Esq., Hon. Mem.

*October 21st*, 1863.—"Notice of a Compressing Air Pump," by Dr. J. P. Joule, F.R.S.

*November 4th*, 1862.—"On a certain class of Linear Differential Equations," by the Rev. Robert Harley, F.R.A.S.

"Note on Differential Resolvents," by William Spottiswoode, Esq., M.A., F.R.S., communicated by the Rev. Robert Harley, F.R.A.S.

*December 2nd*, 1862.—"On certain Linear Differential Equations," by James Cockle, Esq., M.A., F.R.A.S., F.C.P.S., &c., communicated by the Rev. Robert Harley, F.R.A.S.

*December 16th*, 1862.—"On the Influence of the Earth's Rotation on Winds," by Thomas Hopkins, Esq., M.B.M.S.

"On the History of the Mechanical Theory of Heat, by M. Seguin," communicated by Dr. J. P. Joule, F.R.S.

*December 30th*, 1862.—"On the New Red Sandstone and Permian Formations as Sources of Water Supply for Towns," by Edward Hull, Esq., B.A., F.G.S., communicated by the President.

*January 13th*, 1863.—"Note on the Permian Deposit of the North-West of England," by E. W. Binney, Esq., F.R.S.

*January 27th*, 1863.—"On a New Variable Star U Tauri," by Joseph Baxendell, Esq., F.R.A.S.

"On Ocean Swell," by Thomas Heelis, Esq., F.R.A.S.

*February 10th*, 1863.—"Notes on the Introduction of Steam Navigation," by J. C. Dyer, Esq.

"On a Barometer for Measuring small Atmospheric Disturbances," by Dr. J. P. Joule, F.R.S.

*February 24th*, 1863.—"On the Spectrum of the Flame Evolved in the Bessemer Process," by Professor Roscoe, B.A.

"On the Existence of a Crystallizable Carbon Compound and Free Sulphur in the Alais Meteorite," by Professor Roscoe.

"On Maximum Groups," by the Rev. T. P. Kirkman, M.A., F.R.S.

*March* 11*th*, 1863.—"On Bring's Reduction of the Equation of the Fifth Degree to a Trinomial Form," by the Rev. Robert Harley, F.R.A.S.

"On the Solution of the Differential Resolvent," by W. H. L. Russell, A.B., communicated by the Rev. Robert Harley.

"On a New and Extremely Sensitive Thermometer," by Dr. J. P. Joule, F.R.S.

"On Evidences of the Antiquity of Man in the Valley Gravels in the neighbourhood of Manchester," by E. W. Binney, Esq., F.R.S.

"Notes on the Action of Heat and Force upon Matter," by J. C. Dyer, Esq.

"On the Chemical Constitution of American Rock Oil," by C. Schorlemmer, Esq., communicated by Professor Roscoe.

*March* 24*th*, 1863.—"On the Planet Mars," by James Nasmyth, Esq., C.E., communicated by Joseph Sidebotham, Esq.

*April* 7*th*, 1863.—"On a New Diaphragm for Determining the Magnitudes of Stars," by A. Brothers, Esq.

"Notes on Two Events in the History of Steam Navigation," by Dr. W. J. Macquorn Rankine, F.R.S.

Several of these have been ordered by your Council to be printed in the Memoirs of the Society, and others are under consideration.

## LIBRARIAN'S REPORT.

Since the close of last Session 815 vols., 792 parts of vols., and 288 pamphlets have been added to the Library; of which 236 vols. and 43 parts of vols., by purchase, and the remainder by exchange or donations.

The periodical publications which the Librarian is authorised to procure for the Library by purchase, are:—

The London, Edinburgh, and Dublin Philosophical Magazine.
The Quarterly Journal of Pure and Applied Mathematics.
The Cavendish Society's Publications.
The Ray Society's Publications.
The Palæontographical Society's Publications.

The Natural History Review.
The Annals and Magazine of Natural History.
The Edinburgh New Philosophical Journal.
Les Annales de Chimie et de Physique.
Le Journal de l'Ecole Polytechnique.
Les Annales des Sciences Naturelles.
Le Journal de Pharmacie et de Chimie.
Les Archives des Sciences Physiques et Naturelles.
Poggendorff's Annalen der Physik und Chemie.
Annalen der Chemie und Pharmacie.
Journal für die reine und angewandten Mathematik.
Die Astronomische Nachrichten.

The Philosophical Transactions for 1845 and 1846 have been taken out from the Library without being entered in the Registers.

It is to be hoped that these volumes will be returned without delay.

On the motion of Mr. E. Hunt, seconded by Dr. Clay, the Report was unanimously adopted.

### HENRY MERE ORMEROD, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER. FROM 31st MARCH, 1862, TO 31st MARCH, 1863.

**Dr.**

| | | £. | s. | d. | £. | s. | d. |
|---|---|---|---|---|---|---|---|
| To Balance in the Bank of Heywood Brothers & Co. | | 220 | 7 | 9 | | | |
| ,, Balance in Treasurer's hands | | 27 | 18 | 10 | 248 | 6 | 7 |
| ,, Arrears of Subscriptions 1861-2, | 2 | 4 | 4 | 0 | | | |
| ,, Subscriptions 1862-3, | 173 | 363 | 6 | 0 | | | |
| ,, Half Subscriptions 1863, | 2 | 2 | 2 | 0 | | | |
| ,, Subscriptions in Advance 1863-4, | 2 | 4 | 4 | 0 | 373 | 16 | 0 |
| ,, Admission Fees | 4 | | | | 8 | 8 | 0 |
| ,, Memoirs sold—Vol. I. 3rd series | | 14 | 3 | 6 | | | |
| ,, ,, Former volumes | | 1 | 7 | 2 | | | |
| ,, Proceedings sold | | 1 | 7 | 5 | 16 | 18 | 1 |
| ,, Interest from Bankers | | 4 | 0 | 11 | | | |
| ,, Donation to Library | | 1 | 0 | 0 | 5 | 0 | 11 |
| ,, Sectional Contributions—Mathematical | | 2 | 2 | 0 | | | |
| ,, ,, Statistical | | 2 | 2 | 0 | | | |
| ,, ,, Microscopical | | 2 | 2 | 0 | 6 | 6 | 0 |
| ,, Photographic Society attendance, coals, &c., at Meetings | | 11 | 10 | 0 | | | |
| ,, Ditto, donation towards repairs | | 5 | 0 | 0 | 16 | 10 | 0 |
| | | | | | £675 | 5 | 7 |

| | £. | s. | d. |
|---|---|---|---|
| Library Fund | £39 | 16 | 0 |
| Compositions | 72 | 10 | 0 |
| General Balance | 208 | 12 | 7 |
| Total Balance | £320 | 18 | 7 |

**Cr.**

| | £. | s. | d. | £. | s. | d. |
|---|---|---|---|---|---|---|
| By Chief Rent | 12 | 8 | 10 | | | |
| ,, Fire Insurance | 5 | 15 | 0 | | | |
| ,, Property Tax | 6 | 7 | 6 | 24 | 11 | 4 |
| ,, Water Rent | 1 | 1 | 0 | | | |
| ,, Gas | 2 | 17 | 2 | | | |
| ,, Candles | 0 | 4 | 9 | | | |
| ,, Coals | 10 | 7 | 0 | | | |
| ,, Cleaning | 3 | 10 | 5 | | | |
| ,, Furniture | 4 | 14 | 7 | | | |
| ,, Repairs | 33 | 11 | 1 | | | |
| ,, Receipt Stamps | 0 | 19 | 2 | 57 | 5 | 2 |
| ,, Keeper of the Rooms—Salary | 60 | 0 | 0 | | | |
| ,, Assistant to Keeper | 15 | 12 | 0 | 75 | 12 | 0 |
| ,, Tea, Coffee, &c., for Members | 18 | 6 | 4 | | | |
| ,, Postage and Parcels | 22 | 14 | 0 | | | |
| ,, Printing and Stationery | 20 | 15 | 6 | 61 | 15 | 10 |
| ,, Printing Proceedings | 17 | 8 | 6 | | | |
| ,, Memoirs—Printing, Engraving, &c. | 24 | 13 | 9 | | | |
| ,, Memoirs purchased | 0 | 10 | 0 | 42 | 12 | 3 |
| ,, Library—Books, &c. | 33 | 5 | 7 | | | |
| ,, ,, Binding—Special Fund | 46 | 11 | 0 | 79 | 16 | 7 |
| ,, Keeper of the Rooms for attendance on Sections | 6 | 6 | 0 | | | |
| ,, Photographic Society for attendance | 3 | 5 | 0 | 9 | 11 | 0 |
| ,, Solicitors for Registration of Rules | | | | 3 | 2 | 10 |
| Total Disbursement | | | | 354 | 7 | 0 |
| April 1st, 1863.—By Balance in the Bank of Heywood Brothers | 314 | 8 | 8 | | | |
| ,, By ditto in Treasurer's hands | 6 | 9 | 11 | 320 | 18 | 7 |
| | | | | £675 | 5 | 7 |

Examined and found correct, 10th April, 1863.

ROBERT WORTHINGTON, } AUDITORS.
G. V. VERNON, }
HENRY M. ORMEROD, TREASURER.

The following gentlemen were elected Officers of the Society for the ensuing year:—

### President.

**EDWARD WILLIAM BINNEY, F.R.S., F.G.S.**

### Vice-Presidents.

**JAMES PRESCOTT JOULE, LL.D., F.R.S., F.C.S., &c.**
**ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.**
**JOSEPH CHESBOROUGH DYER.**
**EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.**

### Secretaries.

**HENRY ENFIELD ROSCOE, B.A., PH.D., F.C.S.**
**JOSEPH BAXENDELL, F.R.A.S.**

### Treasurer.

**ROBERT WORTHINGTON, F.R.A.S.**

### Librarian.

**CHARLES FREDRIK EKMAN.**

### Of the Council.

**REV. WILLIAM GASKELL, M.A.**
**FREDERICK CRACE CALVERT, PH.D., F.R.S., &c.**
**PETER SPENCE, F.C.S.**
**GEORGE MOSLEY.**
**ALFRED FRYER.**
**GEORGE VENABLES VERNON, F.R.A.S.**

## MICROSCOPICAL SECTION.

April 20th, 1863.

Professor **WILLIAMSON**, F.R.S., President of the Section, in the Chair.

Mr. Charles O'Neill, F.C.S., and Mr. John Shae Perring, M.Inst.C.E., were elected Members of the Section.

Mr. John Slagg, jun., and Mr. H. A. Hurst, were elected auditors of the Treasurer's accounts.

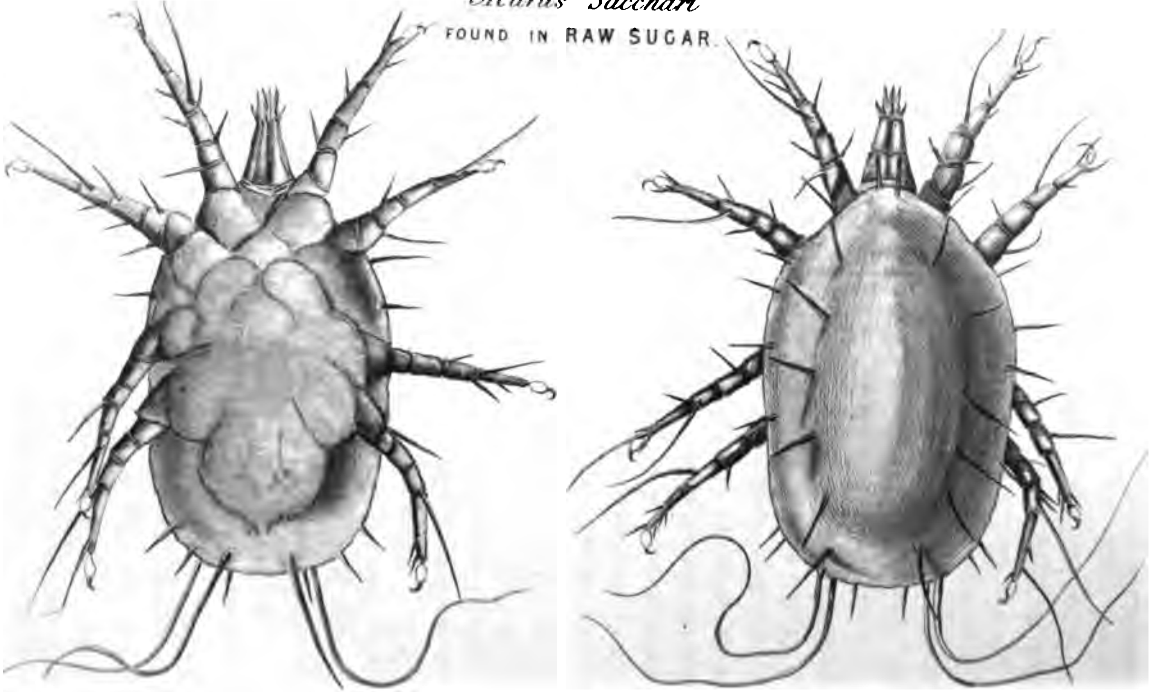
Mr. **ALFRED FRYER** presented for distribution amongst the members a number of impressions of an engraving of the Acarus sacchari found in raw grocery sugar, from Mauritius.

Mr. **BROTHERS** stated that he had made some observations upon the circulation in plants, and he found that a degree of heat which would cause free circulation in Vallisneria, entirely destroyed it in Chara vulgata. Mr. Brothers also described the appearances presented by the cilia of Melicerta ringens, which he had the unusual opportunity of observing whilst the animal was outside its case in a dying state. As the motion of the cilia gradually became fitful and then ceased, it was apparent that the cilia of the inner row are much longer than those of the outer row, over which the former appear to bend and to brush off whatever may be adhering to them into the channel between the two rows. Thus are produced the wavy lines and apparent onward progression of the cilia, which render this, under suitable illumination, so brilliant and interesting a microscopical object.

# SUGAR INSECT,

"*Acarus Sacchari*"

FOUND IN RAW SUGAR.



DRAWN FROM LIFE FROM INSECTS FOUND IN GROCERY MAURITIUS SUGAR.

*By Smith, Beck & Beck, Microscopists London.*

Photographed & Engraved by Maclure Macdonald & Macgregor

Mr. **Charles O'Neill**, F.C.S., made a communication "Upon the Appearances of Cotton Fibre during Solution and Disintegration." These experiments referred to the application of Schweizer's solvent. Two strengths were used; the weaker contained oxide of copper, equal to 4·3 grs. metal per 1,000, and 47 grs. dry ammonia; the stronger contained 15·4 grs. metal and 77 grs. dry ammonia per 1,000. The latter is about the most concentrated solution which can be made. Referring to the researches of Payen, Fresny, Peligot, Schlossberger, and others who have employed this solvent, the author said the only experimenter who seemed to have worked in the same direction with himself, and that apparently only to a small extent, was Dr. Cramer, whose paper he had only been able to see in a translation appended as a note to a memoir of M. Payen in *Comptes Rendus*, p. 319, vol. xlviii.

Mr. O'Neill considers that cotton exhibits, under the action of this solvent, (1) an external membrane distinct from the true cell wall or cellulose matter; (2), spiral vessels situated either in or outside the external membrane; (3), the true cell wall or cellulose; and (4), an inner medullary matter. The external membrane is insoluble in the solvent, and may be obtained in short hollow cylinders by first acting upon the cotton with the dilute solvent so as to gradually remove the cellulose, and then dissolve all soluble matters by the strong solvent. If the strong solution is first applied, the extraordinary dilation of the cellulose bursts the external membrane, and reduces it to such a state of tenuity that it is invisible. This membrane is very elastic, appears to be quite impermeable to the solvent, and when free from fissures protects the enclosed matter from its action. It is not seen in cotton which has been submitted to the action of alkaline acids and bleaching powder, being either chemically altered, or, what is most probable, entirely removed.

The spiral vessels are unmistakeably apparent, running round the fibre in more or less close spirals, sometimes single, sometimes double and parallel, and at other times double and in opposite directions, or again seemingly wound close and tight round the cylinder. They are well seen in the spherical swellings or beads, but are prominent at the points of strangulations of long ovals formed when the ends of the fibres are held tightly. They collect in a close mass, forming a ligature, and are frequently ruptured, the ends projecting from the side of the fibre.

The cellulose is enormously dilated by the weaker solvent, and expands the external membrane into beautiful beads, which are doubtless the result of the spiral vessels acting as ligatures at the points of strangulation; at the open end of a fibre it can be seen oozing out as a mucilaginous substance. The stronger solution bursts the beads, or dissolves all the cellulose into a homogeneous mass, amidst which the empty cuticular membrane and the spiral vessels remain nearly unacted upon.

The substance called medullary matter is seen occupying the axes of the fibres; it is nearly insoluble in the solvents. It may be well seen projecting from the open end of a fibre where the cellulose is exuding, and often remains *in situ* when the fibre has quite disappeared. It has many appearances of being a distinct body, but the author in some cases thought it might be only the thickened or modified inner cell wall; in others it looked like a shrunk membrane, probably the dried up primordial utricle. It is generally absent or indistinct in old cotton, or cotton which has been submitted to bleaching agents.

Mr. O'Neill intends to submit further details when his investigations are more advanced.

Mr. **Hepworth** stated that he had observed spiral markings in Sea Island Cotton, not subjected to Chemical Action, and that he had calculated there would be about 50,000 spirals to an inch of fibre.

# PROCEEDINGS
OF
# THE LITERARY AND PHILOSOPHICAL SOCIETY.

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SESSION 1863—1864.

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## PHYSICAL AND MATHEMATICAL SECTION.

April 30th, 1863.

JOSEPH BAXENDELL, F.R.A.S., President of the Section, in the Chair.

A Paper was read by Mr. THOMAS CARRICK, "On the Wave of High Water, with Hints towards a New Theory of the Tides."

The Author, starting from a new hypothesis on the relations of terrestrial matter to cosmical force, has arrived at the conclusion that the tidal motions of ocean surfaces are caused by a differential action of force centering on land areas. The nature of this hypothesis, and its relation to those motions, were briefly illustrated from the point of view of an assumed nebulous origin of the solar system. The Author nevertheless declined to endorse the received "nebular hypothesis" as a genetic theory, and adopted its ideas and phraseology in his Paper solely from considerations of brevity in this incidental portion of his subject.

Assuming the existence of a diffused nebula, composed of ultimate atoms of matter, each having a normal rotation on a fixed axis in a uniform direction, and with simple forces of attraction and repulsion arising thereout, then, from causes arising out of diverse molecular groupings of these atoms and their poles, the nebulous matter in condensing upon a centre might take up three successive states, constituting the normal types of the solid, liquid, and gaseous states of terrestrial bodies,—the solid matter forming a spherical nucleus, everywhere covered with a concentric layer of fluid, and this overlaid with a gaseous envelope; these varying layers of matter being in stable equilibrium at the respective surfaces of contact. The force exerted upon such a sphere by another of like origin would therefore act by and through the intermediation of these three states,—each successive stage of condensation, alike with the residual uncondensed nebulous matter of space, thus forming an essential link in the chain of gravitative action. But so soon as any portions of the solid nucleus emerge above the surface of the fluid covering, into abnormal contact with the gaseous envelope, a differential action of enormous magnitude, centering upon these upheaved land areas, would be at once originated, the first measure of which would be the cosmical value of the latent forces by which the fluid state of matter was constituted an essential intermediate link between the solid and gaseous states. In the view of the Author, terrestrial matter in all its phases is now related to space, and to bodies in space, in a manner analogous to that which might have resulted from such a hypothetical origin. Not only does this matter exist in the three leading states of earth, water, and air, but each of the simpler forms of inorganic matter can, under given conditions, successively assume the solid, liquid, or gaseous state without undergoing any chemical change. This universal threefold relation of terrestrial matter points strongly towards the simple hypothesis, that the causal laws which now regulate these interchanges of state are the reflex of fundamental laws underlying the entire constitution of matter in the solar system.

Passing over the possible relation of the first land-upheaval to the early changes recorded by geology, and to the formation of heterogeneous solids, liquids, and gases, the differential force arising therefrom would be the initiating cause in the formation of the envelope of comminuted water or vapour which now encircles the whole globe. This vapour-ocean constitutes an intermediate state of matter in unstable equilibrium with other states at all surfaces of contact. By interactions arising thereout, the simple static conditions of force existing prior to land-upheaval are now, in the view of the Author, partly replaced by more complex phases of force; and thus light, heat, electricity, and magnetism, which are expressions of these complex phases, have their root in local reactions between unstable states of terrestrial matter at surfaces of abnormal contact when under the tension of cosmical force—just as all these "imponderable elements" are evoked in the voltaic battery by surface reaction of dissimilar solids and liquids in presence of atmospheric tension. In short, the ceaseless molecular changes and local motions of terrestrial matter would, on this hypothesis, be mainly referred to the differential action arising out of land-upheaval.

Recurring to the "wave of high water" which formed the special subject of the Paper, another phase of the present residual of that differential action would give rise to the tidal motions of ocean surfaces, the perturbative action centering on land areas, and attaining a maximum value on the shores of those areas. By discussing the hours of high water at full and change for the principal places of the globe, given in the Admiralty Tide Tables for 1863 (the data being first reduced to Greenwich mean time), the author arrived at the following law of the progression of the wave of high water:—

*In all land areas in the northern hemisphere the wave of high water tends to revolve round the coast in the direction of the hands of a watch, and in like areas in the southern hemisphere against the hands of a watch.*

Theoretically, this law should hold good in proportion as land areas approximate to the circular form, with wide uninterrupted ocean spaces all round. In a perfectly circular area of this kind, the differential action would have points of maximum and minimum effect on opposite shores at every instant,—these together forming a nodal line, both ends of which would move simultaneously round the coast as the moon passed across the heavens, the wave of high water being everywhere the instantaneous expression of the differential force at its nodal point of maximum action.

By enclosing the continents and land areas which approach nearest to the prescribed conditions within one or more circles intersecting the salient parts of the coast, the Author showed that whenever any systematic progression of the hour of high water could be distinctly traced, that progression is almost invariably in the required direction. Owing, however, to the irregular shape of all existing land areas, to the impossibility of including some of these in a single circle approximating to the coast line, and to the way in which some large areas are massed upon others with little or no intervening ocean spaces, many instances of anomalous results are found; and yet, when rightly considered in relation to disturbing causes, even these tend indirectly to confirm the method of grouping the data of tidal hours in relation to land areas as causal centres.

A Paper was read "On the number of Days on which Rain falls annually in London, from observations made during the 56 years, 1807—1862." By G. V. **Vernon**, F.R.A.S., M.B.M.S.

This paper has been compiled to meet, to some extent, inquiries which have from time to time been made to me by medical men and others as to the number of days on which rain falls annually at any station. Of course, the remarks only apply to London and its immediate neighbourhood.

*Howard's Climate of London*, has been used for the years 1807 to 1831, *Philosophical Transactions* for the years 1832 to 1840, and the *Greenwich Observations* for the years 1841 to 1862. During the entire period of 56 years no month occurred in which rain did not fall.

The minimum number of days occurred in 1832, the cholera year, and 1834; the number of days being 86 and and 82 respectively. The maximum number occurred in 1848, the number being 223 days.

The mean monthly values are as follow:—

| Month. | Mean number of days on which rain fell. | Month. | Mean number of days on which rain fell. |
|---|---|---|---|
| January ...... | 13·4 | July ............ | 13·0 |
| February .... | 12·4 | August .......... | 12·9 |
| March ........ | 11·9 | September ...... | 12·7 |
| April ........ | 12·7 | October.......... | 14·4 |
| May ........ | 12·7 | November......... | 13·6 |
| June ........ | 12·0 | December ......... | 13·6 |
| | | Year........ | 155·3 |

Taking the quarterly values, we find that rain falls on the greatest number of days in autumn, and the least in spring.

Taking the means of five yearly periods, there appears to be a kind of periodicity in the number of days on which rain falls, having a maximum in 1813 to 1817, and a minimum in 1843 to 1847.

A Paper was read "On the Rainfall at Oldham during the years 1836-62," by **JOHN HEAP**, Esq., with Remarks by **G. V. VERNON**, F.R.A.S., M.B.M.S.

From 1836 to 1852 a twelve-inch circular gauge was used, 24 feet from the ground. From 1853 to 1857 the height was only 11 feet above the ground. In 1861 and 1862 the fall was determined by weight, whereas in previous years a float was used.

The mean fall for the first seventeen years, 24 feet above the ground, was 32·468 inches. The mean fall for 1853-57 was 30·802 inches, 11 feet above the ground. The mean fall for 1858 to 1862 was 38·069 inches. The period 1853 to 1862 combined gives 34·432 inches for the fall 11 feet above the ground. Owing to the elevation of the gauge during the first seventeen years, the rainfall at Oldham appears to have been greatly below that at Manchester, whereas it ought, from the locality, to be more.

From careful comparisons with the rainfall at Manchester I find 39·752 inches as the probable rainfall at Oldham for the twenty-seven years, 11 feet from the ground.

The mean monthly fall for each period was as follows:—

| MONTH. | 1836—1852. | 1853—1857. | 1858—1862. | 1853—1862. |
|---|---|---|---|---|
| | Inches. | Inches. | Inches. | Inches. |
| January .... | 2·195 | 1·955 | 2·537 | 2·246 |
| February .... | 1·825 | 2·130 | 2·029 | 2·079 |
| March ...... | 2·090 | 1·509 | 3·481 | 2·495 |
| April ...... | 2·147 | 1·740 | 2·230 | 1·985 |
| May ...... .. | 2·328 | 2·305 | 2·577 | 2·441 |
| June........ | 3·055 | 2·379 | 3·898 | 3·138 |
| July ........ | 3·490 | 3·476 | 3·567 | 3·521 |
| August...... | 3·493 | 3·358 | 3·471 | 3·414 |
| September .. | 2·880 | 1·912 | 4·622 | 3·267 |
| October .... | 3·230 | 4·637 | 4·020 | 4·328 |
| November .. | 3·399 | 2·075 | 2·222 | 2·148 |
| December .. | 2·336 | 3·326 | 3·415 | 3·370 |
| Sums .... | 32·468 | 30·802 | 38·069 | 34·432 |
| Corresponding Periods at Manchester. | 36·859 | 31·371 | 33·757 | 32·564 |

Mr. Joseph Casartelli exhibited a new Registering Barometer. The tube in this instrument is Gay Lussac's, or the syphon form, and is enclosed in a mahogany case, in which is an eight days clock. The arrangement is such that at the completion of every hour the hammer of the clock strikes upon a portion of the apparatus, which impels the index point on to the drum and pierces a hole in the diagram; thus forming an hourly record of the height of the barometer from week to week.

Professor **Clifton** exhibited an instrument devised by him for observing the phenomena of Conical Refraction, both internal and external.

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The following letter was received on May 12th, 1863 :—

## HISTORY OF STEAM NAVIGATION.

To the Secretary of the Literary and Philosophical Society of Manchester.

Sir,

(1.) I regret that my having been acquainted with the abstract only of Mr. Dyer's paper, and not with the entire paper, should have led me to suppose erroneously that Mr. Dyer had overlooked the claims of Henry Bell, and also that he had mentioned Symington's steamboat of 1801 more slightly than he really had done.

(2.) With regard to the bad pecuniary result of Bell's undertaking, I think it is to be ascribed to competition rather than to want of encouragement. In little more than a year after the "Comet" began to ply, a rival steamer of greater size and power was started on the Clyde, and was soon followed by others.

(3.) In supporting the opinion of Mr. Woodcroft, that the title of the "first practical steamboat" is due to that vessel in which the double-acting cranked steam engine—in short, Watt's rotative engine—was first applied to drive the propeller, I proceed on the principle, that to constitute a "practical" machine, that machine must be capable, not merely of working well during a series of experiments, but of continuing to work well for years, with ordinary care in its management and repairs.

(4.) Such certainly never was, and never could have been the case, with any steamboat in which the wheels were made to turn by means of chains and rachet-work:—a sort of mechanism which may answer its purpose during an experiment, but which must rapidly wear itself out by shocks and rattling. Such an engine is not a "practical steam engine;" and a vessel driven by it is not a "practical steamboat." Hence the importance which I am disposed to ascribe to the first actual use of a permanently efficient rotative steam engine to drive a vessel.

(5.) It may be true that as an original inventor, Symington ought to be ranked below his predecessors; because his steamboat of 1801 was only a new combination of parts which had previously been invented separately by others:—the paddle-wheel, by some unknown mechanic of remote antiquity; the application of steam to drive vessels, by a series of inventors, comprising Papin, Hulls, D. Bernouilli, Jouffroy, Miller, and Taylor; and the rotative steam engine by Watt: still the merit of having first used a "practical steam engine" to drive a vessel is due to Symington.

(6.) Considering the intimate personal knowledge possessed by Mr. Dyer of the early history of steam navigation, and the eminent services which he long ago did to the community by his efforts in promoting its introduction into Britain, it is very satisfactory to me to find that the main difference between us relates only to the *degree* of importance to be attached to a particular step in the history of that art.

I am, Sir,
Your most obedient Servant,
W. J. MACQUORN RANKINE.

GLASGOW, 11TH MAY, 1863.

A Paper was communicated on July 1st, entitled, "The Complete Theory of Groups, being the Solution of the Mathematical Prize Question of the French Academy for 1860. By the Rev. THOS. P. KIRKMAN, M.A., F.R.S., and Honorary Member of the Literary and Philosophical Societies of Manchester and Liverpool."

I have the honour to present to this Society a complete solution of the problem proposed early in 1858 by the French Academy, as the subject for their *Grand Prix des Mathématiques*, for June, 1860.

After the first contest of that date, at which something more was accomplished than what is common at a first competition on a question of the same difficulty, by the three candidates, of whom it was my fortune to be one, it was withdrawn from further competition, without any award of the prize, with an unusual suddenness, which created some remark in the world of science. The reason assigned for this abrupt dismissal of a question, on which the *savans* of the imperial Institute had solemnly invited their scientific contemporaries of all nations to undertake a two-years' toil, was this, that while the Academy briefly thanked and commended all the competitors for contributing what was new and really important, on a theory whose complete discussion it had not hoped to obtain, that learned body was unanimously agreed, that its intentions about the unknown subject were not fulfilled. This is placed on record, without the faintest definition of those intentions, or publication of the facts of the contest; the investigators receiving no definite credit, even in brief summary, for their numerous original results. *Vide* the curt report of the Referees, in the copious and weekly *Comptes Rendus de l'Académie*, Mars, 1861.

It is due to the question and to myself to state, that a letter was written by my friend Mr. Ekman, on my part, to the

Academy, on the 25th of April last, offering to them this solution of their prize-question, on condition that they would award to me some kind of *extra* medal, after satisfying themselves that my work is what it pretends to be, the whole of that theory of which, in their proposals of 1858, they desired to obtain only some notable extension.

He received a formal reply, dated May 11, informing him that the matter was referred to the Committee of the competition of 1860: he has since heard nothing more.

The prize-question for 1860 was the following:—

"Quels peuvent être les nombres de valeurs des fonctions bien définies qui contiennent un nombre donné de lettres, et comment peut-on former les fonctions pour lesquelles il existe un nombre donné de valeurs?"

A complete answer to this question, which is more than the Academy required in its proposals in 1858, must supply a clear and direct method of forming every possible transitive $r$-valued function of $n$ letters not divisible by, nor containing, a symmetric function, with an enumeration of all its equivalent functions, that is, functions of like form and degree, of which no one is a value of another, and also with a demonstration that no other such $r$-valued functions of $n$ letters are possible. Such a complete answer is given in the Memoir of which this Paper is the abstract.

To expect here definite results in general formulæ for every value of $n$, is about as reasonable a thing as to ask for a complete exposition of the theory of numbers in general formulæ. I have the honour to present the results demanded, both positive and negative, for values of $n$ below $n=11$, and to shew that all further results can be obtained with certainty by my method up to any value of $n$. It is a great satisfaction to me to be able to say, that the positive results, *i.e.*, all the groups and functions thus far found, fall under the definite and general though defectively arranged theorems, of the treatise which I sent to Paris, and of the supplement, which have been together printed in the Memoirs of this Society; while there is a considerable portion of the positive, besides nearly all the negative results, *the more difficult to establish*, which are comprehended under no theorems elsewhere to be found, whether definitely expressed, or indefinitely by imperfect description of limiting cases. All that is necessary to be added to that treatise, including the supplement, for the complete mastery of the problem, is briefly given in the following nine articles. The tables which next follow give account of all transitive groups made with fewer than eleven elements.

I. Every transitive group T, made with $n$ elements, is of the order $nk$, and contains a group K of the order $k$, made with $n-y-1$ elements, of which the $n$th, or final, is not one, which may be a transitive or intransitive group K, and which we call the *base* of the group T. Hence the problem is completely solved, if, when any such group K of the order $k$ is given, we can construct every transitive group T made with $n$ elements, which contains K, with an accurate description of T, and with the enumeration of its equivalents, or prove that no such group T exists. We thus establish all the required results, both positive and negative.

II. The group K made with $n-y-1$ elements, is given at first sufficiently by its *title*, and by the number $Q_K$ of its equivalents, without its actual construction.

*Definition.* The title of any group K exhibits the number of its substitutions of every form, *i.e.*, having any circular factors. *e.g.* Here are the titles of three transitive groups K of the order $k = 24$, with the numbers Q of their equivalents:—

$$24 = 1 + 6_4 + 6_{21^2} + 8_{31} + 3_{2^2}, \qquad Q = 1;$$

$$24 - 1 + 8_{62} + 6_{4^2} + 8_{3^2 1^2} + 1_{2^4}, \qquad Q = 840;$$

$$24 = 1 + 8_{62} + 8_{3^2 1^2} + 7_{2^4}, \qquad Q = 840.$$

The first is made with four elements, the other two with eight. The term $6_4$ shews that there are six substitutions having each a circular factor of the fourth order. The term $6_{21^2}$ shews six substitutions, having each one circular factor of the second order, and two elements undisturbed; the term $8_{3^2 1^2}$ exhibits eight substitutions, each having two circular factors of the third order, and two elements undisturbed. Exponents in the *signature* (= sub-index) are merely coefficients; $3^2 1^2$ meaning the sum of 3311, and $2^4$ meaning that of 2222.

The *clear terms* of a title (clear of units) as $6_4$, $8_{62}$, are those in which no units are read; these have no element undisturbed: the *unclear terms*, as $6_{21^2}$, $8_{31}$, shew units, that is, undisturbed elements, in the signature.

III. From the title of any group K, transitive or intransitive, made with $n - y - 1$ elements, and of the order $k$, the *unclear* terms of the title of every transitive group T, made with $n$ letters, and of the order $nk$, which contains K, are all written at once by the general theorem following.

*Theorem.* The number of substitutions of a given form and of any order $p$ in the group K, is always the term $(aR_p)_i$ of the title, $R_p$ being the number of integers, unity included, which are less than $p$ and prime to it, and $i$ being the signature, containing $1^m$, or shewing $m$ ($\geqq 0$) undisturbed letters: the whole unclear term $J_j$ of the title of T is determined by $(aR_p)_i$, and is always $\left(R_p \frac{an}{m+y+1}\right)_j$ where the signature $j$ differs from $i$ only by having $y + 1$ more units, or $1^{m+y+1}$ for $1^m$.

If the number $\frac{na}{m+y+1}$ is not an integer thus obtained from every term of the title of K, no transitive group T made with $n$ elements exists containing the base K.

IV. The unclear terms of the title of T being thus all found, the clear terms are next required. First, we know, by the elements of this theory, that no substitution can be in T, unless all its powers are T. The unclear powers of all substitutions in T are already written; hence we know by inspection what clear substitutions having unclear powers are possible. This always reduces the admissible clear signatures to a small number. Let $L_1$ be the sum of the unclear terms of T. Then $nk - L_1 = L_0$ is the sum of the clear terms, and we have

$$L_0 = A_a + B_b + C_c + \, . \, .$$

$a, b, c$ . . being the admissible clear signatures, and ABC . . being sought numbers.

V. Def. *A positive substitution or signature has an even number* ($\geqq 0$) *of even circular factors.* Thus, in the first of the three above written titles, the signatures of 1, $8_{31}$, and $3_{1^2}$ are positive. In the other two titles all are positive. *A signature or substitution not positive, is negative,* as in $6_{21^2}$, $6_4$.

A *mixed group* has as many positive as negative substitutions, and the former of themselves are always a group. A *positive group* has no negative substitutions.

Hence if $L_1$ is all positive and greater than $\frac{1}{2}nk$, we know that $L_0$ has only positive signatures, and all its terms have to be found. If $L_1$ is mixed, we know that T is mixed, and we have its positive half in our tables of titles. It may be transitive or intransitive, and is easily recognised by comparison of $L_1$; and we know that the negative half is the positive half multiplied by any negative substitution in $L_1$; *i.e.* the group T is completely given by construction, although we have not yet its title. We extract from the positive half in our tables the positive clear terms in $L_0$, and the negative terms of $L_0$ alone have to be found. In any case we have, ($M \geqq 0$),

$$L'_0 = L_0 - M = A_a + B_b + C_c + \, . \, .$$

where ABC . . . are sought numbers whose sum is known, and *a b c* .. are either all positive or all negative clear signatures.

VI. An important theorem is the following:—

*Theorem.*—If $\theta_a$ and $\phi_a$ be two substitutions of any order $p$ having the same signature $a$, in any group T, and if $\theta_a$ and $\phi_a$ are not one a power of the other, and have not a common power different from unity, T cannot contain (as $A_a$) fewer than $(2+R_p)R_p$ substitutions having the signature $a$, where $R_p$ has the value above given (III).

If $\theta_a$ and $\phi_a$ have a common power different from unity, that is, if they be roots of the same substitution $\lambda$, and not one a power of the other, the number A may be less than $(2+R_p)R_p$. If $\lambda$ is unclear, or clear and positive in a mixed group T, we have before us the number of substitutions in T of the form of $\lambda$. The number of $r$th roots that any substitution $\lambda$ can have is easily deduced from the theorems of my published Memoir. Thus we have a conditioned limit of A. And, by the inferior groups already registered, which contain more than one $r$th root of $\lambda$, we know in general, by the presence or absence of certain other substitutions, whether our group T can or cannot contain more than one $r$th root of $\lambda$.

VII. The above considerations in most cases enable us to determine the values of ABC . . . . If obscurity remains, we must have recourse to the consideration of the numbers $Q_K$ and $Q_T$, of the equivalents of K and T. $Q_K$ is always given, and we easily prove that

$$Q_T = hQ_K,$$

and also that

$$hs = \frac{1\cdot 2\cdot 3 \ldots\ldots n}{kn\cdot Q_K},$$

*i.e.*, that $h$ is some divisor of a given number. When the group T is mixed, we know the value of $h$ exactly, by virtue of a remark in Art. V. We know also, by the theorems of my printed Memoir, the entire numbers $V_a$ $V_b$ $V_c$ $V_g$ ....

of substitutions having any signatures $a\ b\ c\ g$, whether clear or unclear; and if $\rho_a\ \rho_b\ \rho_c\ \rho_g$ .. be the number of times that $V_a\ V_b\ V_c\ V_g$ .... are repeated in all the $Q_T$ equivalents of T, we readily prove, as in the 86th article of that Memoir, that

$$Q_T = hQ_K = \frac{V_a\rho_a}{A_a} = \frac{V_b\rho_b}{B_b} = \frac{V_c\rho_c}{C_c} = \frac{V_g\rho_g}{G_g} \ldots ;$$

whence we obtain

$$hA_a = F_a\rho_a,\ hB_a = F_b\rho_b,\ hC_c = F_c\rho_c,\ \&c.,$$

$F_aF_bF_c$ being known numbers. Hence

$$hL'_0 = F_a\rho_a + F_b\rho_b + F_c\rho_c + \ldots$$

where either $h$ is known, or the possible factors of $h$ are known. In every case yet considered in which T is impossible, its non-existence, if not exposed before, is proved by a simple absurdity, as that $h$ does and does not contain a certain factor $g$. If, after clearing the two members of the last equation of common factors, no such absurdity appears, we select values of $\rho_a\rho_b\rho_c$ consistent with the preceding considerations, thus determining admissible systems of values of ABC $\cdots$ to fulfil the condition

$$L'_0 = A_a + B_b + C_c + \ \ldots\ ;$$

and we complete with each system a title of a group T.

VIII. It is sometimes a simpler consideration than (VII.) to take account of the products of the substitutions of the second order in the title. If it be a real one, we can account for every such product $\alpha\beta$. These are either permutables, giving the product $\alpha\beta=\gamma$ of the second order, or else didymous factors of higher substitutions of the title. The number of substitutions permutable with $\alpha$ of the second order, the number of different sets of didymous factors that $\phi$ of any order can have, the number of substitutions $\phi$, $\psi$, $\chi$, of like or different orders, under which $\alpha$ can be written as a didymous factor—all these are readily deducible from the theorems of my published Memoir. A title is thus often easily shewn to be impossible, by the fact, that no account can be given of the products of its square roots of unity.

IX. The groups T, whose titles are thus consistently described, are now considered possible: their reality is proved, if we can construct them. This construction is *always indicated* by the group K and by the completed title. We have in general to multiply K by a root or roots of one of its lowest substitutions. These roots are readily found by the *tactical method of evolution,* on which I believe that I have had the honour to write all that has been written, and what is equally important, in this theory, with *involution.* Sometimes we have to multiply K by a substitution $\theta$, which has for one didymous factor one of the square roots of unity in K, and for the other a permutable with another in K. This $\theta$ is always most easily found by a tactical method. In every case in which a possible group is indicated by a completed title, I have thus far found that the construction follows by the most simple and rapid *tactical methods.* The construction of K is of course tactically known, as is that of every group transitive or intransitive in our register. And we can always give a clear account of the equivalent or non-equivalent groups T, which are built on the same base K.

The determination of the titles of groups T is assisted throughout by the general theorems of my Memoir, on grouped, woven, and woven-grouped groups, of which we know the majority for all values of *n,* by well-defined constructions, as well as the enumeration of their equivalents.

The greater number of intransitive groups are found to be inadmissible as bases K of transitive groups T, for all values of *n,* by simple rules deduced from (III). Of those not thus excluded, the bases K, which form part of no T made with *n* elements, are rapidly disposed of, chiefly by the theorem of (III). And the demonstrations, whether affirmative or negative, are always very briefly drawn from the preceding considerations and from what has before been written.

The most remarkable thing in this method is, *that we need no algebraical substitutions:* we are never conscious of their existence. It turns out, that the ingenious and learned efforts of the French and Italian mathematicians to conquer this theory by algebra, with its formidable army of congruences and imaginaries, have been from the beginning one brilliant error. The problem is tactical, and the solution is tactical. I have from the first suspected this, from the fact, that so very few substitutions have been shewn to be expressible by algebraic formulæ, without a crowd of imaginaries, and that, with these few, operation and computation are possible only at the cost of a most irksome complexity. My tactical methods handle at once the numerals (taken for the elements) which this vast algebra vainly attempts to symbolise, and all equivalent substitutions and groups, whatever the number of elements may be, are alike under control. And, *what is the most important thing*, the groups so constructed are exactly the many-valued functions desired. Having your group on the page, you have, in three more seconds, in its most simple and useful form for comparison or for all computations, one of the explicit functions required. You write a single line of exponents over unity, attributing the same exponent to every element in the same vertical row, and your function is before you on the paper. If you choose your exponents properly, you have the *simplest possible function which has the required number of values.* All this I have already shewn, in the tenth section of that Memoir which was consigned to oblivion at Paris in 1860; and it has been nowhere else shewn.

I believe that no transitive groups exist for values of $n \angle 11$, which are not found in the following tables. If any have escaped me, it is the fault, not of my method, but of my carelessness; and I hope that allowance will be made for the extent and difficulty of the negative which has to be proved. The tactical construction of all these groups is given in the Memoir here presented.

Titles of transitive groups made with fewer than eleven elements, with the numbers of their equivalents. (Vide Art. II.)

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$3\cdot1=1+2_{3}$, $Q=1$; $3\cdot2=1+2_{3}+3_{21}$, $Q=1$.

$4\cdot1=1+2_{4}+1_{2^2}$, $Q=3$; $4\cdot2=1+2_{4}+3_{2^2}+2_{21^2}$, $Q=3$;

$4\cdot3=1+8_{31}+3_{2^2}$, $Q=1$; $4\cdot6=1+8_{31}+3_{2^2}+6_{4}+6_{21^2}$, $Q=1$.

$5\cdot1=1+4_{5}$, $Q=6$; $5\cdot2=1+4_{5}+5_{2^21}+10_{41}$, $Q=6$;

$5\cdot4=1+4_{5}+5_{2^21}+10_{41}$, $Q=6$;

$5\cdot4\cdot3=1+24_{5}+20_{31^2}+15_{2^21}$, $Q=1$;

$5\cdot4\cdot3\cdot2\cdot1=1+24_{5}+20_{31^2}+15_{2^21}+30_{41}+10_{21^3}+20_{32}$ $Q=1$.

$6\cdot1=1+2_{3^2}+1_{2^3}+2_{6}$, $Q=60$;

$6\cdot1=1+2_{3^2}+3_{2^3}$, $Q=20$;

$6.2=1+2_{3^2}+3_{2^21^2}+4_{2^3}+2_{6}$, $Q=60$;

$6\cdot3=1+4_{3^2}+4_{31^3}+3_{2^3}+6_{6}$, $Q=20$;

$6\cdot4=1+9_{2^21^2}+6_{42}+8_{3^2}$, $Q=15$;

$6\cdot4=1+8_{3^2}+3_{2^21^2}+8_{6}+3_{21^3}+1_{2^3}$, $Q=15$;

$6\cdot3\cdot2=1+18_{42}+4_{3^2}+4_{31^3}+9_{2^21^2}$, $Q=15$;

$6\cdot3\cdot2=1+4_{3^2}+9_{2^21^2}+4_{31^3}+6_{2^3}+12_{6}$, $Q=10$;

$6\cdot4\cdot2=1+8_{3^2}+6_{42}+9_{2^21^2}+13_{2^3}+3_{21^3}+8_{6}$, $Q=15$;

$6\cdot5\cdot2=1+20_{3^2}+24_{51}+15_{2^21^2}$, $Q=6$;

$6\cdot4\cdot3=1+4_{3^2}+18_{42}+4_{31^3}+9_{2^21^2}+12_{321}+6_{21^4}+12_{6}$
$+6_{2^3}$, $Q=10$;

$6{\cdot}5{\cdot}4 = 1 + 20_{3^2} + 24_{51} + 15_{2^21^2} + 30_{41^2} + 10_{2^3} + 20_{6},$ $Q=6;$

$6{\cdot}5{\cdot}4{\cdot}3 = 1 + 144_{51} + 45_{2^21^2} + 90_{42} + 40_{3^2} + 40_{31^3},$ $Q=1;$

$6{\cdot}5{\cdot}4{\cdot}3{\cdot}2 = 1 + 144_{51} + 45_{2^21^2} + 90_{42} + 40_{3^2} + 40_{31^3} + 90_{41^2}$
$+ 120_{321} + 15_{2^3} + 15_{21^4} + 120_{6},$ $Q=1.$

$7{\cdot}1 = 1 + 6_{7},$ $Q=120;$ $7{\cdot}2 = 1 + 6_{7} + 7_{2^31},$ $Q=120;$

$7{\cdot}3 = 1 + 6_{7} + 14_{3^21},$ $Q=120;$

$7{\cdot}6 = 1 + 6_{7} + 14_{3^21} + 7_{2^31} + 14_{61},$ $Q=120;$

$7{\cdot}6{\cdot}4 = 1 + 48_{7} + 56_{3^21} + 42_{421} + 21_{2^21^3},$ $Q=30;$

$7{\cdot}6{\cdot}5{\cdot}4{\cdot}3 = 1 + 720_{7} + 504_{51^2} + 630_{421} + 70_{31^4} + 280_{3^21}$
$+ 105_{2^21^3} + 210_{32^2}.$ $Q=1;$

$7{\cdot}6{\cdot}5{\cdot}4{\cdot}3{\cdot}2 = 1 + 720_{7} + 504_{51^2} + 630_{421} + 70_{31^4} + 280_{3^21}$
$+ 105_{2^21^3} + 210_{32^2} + 840_{61} + 504_{52} + 420_{43} +$
$420_{321^2} + 105_{2^31} + 21_{21^5} + 210_{41^3},$ $Q=1.$

$8{\cdot}1 = 1 + 1 + 4_{8} + 2_{4^2} + 1_{2^4},$ $Q=1260;$

$8{\cdot}1 = 1 + 2_{44} + 5_{2^4},$ $Q=630;$

$8{\cdot}1 = 1 + 6_{4^2} + 1_{2^4},$ $Q=210;$

$8{\cdot}1 = 1 + 4_{4^2} + 3_{2^4},$ $Q=630;$

$8{\cdot}1 = 1 + 7_{2^4}$ $Q=30;$

$8{\cdot}2 = 1 + 8_{4^2} + 5_{2^4} + 2_{2^21^4},$ $Q=630;$

$8{\cdot}2 = 1 + 4_{4^2} + 2_{2^21^4} + 1_{2^4} + 8_{8},$ $Q=630;$

$8{\cdot}2 = 1 + 4_{4^2} + 9_{2^4} + 2_{2^21^4},$ $Q=315;$

$8{\cdot}2 = 1 + 2_{4^2} + 5_{2^4} + 4_{8} + 4_{2^31^2},$ $Q=1260;$

$8{\cdot}2 = 1 + 6_{4^2} + 1_{2^4} + 4_{8} + 4_{2^31^4},$ $Q=1260;$

$8{\cdot}3 = 1 + 8_{3^21^2} + 8_{62} + 6_{4^2} + 1_{2^4},$ $Q=840;$

$8{\cdot}3 = 1 + 8_{62} + 8_{3^21^2} + 7_{2^4},$ $Q=840;$

$8\cdot4 = 1 + 12_{4^2} + 9_{2^4} + 8_{421^2} + 2_{2^21^4},$ $Q = 315;$

$8\cdot4 = 1 + 8_{4^2} + 5_{2^4} + 2_{2^21^4} + 8_8 + 4_{2^31^2} + 4_{41^4}$ $Q = 630;$

$8\cdot4 = 1 + 20_{4^2} + 6_{2^21^4} + 5_{2^4},$ $Q = 630;$

$8\cdot4 = 1 + 12_{4^2} + 13_{2^4} + 6_{2^21^4},$ $Q = 105;$

$8\cdot6 = 1 + 8_{62} + 6_{4^2} + 8_{3^21^2} + 1_{2^4} + 12_8 + 12_{2^31^2},$ $Q = 840;$

$8\cdot6 = 1 + 8_{62} + 12_{4^2} + 8_{3^21^2} + 13_{2^4} + 6_{2^21^4},$ $Q = 840;$

$8\cdot8 = 1 + 12_{4^2} + 13_{2^4} + 6_{2^21^4} + 24_{42^2} + 4_{2^31^2} + 4_{21^6},$ $Q = 105;$

$8\cdot8 = 1 + 12_{4^2} + 6_{2^21^4} + 13_{2^4} + 12_{42^2} + 8_8 + 8_{2^31^2} + 4_{41^4},$ $Q = 630;$

$8\cdot8 = 1 + 20_{4^2} + 6_{2^21^4} + 5_{2^4} + 16_8 + 8_{42^2} + 4_{2^4} + 4_{21^6},$ $Q = 630;$

$8\cdot8 = 1 + 28_{4^2} + 17_{2^4} + 8_{421} + 10_{2^21^4},$ $Q = 315;$

$8\cdot7 = 1 + 48_{71} + 7_{2^4},$ $Q - 240;$

$8\cdot4\cdot3 = 1 + 12_{4^2} + 6_{2^21^4} + 13_{2^4} + 32_{3^21^2} + 32_{62}$ $Q = 105;$

$8\cdot4\cdot2\cdot2 = 1 + 28_{4^2} + 17_{2^4} + 8_{421^2} + 10_{2^21^4} + 16_8 + 28_{42^2}$
$+ 12_{2^31^2} + 4_{21^6} + 4_{41^4},$ $Q = 315;$

$8\cdot7\cdot3 = 1 + 48_{71} + 56_{3^21^2} + 7_{2^4} + 56_{62},$ $Q = 240;$

$8\cdot7\cdot3 = 1 + 48_{71} + 56_{3^21^2} + 42_{4^2} + 21_{2^4},$ $Q = 120;$

$8\cdot6\cdot4 = 1 + 12_{4^2} + 6_{2^21^4} + 13_{2^4} + 32_{3^21^2} + 32_{62} + 4_{21^6} +$
$4_{2^31^2} + 32_{3^22} + 32_{61^2} + 24_{42^2},$ $Q = 105;$

$8\cdot6\cdot4 = 1 + 60_{4^2} + 32_{62} + 32_{3^21^2} + 24_{421^2} + 25_{2^4} + 18_{2^21^4}$ $Q = 105;$

$8\cdot6\cdot3\cdot2 = 1 + 36_{4^2} + 21_{2^4} + 96_{62} + 64_{3^21^2} + 48_{32^21} + 16_{31^5}$
$+ 6_{2^21^4}$ , $Q = 35;$

$8\cdot7\cdot6 = 1 + 48_{71} + 56_{3^21^2} + 21_{2^4} + 42_{4^2} + 84_8 + 56_{61^2}$
$+ 28_{2^31^2}$ , $Q = 120;$

$8\cdot6\cdot4\cdot2 = 1 + 60_{4^2} + 32_{62} + 32_{3^21^2} + 24_{421^2} + 25_{2^4} + 18_{2^21^4}$
$+ 4_{21^6} + 32_{61^2} + 52_{2^31^2} + 32_{3^22} + 24_{42^2} + 48_8,$ $Q = 105;$

$8\cdot6\cdot4\cdot3 = 1 + 96_{62} + 216_{4^2} + 21_{2^4} + 48_{32^21} + 72_{421^2} +$
$64_{3^21^2} + 16_{31^5} + 42_{2^21^4},$ $Q = 35;$

$8\cdot6\cdot4\cdot3\cdot2 = 1+96_{62}+216_{4^2}+21_{2^4}+48_{32^21}+72_{421^2}+$

$64_{3^21^2}+16_{31^5}+42_{2^21^4}+144_{8}+96_{61^2}+108_{42^2}+$

$72_{431}+36_{2^31^2}+96_{321^3}+12_{41^4}+12_{21^6},$ Q=35;

$8\cdot7\cdot6\cdot4 = 1+384_{71}+224_{62}+252_{4^2}+49_{2^4}+224_{3^21^2}+$

$168_{421^2}+42_{2^21^4},$ Q=30;

$8\cdot7\cdot6\cdot5\cdot4\cdot3 = 1+5760_{71}+3360_{62}+1260_{4^2}+105_{2^4}+$

$1120_{3^21^2}+2520_{421^2}+210_{2^21^4}+2688_{53}+1344_{51^3}$

$+112_{31^5}+1680_{32^21},$ Q=1;

$8\cdot7\cdot6\cdot5\cdot4\cdot3\cdot2 = 8\cdot7\cdot6\cdot5\cdot4\cdot3+3360_{61^2}+1260_{42^2}+4032_{521}$

$+420_{41^4}+420_{2^31^2}+1120_{321^3}+1120_{3^22}+5040_{8}+$

$28_{21^6}+3660_{341},$ Q=1.

$9\cdot1 = 1+6_{9}+2_{3^3},$ Q=6720;

$9\cdot1 = 1+8_{3^3},$ Q=840;

$9\cdot2 = 1+8_{3^3}+9_{2^41},$ Q=840;

$9\cdot2 = 1+8_{3^3}+6_{63}+3_{2^31^3},$ Q=2520;

$9\cdot2 = 1+6_{9}+2_{3^3}+9_{2^41},$ Q=6720;

$9\cdot3 = 1+18_{9}+2_{3^3}+6_{3^21^3},$ Q=2240;

$9\cdot3 = 1+20_{3^3}+6_{3^21^3},$ Q=840;

$9\cdot4 = 1+8_{3^3}+9_{2^41}+12_{63}+6_{2^31^3},$ Q=10080;

$9\cdot4 = 1+8_{3^3}+18_{4^21}+9_{2^41},$ Q=2520;

$9\cdot6 = 1+18_{9}+2_{3^3}+6_{3^21^3}+9_{2^41}+18_{621},$ Q=2240;

$9 \cdot 6 = 1 + 18_{63} + 9_{2^3 1^3} + 6_{3^2 1^3} + 20_{3^3},$ $Q = 1680;$

$9 \cdot 8 = 1 + 8_{3^3} + 18_{4^2 1} + 9_{2^4 1} + 36_{81},$ $Q = 2520;$

$9 \cdot 8 = 1 + 54_{4^2 1} + 9_{2^4 1} + 8_{3^3},$ $Q = 2520;$

$9 \cdot 9 = 1 + 36_{9} + 26_{3^3} + 12_{3^2 1^3} + 6_{31^5},$ $Q = 1120;$

$9 \cdot 8 \cdot 2 = 1 + 36_{81} + 24_{63} + 54_{4^2 1} + 8_{3^3} + 9_{2^4 1} + 12_{2^3 1^3},$ $Q = 2520;$

$9 \cdot 6 \cdot 3 = 1 + 36_{9} + 26_{3^3} + 12_{3^2 1^3} + 6_{31^6} + 27_{2^4 1} + 54_{621}$ $Q = 1120;$

$9 \cdot 6 \cdot 3 = 1 + 36_{9} + 26_{3^3} + 12_{3^2 1^3} + 6_{31^6} + 18_{61^3} + 9_{2^3 1^3} +$

$54_{63},$ $Q = 1120;$

$9 \cdot 6 \cdot 3 \cdot 2 = 1 + 36_{9} + 26_{3^3} + 12_{3^2 1^3} + 6_{31^6} + 27_{2^4 1} + 54_{621}$

$+ 108_{63} + 36_{2^3 1^3} + 18_{61^3},$ $Q = 1120;$

$9 \cdot 6 \cdot 3 \cdot 2 = 1 + 144_{9} + 26_{3^3} + 54_{621} + 27_{2^4 1} + 12_{3^2 1^3} + 6_{31^6}$

$+ 54_{32^2 1^2},$ $Q = 280;$

$9 \cdot 6.4 = 1 + 56_{3^3} + 72_{621} + 54_{4^2 1} + 9_{2^4 1} + 24_{3^2 1^3},$ $Q = 840;$

$9 \cdot 8 \cdot 6 = 1 + 56_{3^3} + 72_{621} + 54_{4^2 1} + 9_{2^4 1} + 24_{3^2 1^3} + 108_{81}$

$+ 36_{2^3 1^3} 72_{63},$ $Q = 840;$

$9 \cdot 8 \cdot 7 = 1 + 168_{9} + 216_{71^2} + 56_{3^3} + 63_{2^4 1},$ $Q = 240;$

$9 \cdot 6 \cdot 4 \cdot 3 = 1 + 144_{9} + 26_{3^3} + 54_{621} + 27_{2^4 1} + 12_{3^2 1^3} + 6_{31^6}$

$+ 54_{32^2 1^2} + 216_{63} + 36_{3^2 21} + 27_{2^3 1^3} + 36_{321^4} +$

$9_{21^7},$ $Q = 280;$

$9 \cdot 6 \cdot 4 \cdot 3 = 1 + 144_{9} + 80_{3^3} + 108_{432} + 108_{621} + 54_{2^4 1} +$

$54_{32^2 1^2} + 12_{3^2 1^3} + 54_{421^3} + 27_{2^4 1} + 6_{31^6}$ $Q = 280;$

$9 \cdot 6 \cdot 4 \cdot 3 \cdot 2 = 1 + 144_{9} + 80_{3^3} + 108_{432} + 54_{2^4 1} + 54_{32^2 1^2} +$

$108_{621} + 12_{3^2 1^3} + 54_{421^3} + 27_{2^4 1} + 6_{31^6} + 288_{63} +$

$36_{32^3}+162_{42^21}+36_{3^221}+36_{61^3}+45_{2^31^3}+36_{321^4}$

$+9_{21^7},$ $Q=280;$

$9\cdot8\cdot7\cdot3=1+504_9+56_{3^3}+216_{71^2}+504_{621}+63_{2^41}+$

$168_{3^21^3},$ $Q=240;$

$9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3=1+40320_9+25920_{71^2}+30240_{621}+2240_{3^3}$

$+7560_{421^3}+3360_{3^21^3}+168_{31^6}+945_{2^41}+15120_{432}$

$+3024_{51^4}+9072_{52^2}+7560_{32^21^2}+24192_{531}+$

$11340_{4^21}+378_{2^21^5},$ $Q=1;$

$9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3\cdot2=9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3+10080_{61^3}+11340_{42^21}+$

$18144_{521^2}+756_{41^5}+1260_{2^31^3}+2520_{321^4}+10080_{3^221}$

$+45360_{81}+36_{21^7}+15120_{431^2}+20160_{63}+25920_{72}$

$+18144_{54}+2520_{2^33},$ $Q=1.$

$10\cdot1=1+4_{10}+4_{5^2}+1_{2^5},$ $Q=90720;$

$10\cdot1=1+4_{5^2}+5_{2^5},$ $Q=18144;$

$10\cdot2=1+4_{10}+4_{5^2}+6_{2^5}+5_{2^41^2},$ $Q=90720;$

$10\cdot4=1+4_{5^2}+10_{4^22}+10_{4^21^2}+5_{2^41^2}+6_{2^5}+4_{10},$ $Q=90720;$

$10\cdot5=1+16_{5^2}+20_{10}+5_{2^5}+8_{51^5},$ $Q=18144;$

$10\cdot6=1+24_{5^2}+20_{3^31}+15_{2^41^2},$ $Q=30240;$

$10\cdot8=1+64_{5^2}+10_{2^21^6}+5_{2^41^2},$ $Q=5670;$

$10\cdot6\cdot2=1+24_{5^2}+15_{2^41^2}+20_{3^31}+24_{10}+20_{631}+10_{2^31^4}$

$+6_{2^5},$ $Q=30240;$

$10\cdot6\cdot2=1+24_{55}+20_{3^21^4}+15_{2^41^2}+20_{62^2}+24_{10}+$
$16_{2^5},$ Q=15120;

$10\cdot8\cdot2=1+64_{5^2}+5_{2^41^2}+10_{2^21^6}+64_{10}+5_{21^8}+10_{2^31^4}$
$+1_{2^5},$ Q=5670;

$10\cdot8\cdot2=1+64_{55}+40_{42^3}+10_{2^21^6}+25_{2^41^2}+20_{4^21^2},$ Q=5670;

$10\cdot5\cdot4=1+16_{5^2}+40_{52^21}+25_{2^41^2}+8_{51^5}+10_{2^21^6}+50_{4^22}$
$+10_{2^5}+40_{10},$ Q=18144;

$10\cdot8\cdot3=1+24_{5^2}+20_{3^22^2}+20_{3^21^4}+15_{2^41^2}+10_{2^21^6}+30_{4^21^2}$
$+24_{10}+40_{62^2}+26_{2^5}+30_{4^22},$ Q=15120;

$10\cdot8\cdot4=1+20_{4^21^2}+25_{2^41^2}+10_{2^21^6}+64_{5^2}+40_{42^3}+40_{42^21^2}$
$+10_{2^31^4}+5_{21^8}+64_{10}+20_{4^22}+21_{2^5},$ Q=5670;

$10\cdot8\cdot4=1+64_{5^2}+80_{82}+40_{42^3}+100_{4^21^2}+25_{2^41^2}+10_{2^21^6},$
Q=5670;

$10\cdot8\cdot5=1+16_{5^2}+100_{4^21^2}+25_{2^41^2}+8_{51^5}+10_{2^21^6}+40_{52^21}$
$+100_{4^22}+80_{10}+20_{2^5},$ Q=4536;

$10\cdot8\cdot5=1+16_{5^2}+200_{82}+40_{52^21}+10_{2^21^6}+100_{4^21^2}+8_{51^5}$
$+25_{2^41^2},$ Q=4536;

$10\cdot8\cdot4\cdot2=1+80_{82}+64_{5^2}+40_{42^3}+100_{4^21^2}+25_{2^41^2}+10_{2^21^6}$
$+21_{2^5}+80_{81^2}+64_{10}+40_{42^21^2}+100_{4^22}+10_{2^31^4}+5_{21^8}$
Q=5670;

$10\cdot9\cdot8=1+180_{82}+144_{5^2}+270_{4^21^2}+45_{2^41^2}+80_{3^31},$ Q=2520;

$10\cdot8\cdot5\cdot2=1+16_{5^2}+200_{82}+40_{52^21}+10_{2^21^6}+100_{4^21^2}+8_{51^5}$
$+25_{2^41^2}+80_{10}+20_{2^5}+20_{41^6}+100_{42^21^2}+80_{541}+$
$100_{4^22},$ Q=4536;

$10\cdot8\cdot6\cdot2=1+384_{5^2}+80_{3^22^2}+120_{42^3}+80_{3^21^4}+10_{2^21^6}+$
$64_{4^21^2}+65_{2^41^2}+160_{621^2},$ $Q=945;$

$10\cdot9\cdot8\cdot2=1+180_{82}+144_{5^2}+270_{4^21^2}+45_{2^41^2}+80_{3^31}+$
$30_{2^31^4}+180_{81^2}=144_{10}+90_{4^22}+240_{631}+36_{2^5},$ $Q=2520;$

$10\cdot8\cdot6\cdot4=1+240_{82}+320_{64}+384_{5^2}+80_{3^22^2}+140_{42^3}+$
$300_{4^21^2}+160_{621^2}+80_{3^21^4}+60_{421^4}+125_{2^41^2}+$
$30_{2^21^6},$ $Q=945;$

$10\cdot8\cdot6\cdot4=1+384_{5^2}+120_{42^3}+80_{3^22^2}+60_{4^21^2}+65_{2^41^2}+$
$160_{621^2}+80_{3^21^4}+10_{2^21^6}+384_{10}+60_{34^22}+61_{2^5}$
$+160_{3^221^2}+120_{42^21^2}+10_{2^31^4}+160_{61^4}+5_{21^8},$ $Q=945;$

$10\cdot8\cdot6\cdot4\cdot2=1+240_{82}+320_{64}+384_{5^2}+80_{3^22^2}+140_{42^3}$
$+300_{4^21^2}+160_{621^2}+30_{2^21^6}+80_{3^21^4}+60_{421^4}+$
$125_{2^41^2}+384_{10}+320_{3^24}+240_{4^22}+160_{62^2}+$
$81_{2^5}+160_{3^221^2}+120_{42^21^2}+240_{81^2}+80_{61^4}+$
$130_{2^31^4}+5_{21^8},$ $Q=945;$

$10\cdot8\cdot6\cdot5\cdot3=1+576_{5^2}+720_{52^21}+960_{531^2}+225_{2^41^2}+$
$600_{32^21^3}+400_{3^21^4}+30_{2^21^6}+48_{51^5}+40_{31^7}+2880_{10}$
$+600_{62^2}+120_{2^5},$ $Q=126;$

$10\cdot8\cdot6\cdot5\cdot3\cdot2=1+3600_{82}+2400_{64}+1200_{42^3}+400_{3^22^2}$
$+576_{5^2}+720_{52^21}+1200_{4321}+960_{531^2}+$
$1000_{32^21^3}+225_{2^41^2}+900_{4^21^2}+400_{3^21^4}+600_{421^4}$
$+48_{51^5}+130_{2^21^6}+40_{31^7},$ $Q=126;$

$10\cdot8\cdot6\cdot5\cdot3\cdot2=1+576_{5^2}+400_{3^22^2}+720_{52^21}+1200_{4321}$
$+960_{531^2}+400_{3^21^4}+1000_{32^21^3}+225_{2^41^2}+$
$900_{4^21^2}+600_{421^4}+48_{51^5}+130_{2^21^6}+40_{31^7}+$
$2880_{10}+2700_{4^22}+960_{532}+600_{62^2}+60_{2^5}$ Q=126;

$10\cdot8\cdot6\cdot5\cdot4\cdot3=1+3600_{82}+2400_{64}+1200_{42^3}+400_{3^22^2}$
$+576_{5^2}+720_{52^21}+1200_{4321}+960_{531^2}+$
$1000_{32^21^3}+225_{2^41^2}+900_{4^21^2}+400_{3^21^4}+600_{421^4}$
$48_{51^5}+130_{2^21^6}+40_{31^7}+60_{41^6}+20_{21^8}+$
$1440_{541}+480_{521^3}+1200_{341^3}+900_{42^21^2}+$
$800_{3^221^2}+600_{32^31}+300_{2^31^4}+3600_{4^22}+$
$2880_{10}+960_{532}+600_{62^2}+120_{2^5}+440_{321^5}$ Q=126;

$10\cdot9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3=1+226800_{82}+151200_{64}+72576_{5^2}+$
$18900_{42^3}+25200_{3^22^2}+172800_{73}+403200_{91}+$
$22400_{3^31}+151200_{4321}+90720_{52^21}+86400_{71^3}$
$+151200_{621^2}+4725_{2^41^2}+120960_{531^2}+56700_{4^21^2}$
$+25200_{32\text{[illegible]}^3}+18900_{421^4}+240_{31^7}+8400_{3^21^4}$
$+6048_{51^5}+630_{2^21^6},$ Q=1;

$10\cdot9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3\cdot2=10\cdot9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3+25200_{61^4}+56700_{42^21^2}$
$+60480_{521^3}+1260_{41^6}+3150_{2^31^4}+5040_{321^5}+$
$50400_{3^221^2}+226800_{81^2}+45_{21^8}+50400_{341^3}+$
$201600_{631}+259200_{721}+181440_{541}+25200_{2^331}$
$+362880_{10}+75600_{62^2}+120960_{532}+56700_{4^22}$
$+50400_{43^2}+945_{2^5},$ Q=1.

This theory of many-valued functions, or what is exactly the same thing, this theory of groups, is, after all, though perplexing enough at first sight, merely a trifle compared with the theory of the polyedra, the subject of competition for the grand prize of the Academy for the years 1861, 1863, 1864, &c. This was also proposed early in 1858 thus: *Perfectionner en quelque point important la théorie géométrique des polyèdres.** The null results of the competitions of 1861 and 1863 may be read in the *Comptes Rendus.* Although I have had the honour to present to the Royal Society a complete discussion of this vast theory (which was first written in French, and intended for the contest of 1861), and which has already so far appeared in their Proceedings and in the Philosophical Transactions as to secure my humble claims, it is a fortunate thing for the competitors for the medal of 1861, that the most difficult part of the investigation has yet to be brought to light. As it is not a subject that many persons are anxious to read, I hope that the Royal Society will be in no hurry to anticipate the efforts of our French friends, and thus to spoil the interest of their contests.

The remarks of the distinguished Rapporteur (*Comptes Rendus*, Mars, 1861) on the competition of 1860, brief as they are, are to me the most tempting subject in the world for criticism, chiefly in their reference to the efforts of my two learned competitors. But, even if this were the place for such criticism, my sincere respect for the illustrious Academy, in whose name and authority those remarks are permitted to appear, would command me to forbear. Some will of course suspect that I have said too much already: many know full well how much I have refrained from saying. One security I have; that no man, who can form the dimmest conception of what I have had the honour to do, will cry out *ὄμφακές εἰσι.* The mere proposal of these two prize-questions has been to me a piece of rare good fortune. And I am more than content with the undivided glory, whatever that may be, of having thoroughly discussed, by methods which owe in one case little and in the other nothing to previous writers, the whole of those two extensive theories, on each of which the Academy, in its proposals of 1858, desired to obtain partial information.

CROFT RECTORY, NEAR WARRINGTON,
*July* 1, 1863.

P.S.—It is necessary to add, in reference to Mr. Ekman's letter named at page 133, that while these pages were passing through the press, that gentleman has been courteously informed of the decision of the Committee (*vide* p. 134), to the effect that, as the competition on the prize question of 1860 has been closed, the Academy cannot award any medal for any further discussion of this subject,

## Ordinary Meeting, October 6th, 1863.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Mr. **BARTHOLOMEW STRETTON** was elected an Ordinary Member of the Society.

R. B. CLIFTON, M.A., F.R.A.S., Professor of Natural Philosophy, Owens College, was elected a member of the Council.

Mr. PLATT exhibited some fine specimens of rock salt, from a boring at Middlesborough. The following are records of the results of this and two other borings, executed by Messrs. Mather and Platt.

Middlesborough bore hole, executed for Messrs. Bolckaw and Vaughan; commenced January 4th, 1862.

| | Feet. | In. |
|---|---|---|
| Well, 180 feet deep, consisting of made ground, river mud, sand, red marl, red sandstone, with occasional nodules of gypsum, to a depth of.... | 127 | 0 |
| Fibrous gypsum | 6 | 0 |
| Brown shale | 1 | 0 |
| Red sandstone | 12 | 0 |
| Blue post stone | 3 | 0 |
| Red sandstone | 31 | 0 |
| Total of well | 180 | 0 |
| After commencing to bore they went through a pretty homogeneous bed of red sandstone, occasionally varied with white or yellow veins and thin beds of red marl, to a depth of | 588 | 4 |
| Red marl | 19 | 0 |
| Red sandstone, with occasional beds of red marl ... | 234 | 8 |
| Red sandstone, with nodules of gypsum | 53 | 0 |
| Fine red sandstone and marl | 10 | 0 |
| Ditto ditto | 97 | 0 |

| | Feet. | In. |
|---|---|---|
| Light brown gypsum | 5 | 8 |
| White ditto mixed with red sandstone and limestone | 6 | 10 |
| Blue rock, blue clay, red sandstone, and white stone | 11 | 6 |
| Rock salt, with occasional small portions of rock like limestone | 106 | 4 |
| Total depth | 1312 | 4 |

Bore hole at Vale Dye Works, Bradford, in the lower part of the Yorkshire Coal Field, made for Messrs. Oates, Ingham, and Sons; commenced July 28th, 1862.

Well, 55 feet deep.

| | Feet. | In. |
|---|---|---|
| Soil | 5 | 0 |
| Clay | 2 | 6 |
| Gravel | 12 | 6 |
| Gravel and clay | 3 | 6 |
| River bed | 11 | 6 |
| Black shale | 20 | 0 |
| Bottom of sinking | 55 | 0 |
| **Bore hole.** | | |
| Black shale | 21 | 0 |
| Hard rag stone | 3 | 0 |
| Stone shale | 12 | 0 |
| Close shale | 13 | 0 |
| Dark shale | 27 | 0 |
| Coal | 0 | 3 |
| Soft white stone | 8 | 0 |
| Hard grey stone | 22 | 0 |
| Hard white do. | 5 | 0 |
| Coal band | 0 | 3 |
| Hard white stone | 8 | 0 |
| Hard dark rag | 24 | 0 |
| Fine grain slaty rock | 15 | 0 |
| Coal | 1 | 8 |
| Fire clay | 5 | 0 |

| | Feet. | In. |
|---|---|---|
| Dark shale | 32 | 0 |
| Blue shale with fossil shells | 11 | 0 |
| Shale | 3 | 0 |
| Coal band | 0 | 8 |
| Shale | 3 | 0 |
| Dark rag | 26 | 0 |
| Dark shale | 44 | 2 |
| Millstone grit | 8 | 0 |
| Total | 348 | 0 |

The third bore hole was at Wirrall, near Birkenhead, commenced March 8th, 1861. The well in which this bore hole was made is 23 feet deep, a portion being in the red stone (Trias). From the bottom of the well the boring was:—

| | Feet. | In. |
|---|---|---|
| Red sandstone | 32 | 0 |
| Red and white sandstone | 14 | 0 |
| Close grained red sandstone with quartz pebbles | 3 | 6 |
| Red and white sandstone | 10 | 6 |
| White rock with bands of brown hard substance like iron cement | 14 | 0 |
| Red sandstone | 51 | 0 |
| Red sandstone, very fine | 73 | 0 |
| Red sandstone, finer still | 8 | 0 |
| Red and white sandstone, coarser and softer | 41 | 0 |
| Coarse soft red sandstone | 2 | 0 |
| Red and white marly clay | 0 | 6 |
| Soft red sandstone | 11 | 6 |
| Red sandstone | 13 | 0 |
| Ditto | 90 | 0 |
| Coarse soft red sandstone | 0 | 4 |
| Total | 364 | 4 |

Dr. Clay presented a fine specimen of granular magnetic oxide of iron, from the district "Taranaki," in New Zealand, where it exists in large quantities, lying in seams of considerable thickness near the surface, and exposed in the bluffs lining the water courses. It is so pure that out of 1,000 grains, five or six grains only were left unattracted by the magnet, and those fine silica. Its appearance at first sight is similar to the finest Dartford gunpowder. It has been used limitedly in this country by M. Bessemer, in his steel process, and others.

The Rev. W. N. **Molesworth** drew attention to the earthquake which occurred that morning.

Mr. **Dyer**, also, and other members, gave an account of the phenomenon as experienced by themselves.

Mr. R. D. **Darbishire**, referring to observations made last spring, mentioned the remarkable multiplicity of coins current in Gibraltar, where silver money of many countries appears to circulate freely at ascertained values, and copper money in still greater varieties, to be used indiscriminately, subject only to a rude valuation according to size and weight.

In remarkable contrast with the British system, under which nearly the whole copper coinage has been recalled and replaced within the last five years, and a gold or silver coin fifty years old would scarcely pass, Mr. Darbishire noticed the fact that the current money of Spain, as observed in several of the cities of Andalusia, Granada, and Valencia at this day, includes gold from the middle and silver from the earliest years of the eighteenth century, and copper from (at least) the year 1601.

Referring to the copper currency, he mentioned the introduction, about 1848, of new pieces to represent decimal parts of the Real, and the ineffectual character of the measure owing to the small quantity of these new coins put into circulation. Practically they are suppressed amidst copper money of two centuries and a half.

As illustrating other peculiarities of the copper currency, Mr. Darbishire stated that he had collected in small change from petty tradesmen, peasants, and market people in Seville and Granada, many rude pieces of money, apparently of provincial mints or the earliest dates of (if not anterior to) the monarchy. In Malaga, especially, he had taken not only the like old Spanish money, but a number of coins of the Arabs in Spain (including silver, tin, and copper pieces), and also a large number of still more ancient types,—Roman and ante-Roman. These coins, no doubt, had from time to time been found in the fields or ruins, and thus again put into circulation. Being mostly small, he had taken them as ochavos (half-farthings). He had brought home above sixty selected coins, of dates from Julius Cæsar to Honorius, several of which were of peculiar rarity and interest, and ten autonomous coins of even greater antiquity. The latter were the subject of a Report:

"On ten ancient Spanish coins taken in change at Malaga," by John Harland, Esq., F.S.A.

I. *a*. Two coins of *Malaca* (Malaga) of the same type, from different dies.

Obv. Head with a square topped cap to the right; behind forceps, and in the left margin portions of Phœnician characters M.L.C.A.

Rev. No legend; a star of eight points within a garland of laurel.

I. *b*. A coin referred with doubt to the same mint.

Obv. A bearded head to the right, with conical cap; behind forceps.

Rev. No legend; a large sun with eleven long rays and a human face.

II. One coin of *Obulco* (Porcuna), apparently of the local mint, in alliance with Edeta and Sætabis.

Obv. Large female head to the right. "Obulco."

Rev. Legend in two lines "L. Aimil. M. Juni. Aid." Above a plough, below an ear of corn.

III. Two coins of *Acinipo* (Ronda la vieja).

Obv. A bunch of grapes, between two stars.

Rev. "Acinipo," between two ears of corn. One coin has only one star and the legend "Acinio."

IV. One coin of *Carteia* (Tartessus).

Obv. Very large head to the right with fillet and a well marked profile outline.

Rev. "C. Vib. Aid. (Cart . . . ?)" and a Dolphin.

V. One Coin of *Castulo* (Cazlona) probably minted before the Roman conquest.

Obv. Laureated or filleted head to the right, with upraised hand in front of the mouth.

Rev. Bonneted Sphinx with head thrown back and a spine on the crown, walking to the right with one foreleg raised high.

VI. Two small coins much corroded.

Professor **Christie** explained the occurrence of coins so ancient, by the absence in Spain of the large class of small collectors, which in England and countries usually frequented by English has put a premium on such specimens, and thus causes their rapid absorption when a find takes place. He had not noticed a similar occurrence in the North of Spain.

Mr. **Hurst** mentioned other particulars as to the variegated coinage in use at Gibraltar, and the almost total removal from the Spanish currency of the old silver dollar with twisted pillars, which he explained in connection with the remarkable demand for that coin by the Chinese, who pay for it a premium of 25 to 40 per cent on its money value in Europe.

Mr. **Darbishire** exhibited a tablet of modern standard Spanish coins (a decimal series), and of the present money of Morocco; the latter singularly rude, and much inferior to that of the Spanish Arabs.

Since the meeting the following letter, by Professor W. Thomson, LL.D., F.R.S., Honorary Member of the Society, has been communicated by Professor TAIT: —

Kilmichael Brodick,
Isle of Arran, Oct. 10, 1863.

My Dear Tait,

Yesterday evening, when engaged in measuring the electrostatic capacities of some specimens of insulated wire designed for submarine telegraph cables, I had occasion frequently to discharge, through a galvanometer coil, a condenser consisting of two parallel plates of metal, separated by a space of air about ·007 inch across, and charged to a difference of potentials equal to that of about 800 Daniell's elements. I remarked at an instant of discharge a sharp sound, with a very slight prolonged resonance, which seemed to come from the interior of the case containing the condenser, and which struck me as resembling a sound I had repeatedly heard before when the condenser had been overcharged and a spark passed across its air-space. But I ascertained that this sound was distinctly audible when there was no spark within the condenser, and the whole discharge took place fairly through the 2,000 yards of fine wire, constituting the galvanometer coil. I arranged the circuit so that the place where the contact was made to produce the discharge was so far from my ear that the initiating spark was inaudible; but still I heard distinctly the same sound as before from within the condenser.

Using instead of the galvanometer coil either a short wire or my own body (as in taking a shock from a Leyden phial), I still heard the sound within the condenser. The shock was imperceptible except by a very faint prick on the finger in the place of the spark, and (the direct sound of the spark being barely if at all sensible) there was still a very audible sound, always of the same character, within the condenser, which I heard at the same instant as I felt the spark on my finger. Mr. Macfarlane could hear it distinctly standing at a distance of several yards. We watched for light within the condenser, but could see none. I have since ascertained that suddenly charging the condenser out of one of the specimens of cable charged for the purpose produces the same sound within the condenser; also that it is produced by suddenly reversing the charge of the condenser.

Thus it is distinctly proved that a plate of air emits a sound on being suddenly subjected to electric force, or on experiencing a sudden change of electric force through it. This seems a most natural result when viewed in connection with the new theory put forward by Faraday in his series regarding the part played by air or other dielectric in manifestations of electric force. It also tends to confirm the hypothesis I suggested to account for the remarkable observation made regarding lightning, when you told me of it about a year ago, and other similar observations which I believe have been reported, proving a sound to be heard at the instant of a flash of lightning in localities at considerable distances from any part of the line of discharge, and which by some have been supposed to demonstrate an error in the common theory of sound. I may add that Mr. Macfarlane tells me he believes he has heard, at the instant of a flash of lightning, a sound as of a heavy body striking the earth, and imagined at first that something close to him had been struck, but heard the ordinary thunder at a sensible time later.

Yours truly,
WILLIAM THOMSON.

## Ordinary Meeting, October 20th, 1863.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

A Paper by the Rev. T. P. KIRKMAN, M.A., F.R.S., "On the Complete Theory of Groups," was read.

The following communication was also made by Mr. KIRKMAN:—

I observe that I have not accurately expressed my meaning at page 138, art. VI., of the Proceedings of this Session

The theorem, which can hardly be new, is correctly enunciated thus:—

**Theorem.** If $\theta_a$ and $\phi_a$ be two substitutions of any order $p$, and of the signature $a$, in any group T, and if

$$H = 1 + \theta_a + \theta_a^2 + \ldots$$

contains no group of powers, $h$, permutable with

$$F = 1 + \phi_a + \phi_a^2 + \ldots$$

F being different from H, T cannot contain, as $A_a$, fewer than $R_p$ $(2+R_p)$ substitutions having the signature $a$; $R_p$ being the number of integers, unity included, which are less than $p$ and prime to it.

The proof is simple. Let $\theta^b\theta$ . . be the $R_p$ principal substitutions of H. Then the $2+R_p$ groups

$$F, H, \theta^b F\theta^{-b}, \theta^c F\theta^{-c} \ldots$$

are all different, unless either

$$F = \theta^e F\theta^{-e},$$

or

$$\theta^e F\theta^{-e} = \theta^f F\theta^{-f},$$

$\theta^e$ and $\theta^f$ being principal.

The first gives

$$\theta^e F = F\theta^e,$$

whence

$$\theta^{re} F = F\theta^{re},$$

whatever be $r$, *i.e.* $HF=FH$.

The second gives

$$\theta^{e-f} F = F\theta^{e-f},$$

whence, if $h$ be the group of powers of $\theta^{e-f}$, $hF=Fh$.

When a group T contains more than $R_p$ and fewer than $R_p\,(2+R_p)$ substitutions having the signature $a$, we have an easy tactical method of constructing T upon H or upon F.

The correction above made does not in the least degree affect either my method or my results, in the solution of the Prize-question of the Academy.

In the list of titles at page 142 of the same abstract there is an accidental omission of the following, which is before me in my first manuscript —

$$6\cdot 2 = 1 + 3_{2^2 1^2} + 8_{3^2}, \quad Q = 15.$$

A Paper was read by the President, entitled "Further Observations on the Carboniferous Permian and Triassic Strata of Cumberland and Dumfries," by E. W. BINNEY, F.R.S., F.G.S.

When in 1848 the red sandstones of the neighbourhood of Dumfries first came under the Author's observation, in company with his friend Professor Harkness, doubts came into his mind as to the propriety of their being classed with the trias, their characters and organic remains clearly indicating more of a permian age.[*] Accordingly in his first paper published on this subject in the Society's Memoirs[†] in 1855, allusion was made to these beds, and they were classed as permian after tracking the permian beds of Lancashire through the north-western counties of York, Westmoreland, and Cumberland. His attention was chiefly directed to the red marls, magnesian limestones, conglomerate, and soft red sandstone strata, those being the common Lancashire types; and where the red sandstones of the neighbourhood of Carlisle and St. Bees were incidentally mentioned, they were treated as upper new red sandstone or trias, as Professor Sedgwick has described them in his valuable memoir. But in his second communication,* published in 1857, where the Howrigg, Shawk, and Westward sections are described, he came to the conclusion that "the brick red sandstones of those places, with their underlying red clays, as well as the breccia at Shawk, I have little doubt will be proved to be permian. It is true that no fossil organic remains have yet been found in them, with the exception of the track alluded to in this paper; but if mineralogical characters and geological superposition are to be taken as evidence of their age, they are as good permian beds as those of West House, Kirby Stephen, and Brough, in England, and Dumfries and other places in the south-west of Scotland, with the latter of which they are most probably connected."

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[*] In the *Quarterly Journal of the Geological Society* for 1851, p. 162, Sir R. I. Murchison doubted the sandstone of Dumfries being of triassic age, and preferred to class it with the permian.

[†] On the Permian Beds of the North-West of England, vol. xii., p. 209, of the Society's Memoirs.

In a paper published by Professor Harkness in 1862,† that geologist adopts in substance this view, and agrees with the Author's opinion of the Howrigg, Shawk, and Westward red clays and sandstones being of permian age, and describes a very beautiful section at Hilton, in Westmoreland, which strongly confirms it. Of course it was not intended to question the triassic age of the soft red sandstones of Dalston and Holmhead, near Carlisle, which are covered by waterstones, red marls and lias, as stated in the Author's paper on the latter deposit.‡

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* Additional Observations on the Permian Beds of the North-West of England, vol. xiv., p. 101, of the Society's Memoirs.

† *Quarterly Journal of the Geological Society* for August, 1862, p. 205.

‡ *Ibid* for May, 1859, p. 549.

The Shawk sandstones are well seen at Westward Chapel near Wigton, West Newton, near Aspatria, near Allonby, and to the north of Maryport, and after the Maryport, Workington, and Whitehaven coalfield is passed, they appear again to the south on the coast, in the magnificent promontory of St. Bees Head, and continue southward certainly to Netherton, Seascales, Gosforth, and Drigg Cross, and probably, as Professor Sedgwick suggests, into Furness.

On the north of the Solway the permian strata on the opposite side of the Vale of Eden are well exposed near Riddings Junction, on the Waverley line of railway, about Carwinlay, Moat, and Canobie, and the range of the Moat sandstone (the same age as that of Shawk) by Glenzier Quarry, Cove, near Kirkpatrick Fleming, above Annan, on to Dumfries, is well marked.

In addition to a description of several permian sections at Penton, Riddings, Carwinlay Burn, Barrow Mouth, and Ben How, two sections were given, which showed the occurrence of the upper coal measures, similar to those described by the author some years since in the valley of the Ayr, near Catrine, and thus rendering it extremely probable that such coal measures extend under the valleys of the Eden and the Esk, their southern outcrop being exposed in the Raw Beck, south of Dalston, and their northern outcrop at Canobie. These carboniferous strata may not be rich in coal, but they contain the limestone of Ardwick, Leebotwood, and Ballochmoyle Braes (formerly termed a freshwater one), and show a great development of coal measures, which are useful to be known if it be only to show the depth that has to be sunk through before the middle and profitable coalfields of Whitehaven and Canobie can be reached. This portion of the coal measures both in Scotland and the north-west of England has generally been termed permian, and summarily dismissed as unprofitable "red measures." In the Author's paper on the Ballochmoyle limestone,* it was shown that a great thickness of unprofitable coal measures had to be traversed before the profitable coalfield at Common could be reached, in that district some 550 yards.

The Canobie section exposes far more coal measures above the limestone than the one at Ballochmoyle, at least 200 yards, and it shows a passage of carboniferous into permian beds, so far as the eye can detect, better than any that has hitherto come under his observation. The strata of these two formations in the bank of the river above the bridge at Canobie from the fine breccia into the underlying clays and shales are most difficult if not impossible to separate from the red shales and sandstones seen between that point and the bridge there.

The district about Canobie, Penton, and Longtown, has been described at length by Mr. Edmund Gibsone, in an elaborate and well illustrated memoir printed in the Transactions of the North of England Institute of Mining Engineers.† In the Penton Linns section that Author describes the mountain limestone seams of coal, in the Penton Railway section the millstone grit series, and in the Canobie coalfield the middle series; and he shows a fault on the south of the latter coalfield which throws the coal measures down and brings in the permian strata. All the red measures south of this fault Mr. Gibsone appears to consider permian, and the fault which brings them in he calls the Great Permian fault. After examining these red measures, the Author said he had come to the conclusion that although a portion of them are permian strata, as Mr. Gibsone describes them to be, a great part of them are unquestionably upper coal measures. The profitable Canobie coalfield, like that at Common in Ayrshire, belongs to the middle or valuable coalfield; but there is also at Canobie a great thickness of upper coal measures containing a seam of limestone, in all respects like the Ballochmoyle Braes, near Catrine, the Ardwick and Leebotwood limestones. Consequently, the permian fault should be called by some other name, say the Great South fault.

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* On some Upper Coal Measures containing a Bed of Limestone, at Catrine in Ayrshire.—*Quarterly Journal of the Geological Society* for August, 1862, p. 437.

† A geological paper on the Border Districts of Dumfriesshire, Cumberland, and part of Roxburghshire, including the coal formation of Canobie, &c., by Edmund Gibsone, vol. xi., p. 65.

Practical mining engineers have frequently classed all the red and variegated beds which they find in the upper part of the coal measures as "red measures" or permian strata. Now, there is no doubt often great difficulty in drawing the line of demarcation between the upper coal measures and the permian strata, and it is possible that, in some sections, one may pass into the other, as appears to be the case in the river section above the bridge at Canobie, previously alluded to, but in the north-west of England this transition is not generally to be seen. The further we investigate the organic remains of these two formations probably more genera and species will be found to be common to both than is at present supposed; but in all cases where the remains of *stigmaria* and *spirorbis carbonarius* (*microconchus*) have been found in the strata the Author has termed them carboniferous. In the absence of organic remains, which is generally the rule and not the exception, the permian character of the strata has been decided by the mechanical character of the deposits and the order of superposition, the beds of breccia and the soft red sandstone generally affording pretty good evidence of the permian age of the strata over a great extent of country, and varying with the character of the older rocks found *in situ* in the district. If the permian beds are taken as the Moat sandstone, the red shales with gypsum and four breccias lying in soft red sandstone at Canobie and Riddings, their identification is pretty easy; but in continuing them downwards into the upper coal measures, or in tracing their boundary upwards into the trias, there is greater difficulty, as natural sections showing the passage of one into the other are not often met with; but he considers the soft red sandstone of Longtown, West Linton, Rockliffe, and Dalston to be of triassic age and covered by the waterstones and red marls of Carlisle, and these, in their turn to the west, overlaid by the lias of Quarry Gill and Oughterby.

The Knotty Holm sandstone and a similar rock at Penton, especially in their lower portions, reminded him of the Whitehaven sandstone, and it is possible that they may be of the same geological age as that rock, but for the present he has included them in upper coal measures.

In the valleys of the Esk and Liddel he described some interesting permian sections, and detailed at length the particulars of the strata found over a distance of above twenty miles from the upper coal measures at Canobie to the same beds, as seen in [the Raw Beck, near Dalston, wher ethe following strata are met with, viz.:—

| | ft. | in. |
|---|---|---|
| 1. Red and variegated clays | 13 | 1 |
| 2. Bed of limestone with *spirorbis* &c. | 1 | 0 |
| 3. Red clays | 10 | 0 |
| 4. Purple shales containing *stigmaria* | 80 | 0 |
| 5. Soft red sandstone | 40 | 0 |
| 6. Purple shales | 16 | 2 |

After tracing the Shawk sandstone by Westward Chapel, Wigton, West Newton, near Allonby, to Maryport, he passed over the West Cumberland coal field, and followed it by St. Bees to the south of Cumberland, as far as Drigg Cross. He described at length the permian strata of Barrow Mouth and Ben How, south of Whitehaven. At the former place the beds occurred in the following descending order, viz.—

| | ft. | in. |
|---|---|---|
| 1. Fine-grained red sandstone, laminated and ripple-marked, same as that seen at Moat, Glenzier, Cove, Shawk, Westward, Maryport, and other places, which may be conveniently termed the St. Bees sandstone, fully | 1000 | 0 |
| 2. Shaly marls | 30 | 0 |
| 3. Red marls containing granular gypsum | 29 | 6 |
| Magnesian limestone of a cream colour, containing shells of *Bakevellia* and *Schizodus* | 10 | 6 |
| Breccia composed of pebbles of coal measures, sandstones, and slate rocks | 3 | 0 |
| Red and purple sandstones | 110 | 0 |
| Conglomerate sandstone full of white quartz pebbles, with per oxide of iron and volcanic ash, containing common coal plants | 30 | 0 |

The two last beds have been long known as the Whitehaven sandstone, and Professor Sedgwick many years since classed them as lower red sandstone. After further investigation the Author is inclined to endorse this opinion, as he cannot find any difference between these sandstones and his lower permian beds of Astley, Bedford, and Moira, near Ashby-de-la-Zouch. These singular sandstones lying unconformably to the breccia above and the coal measures underneath, he thinks will be found to be the English representatives of the lower rothliegendes of the Germans.

The Author showed that although the upper coalfield of Lancashire and the Midland Counties of England contained several workable seams, the same beds in Cumberland and Scotland contained none. On the other hand, the mountain limestone series in the latter districts contained numerous seams of coal, whilst none were to be found in the former.

## PHYSICAL AND MATHEMATICAL SECTION.

### October 15th, 1863.

JOSEPH BAXENDELL, F.R.A.S., President of the Section, in the Chair.

Mr. BAXENDELL, referring to Mr. Brothers' "Iris Diaphragm" for determining star magnitudes, stated that, theoretically, the aperture of the diaphragm should vary as the cosine of half the angle of rotation of the moveable ring, and exhibited a diagram showing the close agreement between the theoretical and actual apertures in an instrument he had had constructed under Mr. Brothers' direction, and adapted to Mr. Worthington's equatorially mounted achromatic of five inches aperture. He also stated that the only difficulty he had experienced in using the diaphragm was that of not being able to read off readily the divisions of the scale without artificial illumination; but this difficulty he had found could be obviated by the use of a small telescope magnifying about six times, which showed the divisions of the scale on the darkest night with sufficient distinctness for all practical purposes, and thus enabled the observer to make a series of observations without exposing the eye to extraneous light or leaving the eye end of the telescope.

Mr. BAXENDELL also read the following extract of a letter from Mr. Heelis, F.R.A.S., dated at sea, off Genoa, Sep. 9th, 1863:—

I promised to write to you if I had anything to communicate. I have very little, but that little relates to a rare phenomenon in the Mediterranean, no less than a genuine fog. In the last voyage of the Palestine, on the 7th of July last, being at noon in 2° 51′ W., a little to the westward of Cape de Gatta, she experienced so dense a fog from day-break to nearly four p.m., that the engines had to be run slow, and on this voyage on Sunday morning last a few hours after leaving Gibraltar, and between that place and Malaga, we ran, about 6 a.m., into a dense fog, which lasted, with short intervals of comparatively clear weather, until a little after 11 a.m. The fog was lying in dense lanes, with clearer weather between them, and by a rough approximation of its height, made by noting how far it extended up the mountain sides during the intervals when the weather with us cleared, extended upwards about 4,000 feet. Barometer about 30·100in. The ship had to be run slow and service dispensed with. I see that Smyth in his "Mediterranean" only mentions fogs in the Syrtis. Our captain has made more than seventy voyages up the Mediterranean, and considers the occurrence of fogs where we met with them very rare indeed. Hence, I hope that this may be of interest to you. Of course the weather was calm.

## Ordinary Meeting, November 3rd, 1863.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Edward Hull, B.A., F.G.S., was elected an ordinary member of the Society.

A Paper, entitled "On the Theory of Equations," by His Honour James Cockle, M.A., F.R.A.S., F.C.P.S., &c., the Chief Justice of Queensland, was communicated by the Rev. Robert Harley, F.R.S., F.R.A.S.

Despite the accession to the subject of Mr. Harley and Mr. Cayley, there remains something to be done in the theory of quintics. I think that a root of the given equation should be expressed in terms of a single value of the resolvent product, and (in the *Philosophical Magazine* for March, 1860, and the *Quarterly Journal* for June, 1860) I have taken some steps in that direction. The latter calculations are pushed somewhat further in a manuscript which I forwarded to Mr. Harley some little time since, but as yet I am not aware whether that intrepid calculator has arrived at any conclusion as to which course it would be advisable to adopt. To express a root in terms of a single value of Mr. Harley's $\tau$ or of Mr. Cayley's $\phi$ would, I believe, be desirable.

Shortly before leaving England for Australia it had occurred to me that the theory of what I propose to call "coresolvents" might throw light on such a question as "What is the most general form of linear differential equation of the second order soluble by means of an algebraical (not necessarily a cubic) equation?" and also on the determination of the points of inflection of curves. I have not pursued these inquiries, though I hope soon to be in a condition to call attention to a class of functions for which I shall propose the name of "differential covariants," the existence of which is suggested by that of "differential critical functions." My memoranda however have not yet arrived here.

With respect to the theory of transcendental solution, I would venture to suggest the publication of Professor Boole's solution of a certain quartic resolvent, in a form involving a single definite integral. Mr. Rawson's researches, as yet unpublished, on the algebraical equations on which given differential equations depend are, too, of high interest.

In reference to my communication printed at pages 16-17 of the "Proceedings of the Literary and Philosophical Society of Manchester," (meeting of December 2, 1862), and to Mr. Harley's remarks thereon, let me observe that we may write the Boolian of ($a$) in the form

$$4^3[2D]^3y - 2^2[4D-3]^3e^{\theta}y=0,$$

and that the portion

$$[4D-3]^3e^{\theta}$$

of the sinister is deducible from the portion

$$\left[nx\frac{d}{dx}-r-1\right]^{n-r-1}\left[\frac{n}{r}x\frac{d}{dx}-\frac{n+r}{r}\right]^{r}x^{r}$$

of the sinister of (F) by changing $r$ into $r$—1 in the *exponent* of the first factorial and throughout the *whole* of the second expression and substituting. For this change and substitution gives

$$\left[nx\frac{d}{dx}-r-1\right]^{n-r}\left[\frac{n}{r-1}x\frac{d}{dx}-\frac{n+r-1}{r-1}\right]^{r-1}x^{r-1}$$

or, making $n=4$, $r=2$,

$$\left[4x\frac{d}{dx}-3\right]^{2}\left[4x\frac{d}{dx}-5\right]^{1}x$$

or

$$\left[4x\frac{d}{dx}-3\right]^{3}x$$

which is identical in substance with the selected portion. The calculation of the differential resolvents of

$$y^5 - 5y^3 + 2x = 0$$

and

$$y^5 - 5y^2 + 3x = 0$$

which seem requisite for completing the discussion of trinomial forms, would in all probability throw further light upon the question.

*Brisbane, Queensland, Australia,*
*Wednesday, Aug.* 12, 1863.

Mr. HARLEY added the following remarks "On Recent Researches on the Theory of Equations."

In seeking to express a root of the general quintic equation in terms of a single value of the resolvent product, we are met with considerable practical difficulties. Starting from the Eulerian equations, which connect the coefficients of the quintic with the constituents of its roots, the question becomes one of elimination, and, looked at in a theoretical point of view, it is simple enough. But the eliminations to be effected are so formidable that I have not hitherto had the courage to attempt to carry further the calculations of my distinguished friend. Considering, however, the enormous calculations which have of late years been effected in this department of algebra, and considering also the facilities of verification which recent investigations afford, I should be unwilling to affirm that the work is altogether impracticable. If any mathematician who has the necessary leisure is disposed to undertake the work, I shall be happy to supply him with a copy of the manuscript referred to in the first paragraph of the foregoing Paper, and to offer him such suggestions and assistance as I may be able.

In my last communication to the Society, published at pp. 69-71 of the current volume of the "Proceedings," I have given a brief sketch of Bring's reduction of the general quintic equation to a trinomial form; and at the close of a Paper on the history of that celebrated problem, published at pp. 38-47, of the 6th vol. of the *Quarterly Journal of Pure and Applied Mathematics*, I have given Bring's solution *in extenso*. Since the publication of these Papers Mr. Samuel Bills, of Hawton, has communicated to me a very ingenious simplification of Bring's method. In place of transforming the quadratic in $d$ by means of the assumptions

$$b = ad + \zeta \text{ and } c = d + \gamma,$$

and making the resulting quadratic vanish identically, Mr. Bills, observing that the equation (E) may be written

$$5(3pc + 4qd + 5r)b + 10qc^2 + 25rcd - 15p^2c - 3p^2d^2 - 23pqd$$
$$- 2q^2 - 20rp = 0,$$

assumes

$$3pc + 4qd + 5r = 0$$

or, what is the same thing,

$$c = -\frac{4qd + 5r}{3p},$$

and by substitution in (E) obtains a quadratic in $d$, the solution of which gives $d$ in terms of $p, q, r$; $c$ and $a$ then also become "known"; and substituting for $a$, $c$, $d$ their several values in the coefficient of $y^3$, and equating the result to zero, there arises a cubic in $b$. Mr. Bills also observes, that if the values of $a$, $c$, $d$ thus found were substituted for these letters in the coefficient of $y$, in place of that of $y^3$, and the result were equated to zero, we should be conducted to a biquadratic in $b$. Hence it follows that the general quintic equation may be deprived of its second, third, and fourth terms by the solution of auxiliary equations none of which rise above the third degree; and of its second, third, and fifth terms by the solution of equations none of which rise above the fourth degree. This method of solution may be extended, as I propose to show shortly elsewhere, to the corresponding problems for any general algebraic equation of a degree higher than the fourth.

I am indebted to Sig. F. Brioschi for politely forwarding to me, through this society, a copy of his interesting work, entitled, *Sulla Risolvente di Malfatti per le Equationi del Quinto Grado.* From this work, which is printed in the 9th vol. of the *Memoirs of the Royal Institute of Lombardy,* I learn that Malfatti, in a Memoir published in 1771, in the Transactions of the Academy of Siena, entitled, *De æquationibus quadrato-cubicis disquisitio analytica,* gives a discussion of quintic equations which anticipates to some extent certain recent investigations on the subject. After having proposed and applied an original method for the solution of the lower equations, after having, that is, deduced from each equation another of one degree inferior, which he calls its *resolvent* ("risolvente"), because the solution thereof leads to that of the given equation, Malfatti proceeds on the same method to deal with the equation of the fifth degree, and by a process of elimination arrives at the actual calculation of its resolvent. This resolvent he finds to be an equation of the sixth degree, that is, one degree higher than the proposed equation. Sig. Brioschi is unable to say certainly whether or not Malfatti expected this result, but he inclines to the opinion that Malfatti would scarcely have persisted in so laborious a calculation if he had not hoped that the final equation would be such as to help to a solution of the given one. While Malfatti was engaged upon this problem, Lagrange was publishing in the *Berlin Memoirs* (1770–71), his celebrated method for the general solution of equations, in which he bases the theory on the number of values which functions can take by permutation of the variables. Eight years later Ruffini gave to the world his *Theory of Equations* (published at Bologna), and accompanied it with a demonstration of the impossibility of resolving by radicals any general algebraic equation of a degree higher than the fourth. Malfatti at first entertained some doubts as to the validity of Ruffini's demonstration, and in a Memoir published in the

Transactions of the Italian Society for 1804, he set forth these doubts, supporting his views by a reference to the results obtained by himself in his earlier researches. This work of Malfatti gave rise to two excellent Memoirs by Ruffini, also published in the Transactions of the Italian Society, in the first of which he disposes of Malfatti's objections, and in the second he shows *a priori* that the quintic resolvent is of the sixth degree, and that each of its roots is of four dimensions with respect to the roots of the quintic. Sig. Brioschi in the Memoir above referred to, shows that the sextic equation calculated by Mr. Cockle (now His Honour The Chief Justice of Queensland) and myself, and which we communicated to this Society in 1858-59 (see *Memoirs*, vol. XV., pp. 131-142, and pp. 172-219), is substantially the same as that calculated by Malfatti. Sig. Brioschi further shows, in an Appendix to his Memoir, that Malfatti's resolvent may be readily transformed into the sextic equation recently calculated by Mr. Cayley. (*Phil. Trans.* for 1861, pp. 263-276.)

In a work entitled *Die Auflösung der Gleichungen fünften Grades*, by Adolf von der Schulenburg, Hauptmann, a D., published by H. W. Schmidt, Halle, 1861, I find calculated a certain sextic equation whose roots are of two dimensions with respect to the roots of the quintic. On comparing this sextic with Mr. Cayley's, and making allowance for difference of notation and for the circumstance that Herr Schulenburg's sextic is calculated for the quintic wanting in its second term, whereas Mr. Cayley's is for the complete quintic, I find that the two exactly coincide. By means of the seminvariant process I have completed the coefficients of Herr Schulenburg's sextic, that is to say, I have determined the form of those coefficients for the complete quintic, and I have thus verified Mr. Cayley's result.

Touching the theory of transcendental solution, I may state that my own efforts in this department are at present being directed to the determination of the most general forms of differential resolvents, and to the detection of such properties as are common to these forms. The field is fruitful of results.

Mr. R. D. Darbishire read a Paper "On Marine Shells in Stratified Drift at High Levels on Moel Tryfaen, Caernarvonshire."

After recapitulating shortly the development of the present theories of alteration of level and glaciation in the Caernarvonshire district, and especially referring to the discovery, in 1831, of marine shells in drift-beds near the top of Moel Tryfaen by the late Mr. Joshua Trimmer, F.G.S., Mr. Darbishire described his rediscovery of the fossiliferous deposit in extensive sand cuttings made during the present year in opening out the Alexandra Slate Quarry on the north side of the hill.

Professor Ramsay had traced marine drift to the height of 2,300 feet above the level of the sea in the recesses of Carnedd Dafydd and Carnedd Llewellyn, and had found fragments of shells near Maenbras, two miles west of Snowdon, at about the same height as Mr. Trimmer had given; but otherwise the discovery of 1831 had scarcely been followed up, nor even had any complete list of Mr. Trimmer's shells been published.

By repeated barometric observations, Mr. Darbishire had ascertained the fossiliferous beds to extend upwards from 1,330 to 1,360 feet above the sea level, confirming Mr. Trimmer's own measurement.

Last week, the section being about 35 feet in vertical height, exposed on one line the following series:—

| FT. | IN. | UNDER THE SWARD. | |
|---|---|---|---|
| 0 | 9 | Black peaty soil. | |
| 1 | 9 | Yellowish brown sandy clay, finely and tortuously stratified with scattered, rounded, and angular pebbles ......... | No shells or fragments observed. |
| 1 | 9 | Fine sandy gravel, stratified............... | Ditto. |

| FT. | IN. | | |
|---|---|---|---|
| 5 | 0 | Fine clean gravel or shingle, stratified with rounded or partially rounded pebbles of white quartz, greenstone, porphyry, green sandstone, slate, and pinkish granite | Shells and fragments. |
| 5 | 0 | Fine clean brownish-yellow sand, stratified | Fragments not rare. |
| 6 | 3 | Fine clean gravel or shingle, stratified with pebbles as in No. 4 | Shells and fragments. |
| 7 | 6 | Coarse greenish clayey gravel | None observed. |
| 0 | 8 | Curious very fine slate gravel or shingle, with greenish and clayey sand | A few fragments. |
| 0 | 8 | Edges of slate rock much broken and distorted | None. |
| 6 | 0 | Unbroken slate rock, vertical. | |

The sandy clay roof had doubtless tended to the preservation in this spot of the fossil shells and fragments found below. They are nowhere common, though occasionally aggregated in horizontal patches.

A series of the shells and fragments found during repeated visits was exhibited.

The whole assortment of fossils might be denoted as Arctic, but with a characteristic infusion of "Celtic" or British-Atlantic species, or otherwise as glacial within the British province; the series already indicating the character of the present provincial association of species, though under glacial conditions, and with many northern forms.

In the table of species, as follows, is added a list of species identified from Mr. Trimmer's original specimens in the Museum of the Geological Society, London, by Mr. J. Gwyn Jeffreys, F.R.S., F.G.S.

Scale of frequency: *a*, abundant; *c*, common; *f*, frequent; *r*, rare; *v r*, very rare.

| | | Mr. Trimmer's Specimens. | Mr. Darbishire's List. | REMARKS. |
|---|---|---|---|---|
| 1 | Saxicava rugosa | .. | *r.* | Scarce fragments, not large. |
| 2 | ,, var. arctica | .. | *v. r.* | |
| 3 | Mya truncata | * | *f.* | Small fragments. |
| 4 | ,, var. Uddevallensis | .. | *v. r.* | Fragments. |
| 5 | Corbula nucleus | .. | *v. r.* | Broken valves. |
| 6 | Donax anatinus | .. | 1 | Broken valve. |
| 7 | Tellina solidula | .. | *a.* | |
| 8 | ,, var. balthica | * | *v. r.* | |
| 9 | ,, proxima, Brown | .. | *r.* | |
| 10 | Mactra solida | .. | *r.* | Var. approaching elliptica. |
| 11 | ,, elliptica | * | 1 | |
| 12 | Venus casina | .. | 1 | Fragment. |
| 13 | ,, striatula | * | 0 | E. Forbes. |
| 14 | ,, ? sp. | .. | 1 | Valve. |
| 15 | Artemis exoleta | .. | 1 | Fragment. |
| 16 | Astarte elliptica | * | *f.* | Whole valves and fragments. |
| 17 | ,, compressa | .. | *c.* | Whole valves, small. |
| 18 | ,, borealis | * | *c.* | Fragments. |
| 19 | ,, crebricostata | .. | *v. r.* | Two fragments. |
| 20 | Cyprina Islandica | * | *c.* | Small fragments. |
| 21 | Cardium edule | * | *c.* | Fragments of young and old valves. |
| 22 | ,, echinatum | * | *r.* | |
| 23 | ,, Norvegicum | .. | 1 | Fragment. |
| 4 | ,, ? sp. | .. | 1 | do. |
| 25 | ,, ? sp. | .. | 1 | do. |
| 26 | Mytilus edulis | * | *f.* | Fragments much worn. |
| 27 | Modiola modiolus | * | *r.* | do. |
| 28 | Leda pernula | .. | *r.* | do. |
| 29 | Pectunculus glycimeris | .. | *v. r.* | do. |

| | | Mr. Trimmer's Specimen's. | Mr. Darbishire's List. | REMARKS. |
|---|---|---|---|---|
| 30 | Pecten opercularis | .. | *r.* | Fragments much worn. |
| 31 | Ostrea edulis | .. | *v. r.* | do. |
| 32 | Dentalium entale | * | *v. r.* | do. |
| 33 | „ abyssorum, Sars. | .. | *v. r.* | do. |
| 34 | Fissurella reticulata | .. | 1 | Fragment. |
| 35 | Littorina littorea | * | *f.* | Rolled shells and fragments. |
| 36 | „ rudis var. | .. | 1 | |
| 37 | Lacuna vincta | .. | 1 | Perfect. |
| 38 | Turritella communis | * | *a.* | do. large and small. |
| 39 | Aporrhais pespelecani | .. | *v. r.* | Two fragments. |
| 40 | Natica clausa | * | *r.* | |
| 41 | Trichotropis borealis | .. | 1 | Perfect. |
| 42 | Murex erinaceus | .. | *r.* | Broken shells and fragments. |
| 43 | Purpura lapillus | .. | *f.* | do. |
| 44 | Nassa reticulata | .. | *r.* | Shells and fragments. |
| 45 | Nassa incrassata | .. | *v. r.* | |
| 46 | Buccinum undatum | * | *f.* | One broken, 2 inches long, and fragments of larger individuals. |
| 47 | Fusus gracilis | .. | *v. r.* | Two broken, one a large and coarse form. |
| 48 | „ antiquus | * | *c.* | One 2½ inches long, many rolled fragments of much larger shells. |
| 49 | „ ? craticulatus | .. | 2 | Fragments. |
| 50 | „ ? Kroyeri | .. | 1 | One young shell. |
| 51 | Trophon clathratus | * | *f.* | Large and small shells, perfect. |
| 52 | „ scalariformis | .. | *f.* | Not large. |

| | | Mr. Trimmer's Specimens. | Mr. Darbishire's List. | REMARKS. |
|---|---|---|---|---|
| 53 | Trophon Gunneri | | *f.* | |
| 54 | Mangelia turricula | * | *f.* | Some in very good condition. |
| 55 | „ nebula | * | 0 | |
| 56 | „ rufa | * | 0 | |
| 57 | „ ? pyramidalis Ström | | *v. r.* | Also at Fort William, Jeffreys. |
| 58 | Balanus Hameri | | 2 | Fragments. |
| 59 | Cliona (? two species) | | *v. r.* | In pieces of Cyprina, &c. |

Total, 56 species in 59 forms.

Charles Clay, M.D., read a paper "On the Existence of the Moa of Naturalists in New Zealand," and related two instances in which it had been seen by different individuals, and its very recent footprints by many others, on the shores of the Middle Island; once on the coast near the gold digging locality, and once on the banks of the Mockilinui River, about two miles from the coast, and twenty-five miles north of the Buller River. It is described as at least eight feet in height; the head as large as that of a full grown calf, with a semicircular scarlet patch of about three fingers breadth under each eye, a thick short straight bill; its legs very thick and clumsy; the neck very short. In fact, the head, neck, and legs appeared out of proportion even with its huge body. The bird was feeding from the tops of the underbush, never attempting to feed from the ground, the shortness of its neck rendering such a process impossible. The neck for a considerable distance from the head downward was denuded of feathers, like the vulture, and the general colour of its plumage was of a dusky brown. Both observers differed slightly from the description as given by Professor Owen, but agreed as to the wonderfully correct idea the zoologist had formed of the animal which he had derived from the inspection of a few bones only.

Owen's sketch, altered according to the suggestions of the above observers, will be shortly laid before this society. The average length of the toes, as measured by different individuals from the footprints, is about ten inches. The bones are very frequently met with, and it is hoped from these facts, more will be known of this gigantic animal so very interesting to naturalists.

Dr. Clay also read a Paper entitled "Remarks on the Habits and Character of the Maoris of New Zealand." The author's remarks were illustrated by an excellently formed skull of one of the Maori chiefs, showing (with the exception of somewhat narrow frontal regions) a skull of fair development, with considerable amount of brain space, and considerable capability of mental cultivation if begun early in life. The present condition of this tribe is most degraded and lamentable.

## Ordinary Meeting, November 17th, 1863.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Mr. Samuel Barton Worthington, C.E., was elected an Ordinary Member of the Society.

Mr. F. R. FAIRBANK exhibited several specimens of Flint Implements which had been ploughed up near Lake Erie in Canada.

A Paper was read "On a New Method of Producing Carbonic Oxide," by Dr. F. CRACE CALVERT, F.R.S.

In 1820, my learned master, M. Chevreul, published a most interesting paper on various colouring matters, and drew the attention of the scientific world at that time to the property which most colouring matters possessed, of absorbing oxygen under the influence of alkalies; and although he demonstrated the great advantage possessed by gallic and pyrogallic acids, as agents for analysing the atmosphere, by the extraordinary rapidity with which they absorbed oxygen under the influence of alkalies, still this valuable observation of M. Chevreul remained dormant for nearly twenty years, when Liebig again proved with what facility air could be analysed by this method. I wish now to call your special attention to this mode of absorbing oxygen in a gas mixture, as it is considered by many chemists to be quite as accurate as those methods which are based upon the employment of phosphorus, or metallic copper as suggested by Gay-Lussac; but this is not correct, for I have observed that it contains a serious source of error, because during the absorption of oxygen by the gallic or pyrogallic acid under the influence of alkalies, a certain quantity of a permanent and combustible gas is produced, viz.—oxide of carbon.

It was during a series of experiments which I had undertaken to verify a most interesting fact, published by M. Boussingault, viz., the production of oxide of carbon during vegetation under water, or the decomposition of carbonic acid under the influence of solar rays (see *Comptes Rendus*, vol. 63, p. 862, 1862), that I discovered that the oxide of carbon which M. Boussingault had obtained in his analysis of the gas mixture produced during vegetation, was not due, as he thinks, to the reduction of carbonic acid into oxide of carbon; but that this gas was a product of oxidation, or the result of the action of oxygen on the pyrogallic acid that he used for analysing his gas mixtures.

The following details of my experiments will, I trust, clearly prove this curious production of oxide of carbon:—

1. If over a mercury trough 100 cubic centimetres of oxygen are introduced under a bell jar together with pyrogallic acid and potash, although the greatest portion of the oxygen is rapidly absorbed, still whatever number of hours the substances are left in contact, there will always remain a certain residue incapable of further absorption, and on examining this residue, it will be found to burn with a blue flame, and to be converted thereby into carbonic acid, which compound is easily characterised.

2. If two litres of oxygen, free from any trace of carbonic acid, are made to pass through a solution of pyrogallate of potash placed in Liebig's bulb apparatus, and from thence over carefully prepared oxide of copper, heated to a dull red heat, carbonic acid will be found to issue from the apparatus, and can be collected and weighed.

3. If an unabsorbed portion of gas remaining in the receiver (Experiment, No. 1), be brought into contact with a solution of protochloride of copper, dissolved in hydrochloric acid or

ammonia, the residue will be absorbed, which is an additional characteristic property of oxide of carbon, as indicated by M. Le Blanc.

4. If one volume of this residual gas be introduced into a endiometer, and after being mixed with one volume of oxygen the mixture be exploded by an electrical spark, it is found that half the volume of oxygen employed has disappeared, and that the volume of carbonic acid formed is equal to that of the oxide of carbon introduced into the eudiometer.

As to the quantity of oxide of carbon produced during these experiments, it depends firstly upon the concentration of the pyrogallate employed, and secondly upon its neutral or alkaline state, the minimum being with the acid liquor, and the maximum with an alkaline.

In taking the mean of ten distinct experiments, I found that 100 volumes of oxygen gave two volumes of oxide of carbon, but in some instances the proportion was as high as four per cent.

The production of oxide of carbon also occurs when atmospheric air is substituted for oxygen, only the oxide of carbon is so much diluted by the nitrogen that its presence cannot be detected by several of the above methods. But if, on the other hand, several quarts of air, carefully deprived of carbonic acid, are passed through an alkaline solution of pyrogallate, or gallate of soda, and from thence over some heated oxide of copper, carbonic acid will be found issuing from the apparatus, and can be easily characterised, and even weighed, as I have done in some experiments.

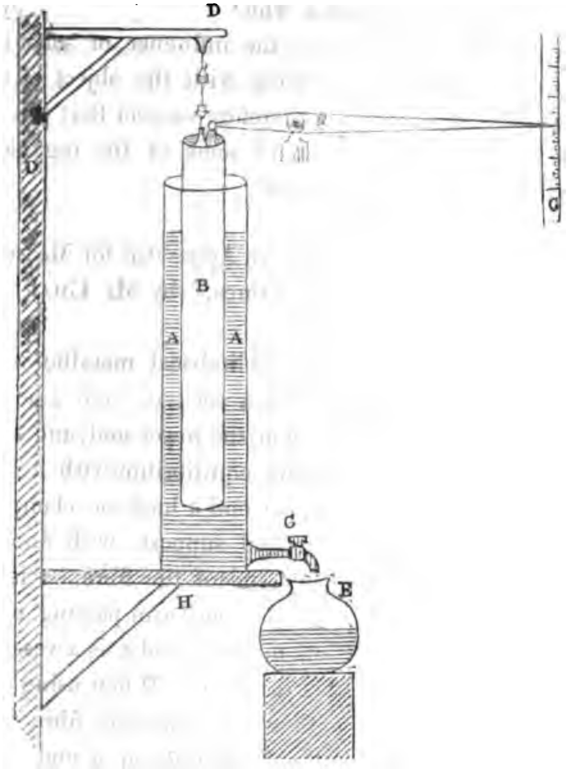
It is no doubt owing to the difficulty of detecting a small quantity of oxide of carbon when diluted with nitrogen, that the presence of that gas has not been observed before by chemists, who have employed Chevreul's and Liebig's method of analysing the atmosphere.

M. Boussingault having observed the presence of a light carburetted hydrogen in the residuum which he obtained in analysing the gases which were produced by vegetation under water, and under the influence of solar rays, I made careful and repeated efforts with the object of detecting it, but failed to do so. I therefore suspect that this gas was the result of decomposition of some of the organic substances present in his experiments.

A paper was read "On an Apparatus for Measuring Tensile Strengths, especially of Fibres," by Mr. CHARLES O'NEILL, F.C.S.

In the sketch, A is a cylindrical metallic vessel to hold water, and provided with a cock C. B is a hollow cylinder of glass or metal, closed at the lower end, and so weighted as to float vertically in stable equilibrium with a portion out of the water; upon its upper end a hook or clamp to hold the fibre is fixed. D is a fixed support, with another hook or clamp to hold the other end of the fibre. F is a lever with a long and short arm, the long arm passing over the scale G. H is the table or support, and E is a vessel into which water drawn from A is received. When using the apparatus it is nearly filled with water, and the fibre to be tested is properly secured to the fastenings on B and D, then drawn taut. Water is now allowed to flow slowly from C until the fibre breaks. The quantity of water drawn off is ascertained, and from it the strain put upon the fibre calculated. The indications of the long arm of the lever are also noted in order to show the stretch, and also to give the elements for a correction to be made upon the quantity of water drawn off.

Stops and guides not shown in the sketch serve to keep the floating cylinder off the sides of the vessel, and prevent it falling too far upon the rupture of the fibre.



The principle upon which the apparatus works is so simple that it hardly requires explanation. At the beginning of the operation the weight of the tube is wholly supported by the water; by drawing off the water the support is very gently removed and the weight thrown upon the fibre. The relation between the actual weight put upon the fibre and the weight of water drawn off will vary for every different dimension of the containing vessel and floating cylinder, but in regularly shaped vessels it will always be in the direct ratio of the sectional areas of the floating vessel and the difference between this and the sectional area of the containing vessel, *i.e.*, in cylindrical vessels the sectional area of the ring of water surrounding the floating cylinder.

In the apparatus brought down for illustration and actually in use by the writer, there were three floating cylinders whose sectional areas bore the following ratios to the ring of water by measurement:—

| | |
|---|---|
| Larger cylinder | 1 : 0·925 |
| Medium cylinder | 1 : 20·610 |
| Smaller cylinder | 1 : 492·6 |

In actually testing, by means of a chemical balance, the relation between the weights of water drawn off and the weights put upon a fibre, the following numbers were found:—

| | | |
|---|---|---|
| Larger cylinder.. | 0·926 | gr. water = 1 grain strain. |
| Medium cylinder | 21·09 | gr. water = 1 grain strain. |
| Smaller cylinder | 476·10 | gr. water = 1 grain strain. |

The large discrepancy in the case of the smaller cylinder is owing to the difficulty of measuring it correctly; its sectional area was computed to be 0·001989 inch.

This apparatus has several advantages: the strain is put on in the most gradual manner, without jerks or shocks; it can be put on at any rate per minute or hour, and there is hardly any assignable limits to either its power or delicacy. By the smaller floating cylinder a strain of 0·0002 grain can be measured, and by increasing the size of the apparatus a strain of a hundred tons could be put on with the most perfect gradation.

Mr. O'Neill also read a Paper, entitled "Experiments and Observations upon Cotton."

(1.) The author began to make experiments upon the chemistry of cotton dyeing, but found himself compelled to abandon experiments upon manufactured cotton and to come down to the primary fibre or hairs of cotton.

(2.) He has made very numerous experiments upon seventeen samples of cotton supplied to him from reliable sources, and compared their physical and chemical properties.

(3.) He has given about 400 experiments upon the length of cotton hairs measured separately by a simple process, which he fully described, and exhibited a diagram upon an enlarged scale, showing the mean, maximum, and minimum lengths of the seventeen qualities of cotton experimented upon. The table below is a résumé of the experiments, but the author furnishes it in this abstract with the caution that, taken apart from the detailed measurements as given in the full paper, it may give rise to incorrect conclusions.

| NAME. | Price. | Date. | Longest Fibre. in. | Mean Length. in. | Shortest Fibre. in. |
|---|---|---|---|---|---|
| Sea Island Edisto | 26d. | Dec., 1860 | 2·00 | 1·680 | 1·35 |
| Sea Island | 54d. | Mar. 1863 | 1·95 | 1·501 | 1·10 |
| Queensland Cotton | | | 1·80 | 1·475 | 1·20 |
| Sea Island | 16d. | Dec., 1860 | 2·05 | 1·444 | 1·10 |
| Egyptian | 9¼d. to 9½d. | „ | 1·55 | 1·252 | 0·95 |
| Egyptian fair | 22d. | Mar., 1863 | 1·50 | 1·185 | 0·85 |
| Maranham | | | 1·40 | 1·220 | 0·95 |
| Benquella | | | 1·50 | 1·177 | 0·85 |
| Pernambuco | 28d. | Mar., 1863 | 1·50 | 1·1675 | 0·75 |
| Maranham | 8½d. | Dec., 1860 | 1·35 | 1·127 | 0·85 |
| Mobile | 6⅞d. | Dec., 1860 | 1·20 | 1·035 | 0·75 |
| Orleans | 7⅛d. | Dec., 1860 | 1·25 | 1·002 | 0·70 |
| Upland | 6⅞d. | Dec., 1860 | 1·20 | 0·9925 | 0·80 |
| Orleans (good middling) | 22½d. | Mar., 1863 | 1·15 | 0·970 | 0·85 |
| Surat (fair Dhollerah) | 17¾d. | Mar., 1863 | 1·15 | 0·9425 | 0·75 |
| Surat (Dhollerah) | 5¼d. | Dec., 1860 | 1·10 | 0·925 | 0·55 |
| Surat (middling Comptah) | 15d. | Mar., 1863 | 1·05 | 0·905 | 0·70 |

(5.) The author has determined the tensile strengths of the hairs of the various qualities of cotton by means of the apparatus described in the previous abstract, and has given, in a series of tables, the breaking weights in grains of every hair tested, with remarks upon them. The following table gives the mean and maximum strengths of the hairs; but, like the preceding table, it ought not to be taken apart from the detailed tables, where the particulars of the breaking of about twenty hairs of each kind of cotton are given:—

| | Mean. Grains. | Maximum. Grains. |
|---|---|---|
| Edisto Sea Island | 83·9 | 142·5 |
| Sea Island (good quality) | 90·0 | 132·0 |
| Benquella | 100·6 | 218·8 |
| Sea Island Cotton | 102·6 | 203·0 |
| Uplands | 104·5 | 212·6 |
| Surat (fair Dhollerah) | 105·8 | 215·5 |
| Maranham | 107·1 | 187·2 |
| Egyptian (fair) | 108·0 | 157·9 |
| Mobile | 118·8 | 172·3 |
| Egyptian | 127·2 | 191·0 |
| Orleans | 139·7 | 289·4 |
| Pernambuco | 140·2 | 251·1 |
| Surat (Dhollerah) | 141·9 | 236·6 |
| Maranham (good middling) | 142·9 | 242·4 |
| Queensland | 147·6 | 246·2 |
| Orleans (good middling) | 147·7 | 264·0 |
| Surat (middling Comptah) | 163·7 | 280·2 |

## PHYSICAL AND MATHEMATICAL SECTION.

### November 12th, 1863.

E. W. Binney, F.R.S., F.G.S., Vice-President of the Section, in the Chair.

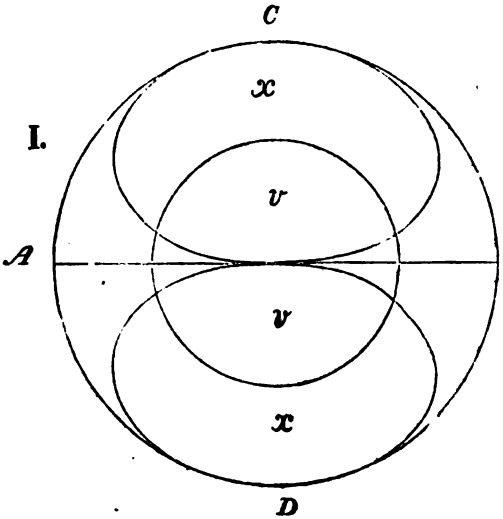
A Paper was read by Mr. J. Atkinson, F.G.S., "On the Construction of Bi-focal Lenses for Spectacles."

The author stated that he had for many years been obliged to wear concave glasses to remedy what is called "short-sight." About seven years ago he found that from some change in his eyes he could no longer read with the concave glasses which he used for looking at distant objects. Not being able to see things distinctly at the distance of even a few feet without spectacles, and at the same time being obliged to take off his glasses to read or to look at any thing close at hand, was such an inconvenience that he sought to obviate it by applying to an eminent optician to make him a pair of glasses with bi-focal lenses. After some delay Mr. Atkinson was informed by this gentleman that the thing could not be done; that the only way for making a spectacle glass to meet the defect complained of, was to cut a convex and a concave lens, each of the proper focus, and to put them edge to edge in the frame, so as to have the convex part below and the concave part above. His arguments to the contrary, on the plea of "ne sutor supra crepidam," not being listened to, Mr. Atkinson was obliged to adopt spectacles of the kind recommended. These glasses answered their intended purpose very well, so far as to seeing both near and distant objects, but besides having an unsightly bar directly across the centre of each glass, they were very liable to accident from falling out of the frame.

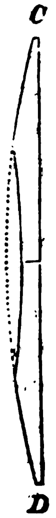
A few months ago, having seen a gentleman with a pair of spectacles with bi-focal glasses, Mr. Atkinson ascertained the name of the optician who made them, and applied to him to have a pair made of a similar kind. He was told that the glasses alluded to were composed of two convex ones of different focal lengths—a kind that could easily be made; but that a convex and a concave lens could not be made as required on the same piece, and that the only method of making spectacles of the peculiar kind he wanted was that which he had already had recourse to. Being still satisfied that this representation was incorrect, Mr. Atkinson succeeded in inducing the optician in question to listen to his suggestions for effecting the object he had in view.

The plan suggested will be best understood by reference to the accompanying diagram, in which *A C B D* represents a circular piece of glass sufficiently large to admit of two spectacle glasses being cut out of it. This, in the first place, is to be ground into a plano-convex lens of the required focus; then in the centre of this is to be ground a concave lens of the right focus, as represented by the inner circle *v v*.

I.



II.



After this the entire piece is to be divided into two equal parts, by any diameter *A B*, and out of each of these a spectacle glass is to be formed as in fig. I. In these *x x* will be the convex portions, forming the lower part of each glass when completed; and *v v* the concave portions, or upper halves of each glass. In fig. II. *C D* represents a section of fig. I. along a line from *C* to *D*. It will be seen that each glass when finished consists of a portion of a plano-convex and a plano-concave lens; and that these can of course be made of any focal length required for correcting the abnormal action of the eye upon the visual rays.*

After the above plan of construction had been described by means of a diagram, the optician last referred to was satisfied of its practicability. He sent the diagram to the firm who manufactured lenses for him (and who had before pronounced the making of such glasses impossible), and in a few days Mr. Atkinson obtained a pair of glasses of the kind he wished for, and which answered his purpose so perfectly as to leave nothing to be desired in the way of improvement.

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* Glasses each being part of a double convex and a double concave lens might, if required, be constructed by similar means.

As he has met with many persons inconvenienced by a defect of vision similar to his own, he considered the foregoing plan of constructing glasses for remedying it worthy to be put on record in the Proceedings of this society.

Mr. Baxendell read the following extracts of a letter from Mr. Heelis, F.R.A.S., dated St. George's Channel, October 23rd, 1863: —

On the 17th Obtober, while running between Cape de Gatte and Malaga in long. 2° 49′ W., lat. 36° 19·1′ N., the captain and I were observing the planet Venus, which was visible to the naked eye and showed her crescent beautifully in one of the ship's signal telescopes, when the quarter-master at the con suddenly, at 10 45 a.m., called my attention to an object in the sky, I looked up and saw a meteor nearly, I should think, twice as bright as Venus, and apparently consisting of two nuclei, with a bright streak joining them, the whole about a degree in length, passing rapidly across the sky in a due easterly direction. It disappeared at an angle of 40° from the horizon, measured by the captain's sextant, which happened to be on deck. The sky was cloudless and very clear. From inquiries from the quarter-master, it appears that he, while looking aloft for Venus, first saw it about 10° or 15° S. of the zenith, and the duration of the apparition was as nearly as I could judge about fifteen seconds. On the two preceding nights we had had many magnificent meteors; and on our passage across the bay, on the 21st and 22nd instant, have since seen several more, notwithstanding the moonlight. At the time of the appearance of the meteor we were nearly abreast of the highest peak of the Sierra Nevada, which was covered with snow. The wind and sea were both calm. Venus was observed every day until the 19th, on which day she was so bright that the captain took her altitude with the sextant soon after her meridian passage. A little before noon on this day we rounded Cape St. Vincent, but shortly before doing so, and when off Cape Sagres, we ran through several large patches of the gulf weed or fucus natans. I could not be mistaken in this weed, and my judgment was corroborated by the captain, who is an old East Indian commander, and who told me also, but without mentioning dates, that he has on several occasions seen this weed in the same place before, and even has at times seen it inside of the Gut of Gibraltar. If this be so, the statement which is, if I remember rightly, made by Maury that it is hardly ever found except on the right of the axis of the Gulf stream, must be incorrect,—and drains of the stream must find their way more commonly than is generally imagined on to the coast of Portugal.

Connected as I think with this, we have had dense fogs along the coast, from Cape Espichel to this side of Cape Finisterre, clearing during the day, but settling down in the afternoon and night, except off Finisterre, where the sun seemed unable to pierce them.

On the 9th September, on the passage up from Gibraltar to Genoa, we were in the morning off the Hyeres Islands, near Toulon, our distance from them when abeam being about twelve miles. We steered E.N.E. by compass all day until 7 p.m., the long. at noon being by chron. 6° 54′ E., lat. observed 43° 00′, variation by amplitude of the sun at sunrise, including deviation 16° 26′ W. At sundown, Cape Della Mele being on the port bow, and the land composed of spurs of the Maritime Alps, being about fifteen miles distant, we found by an observation of Polaris that the variation, including deviation as before, had increased to 22°, and the captain told me that he had on three or four other occasions found the same thing, or rather had found that the ship steered by compass had not at this particular point made a correct course, but that he had always before set the error down to a strong current, which he had assumed to sweep along the coast. This seems a well authenticated case of error induced in highly magnetised compasses by these mountains. From the courses steered by small wooden coasters in company with us, we gathered that their compasses were not affected. Working the ship's dead reckoning from noon, I place her at the time of the morning amplitude in lat. 42° 25·1′, long. 6° 07·7′ E., and at night when the variation was ascertained by an observation of Polaris in lat. 43° 34′, long. 7° 34′ E.

Mr. **Baxendell** read a Paper "On a New Variable Star (T Aquilæ)."

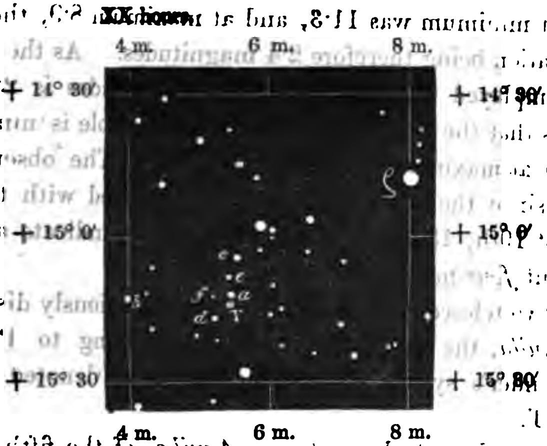
On the night of the 28th of July last, I observed that the star No. 4078 of Zone + 15° of the "*Bonner Sternverzeichniss*," was about *one magnitude and a quarter* less than its neighbour No. 4079, although on the night of the 25th of June I had noted down these two stars as being nearly equal in brightness. Afterwards, on referring to my notes of reviews of this part of the heavens, made at Mr. Worthington's observatory on August 19, 1859, and June 18, 1860, I found that at the former date both these stars were seen, and were then nearly equal in magnitude; but that a third star, No. 4077, was invisible; while under the latter date it is stated that No. 4077 was then visible and estimated to be of the ninth magnitude, but no mention is made of the two stars Nos. 4078 and 4079. I have since made a careful examination of the positions of these and some of the neighbouring stars, and have satisfied myself that the star seen on June 18th, 1860, was No. 4079, and not No. 4077 as was then supposed, and that therefore both 4077 and 4078 were at that time invisible, or, at all events, below the eleventh magnitude. After the 28th of July last No. 4078 gradually diminished in brightness until about the 24th of August, when it passed a minimum. It has since increased, and attained a maximum about the 25th of October. Its magnitude at minimum was 11·3, and at maximum 8·9, the range of variation being therefore 2·4 magnitudes. As the ratio of light employed in determining the magnitudes is 2·512, it follows that the brightness of the new variable is nine times greater at maximum than at minimum. The observations made since the 28th of July last, compared with those of August 19th, 1859, and June 18th, 1860, indicate a period of about *four* months.

As two telescopic variables had been previously discovered in *Aquila*, the new variable will, according to Professor Argelander's system of nomenclature, be denoted by the letter T.

Taking the naked-eye star *ρ Aquilæ*, of the fifth magnitude, as a point of departure, the small chart annexed to this communication will enable observers whose telescopes are not mounted equatorially to find the new variable without difficulty. The magnitudes of the comparison stars are:—

$a=9{\cdot}2.$ $d=10{\cdot}4.$
$b=9{\cdot}4.$ $e=11{\cdot}0.$
$c=10{\cdot}1.$ $f=12{\cdot}0.$

These magnitudes have been determined photometrically by means of Mr. Brothers's "Iris Diaphragm." I may however remark that the results for the star *f* have been rather discordant, and it may therefore prove to be slightly variable. It is very near the place of Argelander's No. 4077, and may probably be the star which he observed and which he estimated to be of the 9·4 magnitude. The place of T Aquilæ brought up to 1865 is 20h. 5m. 25·4s. + 15°·13·4′.



## MICROSCOPICAL SECTION.

### Annual Meeting, May 18th, 1863.

A. G. Latham, Esq., in the Chair.

The Annual Report of the Section for Session 1862-63 was read, and officers appointed for the ensuing Session.

A communication on "The Structure of the Cotton Fibre," by Mr. Charles O'Neill, F.C.S., was then read, in which the author states that chloride of zinc, as neutral as it could be made by digesting with metallic zinc, and also diluted sulphuric acid, would under favourable circumstances exhibit all the phenomena described by the author in his first communication. Chloride of zinc, however, required to be heated to its boiling point, and sulphuric acid appeared very capricious in its action. The appearances produced by these re-agents lead him to the same conclusions with regard to the structure of cotton; but he is more decidedly of opinion than he was before, that the so-called medullary matter is in reality a shrunk membrane similar in appearance to the membrane in dried quills. Finding that all known solvents of cotton gave the same appearances, Mr. O'Neill tried the action of solvents on gun-cotton, and found a further confirmation in the action of ether upon it.

It is well known that there are two modifications of gun-cotton, one soluble, the other insoluble in ether; but the author finds three varieties—(1.) Soluble in ether, but insoluble in ammoniuret of copper; (2.) Insoluble in ether, but soluble or dilutable in ammoniuret of copper; and, (3.) Perfectly unacted upon either by ether or ammoniuret of of copper. Operating on the first variety on the stage of the microscope with ordinary ether, it is almost instantly dissolved with no evidence of structure, until, after a while, careful observation shows some remains of spiral vessels. By gradually diluting the ether with alcohol, the action is slackened until a point is arrived at when exactly the same phenomena are produced as by the copper solution. About two-thirds ether and one-third alcohol was found to be a suitable mixture; but this will evidently vary with different preparations.

Mr. O'Neill considers the number of turns of one spiral to be certainly not greater than from 1100 to 1300 in the inch, and generally much less than this, the mean of many countings running between 600 and 700 for the contracted fibre.

Mr. A. G. LATHAM made the following communication:—

It may be remembered that some few months ago I proposed to this Section as a subject for discussion, "The Causes of the Metallic Lustre of the Scales on the Wings of certain Moths."

I then suggested that the metallic markings, and lustre of the scales themselves forming these markings, are consequent on the fact of the scales containing a particular pigment or colouring matter, while other members thought it might proceed solely from light reflected from the irregular surfaces of the scales.

On examining lately, by transmitted light, the wings on one of the clear-winged moths—Sesia Tipuliformis—I found on the transparent portion of the wing, and in addition to the markings on the wing, certain other scales of battledore form and perfectly transparent.

An examination with a higher power showed these scales to be highly striate, and therefore in the most proper condition for producing, according to the advocates of the theory I oppose, metallic lustre and metallic markings; and that they are in a condition to produce these effects, were the theory correct, is further shown on examination by reflected light—when, as might be expected from the markings, the scales are most beautifully opalescent, but wanting internal pigment give out no metallic markings on the wing, and a strong proof is, therefore, given in favour of the theory broached by me.

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## Ordinary Meeting, October 19th, 1863.

Prof. W. C. Williamson, F.R.S., in the Chair.

The following paper "On Transparent Injections," by Messrs. J. G. Dale, F.C.S., and Thos. Davies, was read by the Secretary.

After enumerating the various desiderata of a transparent injecting fluid, it was observed that soluble colouring matters failed to fulfil them, owing to the action of endosmos, causing them merely to dye the tissue sought to be injected. This defect is shown to be remedied by the use of insoluble colouring matters in an exceedingly fine state of subdivision, which can only be prepared by precipitation under constant agitation, and the following recipe is stated to succeed admirably, showing vessels of $\frac{1}{2000}$ of an inch, with a clear outline even under a $\frac{1}{8}$ objective, without any grain or extravasation of the colouring matter:—

Take 180 grains best carmine, $\frac{1}{2}$ fluid oz. ammonia, com. strength, SG 0·92, or 15 degrees ammonia meter, 3 to 4 oz. distilled water. Put into a small flask, and allow to digest without heat 24 to 36 hours, or until the carmine is dissolved. Then take a Winchester quart bottle, and with a diamond mark upon it the spot to which 16oz. of water extend. The coloured solution must then be filtered into the bottle, and to this pure water must be added until the whole is equal to 16oz. Next dissolve 600 grains in potash alum in about 10 fluid oz. of water, and add to this under constant boiling a solution of carbonate of sodium, until a slight permanent precipitate is produced. Filter and add water up to 16 fluid oz. Boil, and add this solution while boiling to the cold ammoniacal solution of carmine in the Winchester quart, and shake vigorously for a few minutes. A drop now placed upon white filtering paper should show no colouring ring; should it do so, the whole must be rejected. Supposing the precipitation to be complete or very nearly so, shake vigorously for half an hour, and allow to stand till quite cold; the shaking must then be renewed and the bottle filled up with cold water.

After allowing the precipitate to settle for a day, draw off the clear supernatant fluid with a syphon. Repeat the washing till the clear fluid gives little or no precipitate with chloride of barium. So much water must be left with the fluid that at last it must measure 40 fluid oz. For the injection fluid take 24oz. of the above coloured fluid and 3oz. of good gelatine, allow these to remain together all night, then dissolve by the heat of a water bath, after which it should be strained through fine muslins. On injecting, the ordinary precautions for a gelatine injection are alone necessary.

Professor **Williamson** stated that, owing to the unexpected absence of his esteemed friend Mr. Sidebotham, he had been suddenly called upon to give the members of the Society an address at the opening of the session. With so short a warning it was not an easy task; still, as a few stimulating words might lead to extra exertion, he would make a few remarks on the present position of the microscopic observers. Their numbers in Manchester were necessarily small compared with London. Perhaps there were not twenty microscopists in this city really at work; few were able to devote the time to the energetic and laborious efforts which original investigation required, and of these fewer had the talent or even the ambition to undertake what requires weeks, months, nay, often years of arduous toil. The hindrances are increased by the fact, that there is rarely a definite end sufficiently certain of attainment in the way of a new discovery, calculated to repay the expenditure of labour.

Hence, in a small society like ours, we cannot expect great or brilliant results. But further, the present is not an epoch like that when Ehrenberg revolutionised an entire branch of science, or when Greu laid the foundations of vegetable physiology, and Malpighi that of the animal kingdom. These men revealed entirely new fields of enquiry. But though no such new worlds of histology are opened out to us, there are such a multitude of secondary details requiring elucidation, that we cannot take up a plant or insect without stumbling upon a multiplicity of problems awaiting investigation. One shrewd observer, when eating his orange, discovers upon them some brown scales. He follows up the enquiry they suggest, and the result is an elaborate paper on the coccus of the orange.

Even where members are not prepared for original researches, they still may do excellent service by examining the ground gone over by other men, whose views require corroboration before their somewhat startling conclusions can be unhesitatingly received. He would refer to such inquiries as Dr. Hincks's on the conversion of the protoplasm of the volvox into free moving Amœbæ and to those of Dr. Balbiani on the sexuality of the Polygastrica, as illustrations. These researches require re-examination and further confirmation; and whilst the latter would give the results attained a fixed place in scientific annals, their rejection, should they prove erroneous, would remove stumbling blocks out of the way. In fact, all discoveries required careful re-investigation. Observers were often too sanguine, and drew large inductions from small and defective data, and this work of supervision was one in which our members might successfully engage. He also thought it desirable to warn the members against the contracting tendencies of minute microscopic research as opposed to philosophic breadth. If men limit their ambition to resolving the small markings of diatoms apart from the great physiological questions to which they bear relation, they will inevitably succumb to this paralysing influence. They must be careful not to lose themselves in the mere examination of details, but to keep in view that the discovery of general laws should be their object, to the attainment of which the former was only a means. Mere details were useful, but to limit our attention to them crippled the intellect, and rendered it unable to combine them and trace out their connection with general laws. It was by keeping the attention fixed on this higher object that placed our most distinguished histologists on the pedestals they now occupy—and, as it is the duty of every man to do what he does in the best manner he can, it behoves all members to keep this lofty aim carefully in view.

The results would then not only advance science and benefit their fellow-men, but, if worldly fame were their object, they would reap it in the fullest measure to which they were entitled.

## Ordinary Meeting, December 1st, 1863.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

A Paper was read by J. P. Joule, LL.D., entitled "Note on the Meteor of February 6, 1818."

This meteor is noticed in Mr. Greg's very complete list of these phenomena in the British Association Report for 1860. The account of it is, however, given at greater length in the *Gentleman's Magazine*, a periodical which I think is not included in the large number of works consulted by Mr. Greg. This meteor is one of the few which have been seen in the day time, and is also interesting as having been one of the first whose observation afforded materials for the estimation of its altitude. In the work just referred to it is described as follows:—

"At two o'clock p.m. a large and luminous meteor was seen descending vertically from the zenith towards the horizon, in the northern part of the hemisphere, by persons in the neighbourhood of Cambridge. The most remarkable circumstance attending this phenomenon is, that it was thus visible in broad daylight, the sun shining at the same time in great splendour, in a cloudless sky. The same meteor was seen at Swaffham, in Norfolk, at the same hour. It was seen also at Middleton Cheney, near Banbury, in the county of Northampton, not in the zenith, but perhaps 45° from it, in the north eastern quarter of the heavens, shooting along towards the north. It seemed to be divided into two before it became extinct."

The distance between Middleton Cheney and Cambridge in a straight line is 61 miles; which, therefore, will be the elevation of this meteor above the surface of the earth, according to the above observation.

The phenomena presented by meteors have for a long time occupied a good deal of my attention. Many years ago I published my opinion that they arise from the resistance of the atmosphere to the motion of bodies which, wandering through space, become entangled by the earth's attraction. I endeavoured to show that at a very considerable elevation there would be sufficient air to cause the liquefaction and even vaporisation by heat of bodies moving at a velocity so enormous as must be assigned to them if arrested in their motion through space. Latterly new facts have been collected by the Committee of the British Association, and also new experiments in physics have been made bearing on the subject. My object is now to investigate these to see how far they confirm, oppose, or modify my original opinion.

In the Report of the Association for 1862 we have the following results for five remarkable meteors:—

| | Height at | | |
|---|---|---|---|
| Date. | Beginning. | End. | Velocity. |
| 1861—July 16. | 195 | 65 | 55 |
| Aug. 6. | 126 | 21 | 35 |
| Nov. 12. | 95 | 20 | 48 |
| Nov. 19. | 55 | 30 | 24 |
| Dec. 8. | 110 | 45 | 23 |
| 1862—Jan. 28. | 44 | 47 | 46 |
| Feb. 2. | 190 | 15 | 39 |
| Average | 116 | 35 | 39 |

Simultaneous observations at Hawkhurst and Cambridge of the meteors of August 10th of the present year give for the average of 10— 81 ...... 66 ...... 34

The above results give us a very good idea of the height and velocity of meteors. The observations are attended by some difficulty, so that errors are liable to occur in individual instances, but we may rely on the general statement that the larger meteors have their origin at a higher elevation than the smaller ones, and that they attain a nearer distance from the earth at the end of their course.

In addition to the most valuable facts collected by Mr. Herschel, we have a telescopic observation by Schmidt, of the Observatory of Athens, of a splendid meteor on the 19th of October, at 2 55 a.m. Mr. Schmidt was occupied in observing shooting stars, and saw the one in question at its commencement. At first it appeared like a star of the fourth magnitude; after two seconds, it was of the second magnitude; at the third and fourth second, it surpassed the splendour of Sirius. It slowly passed towards the west, appearing a dazzling meteor of 10′ to 15′ diameter. At this moment M. Schmidt followed the meteor with his telescope for fourteen seconds. Its appearance was most remarkable. There were two brilliant bodies of a yellowish green in the form of elongated drops, each followed by a well defined tail of reddish colour. These were followed by smaller luminous bodies of the same shape, each followed by its red trace. The meteor disappeared at an elevation of 1° above the horizon. Four minutes afterwards M. Schmidt still observed the remains of the meteoric train of a yellowish white, and covering an area of nearly 5°.

The above observation is extremely important. It shows, firstly, that meteors are probably for the most part of a compound nature, consisting of a greater or less number of bodies associated together by the force of gravitation—small systems in fact; and, secondly, that they present evident marks of fusion, either in the whole or in part, after passing some time through the rarer portions of our atmosphere.

The researches by Professor Thomson and myself enable us to arrive at reliable conclusions with respect to the temperature acquired by bodies moving rapidly through the air. The law is very simple, viz.:—The temperature ultimately acquired by the moving body is the equivalent of the force with which the particles of air come in contact with it. It is, in fact, the temperature acquired by each particle of air on being caught and suddenly dragged on. This temperature, cleared from the effects of radiation, is 1° C. for a velocity of 145 feet per second, and goes on increasing with the square of the velocity.

Hence we find that the ultimate temperature acquired by a body moving through air of whatever density is for the velocity of 39 miles per second 2000000° C. The question to be solved is whether at the known height of meteors, as above stated, the density of the air is sufficient to give rise to effects in quantity sufficient to account for the actual phenomena.

Now if we reckon the decrease of density to be one quarter for every seven miles, we shall find the quantity of air in a column of, say a mile long and one square foot section, to be about ·0003 of a grain at the height of 116 miles, the elevation at which meteors in general are first observed. The temperature acquired at the surface of a meteorite of a foot section and of the specific heat ·23, moving at the average velocity at the average highest elevation through one mile, will be ·0003 gr. raised 2000000° C., or otherwise ·2 gr. raised 3000° C. which would be doubtless able to fuse any known substance, and bring it to a condition of dazzling brilliancy. The meteorite of a foot section might, I believe, have $\frac{1}{8}$th of a grain of its surface brought to this condition in its passage of one mile in the $\frac{1}{39}$th of a second. I do not think that the luminous effect would be neutralised by conduction of heat to the interior of the meteorite, as it is very likely that the spheroidal condition would be produced. I can therefore easily believe that the $\frac{1}{8}$th grain at 3000° C. would give sufficient light to attract an observer at the distance of 100 or 200 miles.

From data given in Herschel's "Outlines of Astronomy" I find that, at the elevation and velocity above stated, viz., 116 and 39, and supposing as before the entire effect to be given out by radiation, a meteorite of five feet diameter would have the brilliancy of $\alpha$ Centauri.

As the meteorite descends towards the earth its brilliancy will increase to a certain point, when, from the quantity of fused matter, a longer tail will be left behind. This process will of course be sustained for a longer time by the larger meteorites, which are thus enabled to penetrate to a nearer distance from the earth's surface.

## Ordinary Meeting, December 15, 1863.

J. P. Joule, LL.D., F.R.S., Vice-President, in the Chair.

Mr. Robert Leake was elected an Ordinary Member of the Society.

J. C. Dyer, V.P., read a Paper entitled "Notes on some recent Discoveries in Elemental Physics."

He stated that the discoveries in question related to the nature of heat and force. From the earliest times the nature of heat had been treated upon opposite theories; the one maintaining heat to be a material element *sui generis*, pervading space and bodies, but in itself imperceptible except by the effects it produces in connection with bodies: the other theory considering heat to be the result of mechanical force exciting the particles or molecules of bodies, and producing the effects of sensible heat in them. Boerhaave was among the most able expounders of the former theory, Bacon and Boyle of the latter.

In later times the materiality of heat has been forcibly sustained by the discoveries of the existence of latent or specific heat in bodies, by the experiments of Drs. Black, Irvine, Crawford, and others; whilst the mechanical origin of heat has been sustained by the experiments of Count Rumford and of Dr. Joule.

Upon these conflicting views many able treatises have been written on both sides; yet the question as to the nature of heat remains involved in such obscurity as to require far more decisive evidence than has yet appeared for settling either of them upon a sound and immovable basis.

The author gave several extracts from the able address of the President (Sir W. Armstrong) delivered at the last meeting of the British Association—Newcastle, August, 1863—wherein Sir William, in support of the mechanical theory of heat, adduced several facts and considerations which appear inconsistent with that doctrine, or the non-materiality of heat; and as he assumed the dynamical theory of heat to be probably the most important discovery of the present century, Mr. Dyer was led by the same facts and considerations, as well as by many others which he has himself adduced, to conclude that no such definitive discovery has been made in our times, and that the question as to the entity or nonentity of heat remains in the like obscurity in the present as it was in the beginning of the last century.

The author then quoted some passages from the lectures of Professor Tyndall, "on the forms and action of water," and on radiation through the atmosphere, wherein the Professor had assumed that great mechanical forces were exerted in the combination of the gases to form water, as also in the changes of water from its several conditions of solid, liquid, and vapour, and *vice versa*, giving the sum of such forces in equivalents of a ton weight falling from different heights; which phenomena Mr. Dyer considers as solely due to the chemical forces called into action by those changes in the forms and action of water; and as the latter class of forces consists of the affinities and repulsions of the particles of the water excited by the different degrees of heat combined therewith, the assumption of such mechanical forces being exerted in those mutations is untenable.

In giving the results of his interesting experiments of the absorption of heat by aqueous vapour in the air, and of its free radiation through dry air, the Professor says that "this newly discovered property of transparent aqueous vapour must exercise immense influence on the phenomena of meteorology;" but Mr. Dyer gave quotations from Dr. Dalton (in the Memoirs of this Society) and from Dr. Thomson's System of Chemistry, published some sixty years ago, wherein the absorption and radiation of heat by aqueous vapour in the formation and dissolution of clouds, dew, fogs, &c., are clearly explained upon the established doctrine of the mutation of heat reciprocally from and into latent and sensible states; hence the doubt as to any *new* discovery on these points.

To give the course of reasoning submitted by the author against the mechanical theory of heat, and to sustain that of a material calorific element, would require a more copious abstract of his paper than the limits of the printed Proceedings would allow; so that while calling in question both the novelty and the soundness of the discoveries on which he has commented, he refers to his paper *in extenso* for the explanation of his own views on the nature of a material calorific element, and of the elastic forces exerted by the mutations of such elements.

## MICROSCOPICAL SECTION.

### October 19, 1863.

Professor Williamson in the Chair.

A letter from Captain J. Mitchell, dated Madras, the 18th May, 1863, was read, of which the following is an extract:

The view that the universal form of cotton hairs is a flattened fibre consisting of scarcely anything but membrane, is by no means novel; for much the same description of the cotton fibre has been given by Quekett, Henfry, and many others. Indeed that opinion is general. Almost every one you ask will tell you that the cotton fibre is flat and twisted, or perhaps a flat spiral. That this is not the general form of the hairs of good cotton any microscopist may satisfy himself in five minutes. An examination of many varieties of cotton has led me to believe that in those descriptions which find most favour in the English market there is a very large proportion of hairs that are entirely or nearly filled with secondary deposits; and on the contrary, in the low priced cotton the flat fibre consists of hardly anything but membrane; in fact, an apparently undernourished cell predominates. The knotty portions, which I believe are considered refuse, consist almost entirely of these flat fibres. I believe that the absence of secondary deposit is an indication of careless culture, or, what is much the same thing, of a poor soil.

November 16th, 1863.

J. SIDEBOTHAM, Esq., President of the Section, in the Chair.

The PRESIDENT exhibited some spurious gold slides, on which he remarked that the first offered for sale were made by rolling gummed sand in gold leaf, some even more roughly by dabbing gold leaf on sand gummed to the slide; whereas the practice now appears to be to electro-plate plumbago, the result of which however differs from the nugget-like form of real gold by being nicely broken off at what would be the angles. Some very good imitations were made by boiling plumbago in oxalic acid and chloride of gold.

Mr. C. O'NEIL stated that very beautiful spires of gold were observable on the surface of auriferous quartz boiled in carbonate of soda and then submitted to the action of heat for a short time under the muffle.

The PRESIDENT exhibited Phascum rectum and Hookeria lucens in fruit.

## Ordinary Meeting, December 29th, 1863.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Mr. Sidebotham exhibited two photographs which he had taken from a book in the possession of Mr. Buxton, of Daresbury, entitled "Histoire de la Navigation aux Indes Orientales, par les Hollandois," Amsterdam, 1609. One of these represents the title page, which is interesting as showing, in the map of Africa which it contains, the course of the Nile and the two lakes from which it springs, one of them having two outlets. The other photograph shows, among figures of other productions of the Mauritius, that of the Dodo. It would appear that this is the earliest figure of this now extinct bird.

T. T. Wilkinson, F.R.A.S., &c., communicated the following note on the late meteor.

On December 5th, 1863, at about 7h. 55m. p.m., I observed a very brilliant meteor at Haslingden. It passed from the north towards the west, and was first visible to me when near to $\gamma$ *Ursæ Majoris*. The nucleus was of an egg-like form, the head being intensely bright and considerably broader than the rest. The atmosphere was quite clear at the time, and hence I had a good view of its whole passage. Its color was pale blue, and the light it emitted was so intense that the public gas lamps were immediately put into shade and suddenly threw their shadows across the road. The tail of the meteor gradually tapered to a point, and appeared to give out sparks of a purplish tint. When about 4° beneath $\alpha$ *Lyræ*, the meteor exploded, and then the sparks were so variegated in color—the red and purple tints prevailing—that they gave me the idea of a rocket, and I for a moment thought that the Haslingden people were doing this in honor of the Marquis of Hartington's visit to their town. This illusion was, however, immediately dispelled. The whole time of passage did not occupy more than a few seconds. Although several hundreds of persons witnessed the phenomenon, I cannot find that any one heard any sound made by the meteor either during its passage or on its explosion.

**Edward Hull**, B.A., F.G.S., read a paper, entitled "Additional Observations on the Drift-Deposits and more recent Gravels around Manchester."

The object of this paper was to show that the drift or post-pliocene deposits of the borders of Lancashire, Cheshire, and Derbyshire, are divisible into three stages, viz.:—

1. Upper boulder clay.
2. Middle sand and gravel.
3. Lower boulder clay.

The 2 and 3 members had already been described by the president, Mr. E. W. Binney ("Mem. Lit. and Phil. Soc." vol. viii., 2nd series), as also a lower bed of sand and gravel, of whose existence as a distinct subdivision the author had considerable doubts, and considered it as merely accidental. The upper boulder clay had also been alluded to by Mr. Binney, but the author considered it to be quite as important as the lower, both in thickness and area.

The author had found that these subdivisions rose from the plain, or the Valley of the Mersey towards the hills both northward in the direction of the Bolton moorlands, and eastward towards the Derbyshire and Cheshire hills; and amongst these uplands only the upper boulder clay and the gravel made their appearance. These divisions of the drift had already been traced in an area of several hundred square miles.

The author also described a very wide-spread river terrace stretching along the Valleys of the Mersey and Irwell from Didsbury and Manchester westward to near Warrington, and stretching from Altrincham to Eccles. He considered this old terrace of gravel to have been formed at a time when the sea extended further up the valley than at present, and the rivers covered during floods a much wider area than at present.

## Ordinary Meeting, January 12, 1864.

J. C. Dyer, Esq., Vice-President, in the Chair.

The President, in the name of a number of members, presented to the Society a Portrait of one of its Vice-Presidents, J. P. Joule, LL.D., F.R.S., by G. Patten, A.R.A., of London, and intended to be hung up on the walls of the meeting room.

On the motion of Professor Christie, seconded by Mr. Atkinson, it was resolved, "That the best thanks of the Society be given to Mr. Binney and the other donors of Dr. Joule's valuable portrait."

Mr. John Rogerson was elected an ordinary member of the Society.

On the motion of Mr. Dyer, seconded by Mr. Sidebotham, it was resolved, "That the Society do hereby authorise the Council to procure a die for a medal, and to settle the design."

The following extract from a letter from T. T. Wilkinson, F.R.A.S, was read:—"On referring to the article 'Dodo,' in the *English Cyclopædia*, I find that Mr. Sidebotham is mistaken in supposing that his photographs exhibit 'the *earliest* figure of this now extinct bird.' The frontispiece of De Bry's '*Quinta Pars Indiæ Orientalis*,' A.D. 1601, contains 'a pair of these birds on the cornice on each side' of the ornamental border. Clusius, in his '*Exotica*,' A.D. 1605, also gives a figure, which he says is copied from the Journal of a Dutch Voyager, who had seen the bird in a voyage to the Moluccas in A.D. 1598. Mr. Buxton's '*Histoire*,' appears to follow next in order; since the Journal of Admiral Peter Wilhelm Verhuffen, as quoted by the late Mr. Strickland, was printed at Frankfort, in A.D. 1613."

The PRESIDENT said he wished to make a few remarks on the Lancashire and Cheshire Drift. In the year 1841 he first attempted to class the drift deposits found in the neighbourhood of Manchester, in a small Paper with a map, which he prepared for the Statistical Society of Manchester. In that Memoir he divided the foreign drift in the ascending order: (1) lower sand and gravel; (2) till; (3) upper sand and gravel; and he described the more modern deposits found in valleys; (No. 4) as valley gravel. This order he adopted in a Paper read before the Manchester Geological Society on the 22nd December, 1842—"Notes on the Lancashire and Cheshire Drift,"—and printed by that Society in their proceedings of 1843. In that Paper, in treating of the upper beds of sand and gravel, he says—"At Manchester, it (the Higher Drift) is composed of lower gravel, till, and sand and gravel, while at Heywood and Poynton, near the base of the Pennine chain, the beds of sand and gravel are parted by several beds of loam and clay." Again, in speaking of No. 3 deposit, he says—"The gently rising lands of the two counties are generally composed of this deposit. It varies much both in its composition and thickness. Near the sea at Ormskirk, the till is sometimes found without it; but as you proceed to the East it makes its appearance, and gradually thickens until it attains its greatest thickness near the base of the Pennine chain: not only does it increase in thickness, but it becomes more complex, and contains beds of clay, marl, and loam of several yards in thickness. The country lying between Manchester, Bolton, Bury, Rochdale, Ashton, and Stockport, for the most part is upon it, and forms one great sand bank, which continues South into Cheshire." The same classification he adopted in two Papers, one on the Drift of Manchester, and the other on the same deposits at Blackpool, printed in Vols. VIII. and X. of the Society's Memoirs, as well as in a Paper printed in the Manchester Geological Society's Transactions for June, 1862.

Mr. Hull, in his communication, read at the last meeting of the Society, divided the higher drift deposits into (in descending order):—"(1) Upper Boulder Clay; (2) Middle Sand and Gravel; and (3) Lower Boulder Clay. The Nos. 2 and 3 had been described by the President as also a lower bed of sand and gravel, of whose existence he (Mr. Hull) had considerable doubts, and considered it as merely accidental." Now, in his (the President's) Paper on the Drift of Manchester, 11 sections of wells and bores are given, and in ten of those the lower sand and gravel had been met with; thus shewing that it can scarcely be considered as merely accidental, as Mr. Hull states. In many other sections since examined in Lancashire, this deposit has also been found under the till. With regard to the upper bed of boulder clay, Mr. Hull stated that he (the President) had alluded to it, but Mr. Hull considered it to be quite as important as the lower, both in thickness and area.

The old term "till" is as good as that of boulder clay, and as it has been long used there is not much use in changing it. During the last twenty years he had collected many facts, which he intended to publish when he had completed his collection, but these did not show one bed of clay or marl which could be called upper boulder clay, but several; in fact there were numerous intercalations of it in the sand and gravel, one of which he had seen occurring at Kersall Moor entirely surrounded by sand. To show the complexity of these deposits, and the difficulty of reducing them to two, he gave two sections, one near Hyde and the other at Outwood, where the following strata were met with:—

| HYDE. | feet | in. |
|---|---|---|
| Clay | 11 | 0 |
| Quick sand | 2 | 6 |
| Strong marl | 22 | 6 |
| Quick sand | 2 | 6 |
| Loam with pebbles | 12 | 6 |
| Buck leaf marl | 19 | 0 |
| Dry sand | 9 | 0 |
| Quick sand and loam | 6 | 0 |
| Gravel | 3 | 0 |
| Loam | 7 | 6 |
| Gravel and sand | 3 | 0 |
| Clay and loam | 15 | 6 |
| Gravel and soft metal containing pebbles | 10 | 0 |
| Coal measures | | |
| | 124 | 0 |

| OUTWOOD. | feet | in |
|---|---|---|
| Bog | 11 | 0 |
| Quick sand | 53 | 3 |
| Buck leaf marl | 31 | 2 |
| Red sand and gravel with a yard of clay in it | 15 | 0 |
| Toad-back marl | 32 | 3 |
| Gravel | 3 | 0 |
| Coal measures | | |
| | 145 | 8 |

From the position of the Outwood section in a slight depression, and the higher grounds adjoining being capped with a bed of clay containing pebbles eight or ten feet in thickness, another deposit of clay should be placed on its top. Thus in one case there are six beds of boulder clay, and in the other only three. These are two of the many instances which could be adduced, and suggest caution in attempting to classify these deposits without collecting and consulting numerous sections.

A Paper was read entitled "Enquiry into the question, Whether excess or deficiency of Temperature during part of the Year, is usually Compensated during the remainder of of the same year," by G. V. VERNON, F.R.A.S.. M.B.M.S.

In order to see what truth there might be as to the temperature of part of the year, if above or below the average, being compensated during the remainder of the year, the mean monthly temperatures for 92 years for Greenwich, have been made use of. From 1771 to 1849, the observations are those supplied by Mr. Glaisher, in the Philosophical Transactions for 1850. The mean monthly temperatures from 1850 to 1862, are those given in the Greenwich Observations.

The difference between the mean temperature of each month and the 92 years' average for the same month was found for every month during the year, and the figures were collected into two columns according as they were positive or negative. The sums of these two columns will be found in the table annexed to this Paper, and serve to show the number of degrees the mean temperature was above and below the average during the year. These figures have been laid down in a diagram annexed, and which show the irregular character, and at the same time proves most distinctly that if the mean temperature is above or below the average during part of the year, it by no means follows that the converse holds good during the remainder of the year.

During the 92 years, there were 23 in which the total excess or deficiency of the mean temperature amounted to over 20°. There were 28 in which it amounted to from 10° to 20°; 19 years in which it amounted to at least 5°, and 22 in which it was less than 5°.

During a very small number of years only is the mean temperature, viewed in this manner, at all in a proximate state of equilibrium during the year.

The variations below the average reached their maximum value during the period 1781 to 1791, every year but 1781 being below the average. From 1841 to 1851 there was only one year below the average; and from 1851 to 1861, only two years below the average.

Careful inspection of the annexed diagram at once shows, that these variations are exceedingly irregular, and show no approach to periodicity of any kind whatever.

Another Paper was also read by Mr. **Vernon**, entitled, "Examination as to the Truth of the Assertion that when November has a Mean Temperature above the average, it is usually followed by Excessive Cold between the December and March following."

In the table annexed to this Paper all the years since 1771, in which November had a mean temperature above the average, are tabulated, as well as the differences from the mean of the succeeding months of December, January, February, and March.

Following a warm November we find the following figures:

| Months. | Number of Months above the Average. | Number of Months below the Average. |
|---|---|---|
| December | 25 | 15 |
| January | 22 | 19 |
| February | 21 | 20 |
| March | 23 | 16 |
| Sums | 91 | 70 |

Or 91 months above the average against 70 months below the average.

In place of a warm November preceding excessive cold, we find that in most of the years in which severe frosts have occurred early in the year, the November previous has had a mean temperature below the average.

November, 1784, had a temperature 1·7° below the average, succeeded by December 7·8° below, January 0·4° above, February 7·8° below, and March 7·0° below the average.

The great frost which set in severely on January 6, 1814, was preceded by a November 2·2° below the average, and December 2·2° below the average temperature; January was 8·8° below, February 4·2°, and March 5·8° below the average temperature.

The cold period in January and February, 1838, was also preceded by a November 1·3° below the average, and

December 2·4° above; January, 1838, was 6·8° below, and February 5·3° below the average temperature.

Cold winters succeeding a warm November appear to be very few in number, and generally these winters are preceded by a November not much above the average, as in 1783, 1794, and 1799, in which years the mean temperature of November was only 0·5°, 0·9°, and 0.5° above the average respectively.

November, 1822, and 1846, were the only two Novembers much above the average which were followed by a cold period immediately afterwards.

A Paper was read entitled "Note on the amount of Carbonic Acid contained in the air of Manchester," by HENRY E. ROSCOE, B.A., Ph.D., F.R.S.

Determinations of the quantity of carbonic acid contained in the air of towns have been made by Dr. Angus Smith in Manchester, and by Lewry in Paris; but as the experiments hitherto made upon the subject are few in number, and have yielded somewhat remarkable results, it appeared of interest to carry out a series of determinations of atmospheric carbonic acid, made by unimpeachable methods, and extending for a considerable time, under wide variation of weather.

The analytical method employed was the excellent volumetric one proposed by Pettenkofer, and this was checked in several instances by simultaneous weight determinations made by absorption in caustic potash. The close agreement of the results in experiments Nos. 8 and 9; Nos. 15 and 16; Nos. 42 and 43; and Nos. 45 and 46, in the accompanying table, gives proof of the reliability of the methods. The experiments were made under my supervision by Mr. Arthur Mc. Dougall.

In the case of the weight analyses, a given volume (not less than 35 litres) of air was drawn in the first place over three weighed tubes containing sulphuric acid and pumice-stone, the weight of the third tube being shown to remain constant; the air then passed through a Liebig's Bulbs containing potash, over two tubes containing solid potash; and, lastly, through two tubes containing sulphuric acid and pumice-stone, the weight of the second of these remaining constant. The volumetric analyses were made in globes of 7-10 litres capacity, with standard solutions of Baryta-water and Oxalic acid, exactly according to the method described by Pettenkofer.

The accompanying Table gives the results of 54 separate determinations of the carbonic acid in the open air of Manchester and the neighbourhood, made during the autumn and winter months of 1862-63-64; days being especially chosen upon which the amount of carbonic acid might be considered likely to be the greatest.

The first and most important conclusion to which these experiments lead is that the amount of carbonic acid contained in Manchester town air differs but very slightly (if at all) from that contained in the air of the neighbouring country. Thus, from experiments Nos. 23 and 24, made at Stretford (four miles west of Manchester) with the wind blowing towards Manchester, the quantity of carbonic acid found on Feb. 3, 1863, was 3·85 volumes in 10,000 volumes of air, as mean of two experiments, whereas on the same day the quantity found in the centre of Manchester (Owens College) was found to be 3·90 vols. in 10,000 vols. of air, as a mean of two experiments. Nos. 31, 32, and 33, made at Stretford on Feb. 19th, 1863, a damp day, with wind blowing from Manchester, showed a mean of 2·77 vols. of carbonic acid, whilst at Manchester, on the same day, the volume of carbonic acid was found to be 2·8 in 10,000 vols. of air.

The maximum quantity of carbonic acid was found in Manchester air on January 7th, 1864, (on which day there was a dense fog) when the amount reached 5·6 vols. per 10,000 of air; the minimum quantity on February 19th, 1863, being 2.8 vols, per 10,000 of air. The mean of 46 determinations made in the centre of the town of Manchester, gives the volume of carbonic acid as 3·92 in 10,000, and that of eight experiments made outside the town gives the number 4·02 as the composition of the country air regarding carbonic acid. These numbers closely agree with a determination by weight which I made in London, on February 27th, 1857, from which the carbonic acid London air was found to be 3·7 vols per. 10,000. 3 vols.

Experiments 47, 48, and 49 prove that continuous rain may lower the amount of atmospheric carbonic acid from 4·8 to 3·3 volumes per 10,000.

The above results show that the maximum quantity of carbonic acid contained in Manchester air, even in a dense fog, and when there is no wind, does not exceed 6 volumes per 10,000 of air; whilst the mean quantity, 3·9 volumes, closely agrees with that (4·0) generally assumed, from Saussure's early experiments, to represent the average composition of the atmosphere as regards carbonic acid.

## BONIC ACID IN THE AIR OF MANCHESTER AND THE NEIGHBOURHOOD.

| Place. | Date. | Wind, Weather, &c. | Barometer, in Millimetres. | Temp. C.° | Vol. of Carbonic acid in 10,000 vols. of air. | Description of Method Employed. |
|---|---|---|---|---|---|---|
| | 1862. | | MEAN. | | | |
| anchester | Oct. 19 | … … … … | 753·0 | 10·5 | 4·0 | Pettenkofer's |
| „ | „ 22 | … … … … | 740·0 | 16·0 | 4·0 | „ |
| „ | „ 22 | … .. … … | 741·0 | 15·8 | 4·0 | „ |
| „ | Nov. 7 | Heavy, very damp mist—calm ……… | 767·0 | 5·0 | 3·9 | „ |
| „ | „ 10 | Snow, with rain—wind strong ……… | 740·7 | 6·0 | 3·6 | „ |
| „ | „ 11 | Snow and rain—east wind ……… | 750·5 | 3·5 | 3·6 | „ |
| „ | „ 12 | Exceedingly thick fog—no wind …… | 760·0 | 2·0 | 4·5 | „ |
| „ | „ 13 | Very clear—frosty and calm ……… | 758·5 | 4·0 | 3·7 | „ |
| „ | „ 13 | „ „ „ ……… | „ | „ | 3·5 | By weight |
| „ | „ 18 | Very thick fog—calm and cold ……… | 769·0 | 2·0 | 5·2 | Pettenkofer's |
| „ | „ 19 | Slight rain—no wind ……… | 766·8 | 5·6 | 3·5 | „ |
| „ | „ 20 | „ „ ……… | 766·0 | 5·0 | 3·7 | „ |
| „ | „ 25 | Slight fog—calm ……… | 755·0 | 1·0 | 4·1 | „ |
| „ | Dec. 3 | Fine and clear ……… | 751·8 | 10·0 | 3·6 | „ |
| „ | „ 10 | Clear—S.W. wind ……… | 754·5 | 8·0 | 3·6 | „ |
| „ | „ 10 | „ „ ……… | „ | „ | 3·63 | By weight |
| „ | „ 11 | … .. … … | 754·0 | 8·0 | 3·60 | Pettenkofer's |
| | 1863. | | | | | |
| „ | Jan. 16 | Damp—slight wind ……… | 754·0 | 8·0 | 3·6 | „ |
| „ | „ 26 | Fine and dry—fresh wind ……… | 758·0 | 10·0 | 3·5 | „ |
| „ | „ 27 | „ „ ……… | 765·0 | 7·0 | 3·3 | „ |
| ham … | „ 31 | Clear, bright—wind N.W. from the sea. | 745·5 | 5·5 | 4·3 | „ |
| „ | „ 31 | „ „ „ | „ | „ | 4·6 | „ |
| ford … | Feb. 3 | Clear—wind N.W. towards Manchester. | 758·0 | 9·5 | 3·9 | „ |
| | „ 3 | „ „ „ „ | „ | „ | 3·8 | „ |
| Wh | „ 3 | „ „ „ „ | „ | „ | 3·9 | „ |
| | „ 3 | „ „ „ „ | „ | „ | 3·9 | „ |
| likel | „ 3 | „ „ „ „ | „ | „ | 3·7 | „ |
| … | „ 10 | „ „ „ „ | 762·4 | 11·0 | 5·0 | „ |
| | „ 10 | „ „ „ „ | „ | „ | 5·0 | „ |
| anchester | „ 10 | „ „ „ „ | „ | „ | 4·6 | „ |
| retford … | „ 19 | Wind S.E. from Manchester—damp… | 772·0 | 10·0 | 2·7 | „ |
| „ | „ 19 | „ „ „ „ | „ | „ | 2·85 | „ |
| anchester | „ 19 | Damp—wind S.E. ……… | „ | „ | 2·8 | „ |
| „ | „ 20 | „ „ ……… | 768·0 | 10·0 | 3·0 | „ |
| „ | „ 23 | „ „ ……… | 767·0 | 9·0 | 2·9 | „ |
| „ | „ 25 | „ „ ……… | … | … | 3·7 | „ |
| „ | „ 25 | … … … … | … | … | 3·7 | „ |
| „ | „ 27 | … … … … | 763·0 | 11·0 | 3·6 | „ |
| „ | „ 27 | … … … … | „ | „ | 3·6 | „ |
| „ | Mar. 2 | … … … … | 753·0 | 11·0 | 3·6 | „ |
| „ | „ 3 | Fine—wind due north ……… | 752·0 | 18·0 | 5·2 | „ |
| „ | „ 3 | „ „ „ ……… | „ | 15·0 | 5·1 | „ |
| „ | „ 3 | „ „ „ ……… | „ | 15·0 | 4·7 | By weight |
| „ | „ 6 | „ „ „ ……… | 752·0 | 15·0 | 3·5 | Pettenkofer's |
| „ | „ 6 | „ „ „ ……… | „ | „ | 3·5 | „ |
| „ | „ 6 | „ „ „ ……… | „ | „ | 3·7 | By weight |
| „ | Dec. 28 | Damp, foggy—a little snow ……… | 765·6 | 2·5 | 4·9 | Pettenkofer's |
| „ | „ 28 | „ „ „ ……… | „ | „ | 4·7 | „ |
| „ | „ 29 | Raining all night previously, and during the experiment—wind W. | 752·5 | 10·0 | 3·3 | „ |
| „ | „ 31 | Fine—wind fresh, S.E. ……… | 755·0 | 4·0 | 3·56 | „ |
| | 1864. | | | | | |
| „ | Jan. 7 | Very thick fog—no wind—hoar frost. | … | −6·5 | 5·6 | „ |
| „ | „ 7 | „ „ „ | … | „ | 5·6 | „ |
| „ | „ 8 | Fog in morning—afternoon clear …… | … | 0·5 | 3·7 | „ |
| „ | „ 9 | Clear and bright—frost ……… | … | 0·1 | 4·1 | „ |

| Mean of | | Vols. of carbonic acid in 10,000 vols. of air. |
|---|---|---|
| 46 | Experiments made in Manchester ……… | 3·92 |
| 8 | „ the Country ……… | 4·02 |
| 54 | „ Manchester and district | |

Hence we may conclude that the combustion of coal and the respiration of animals exert no appreciable influence on the quantity of carbonic acid contained in the town air of Manchester collected in an open situation; gaseous diffusion and the great motions of the atmosphere serving completely to disperse the millions of tons of this gas which every year are evolved by the above-mentioned causes in this neighbourhood.

I may add the following determinations, made in the same way, of the carbonic acid contained in the air of closed inhabited spaces:—

1. Chemical Theatre, Owens College, during lecture.
   Temp. 9° C. Bar. 755 mm. $CO_2$ in 10,000 of air 9·5 vols.
2. Chemical Laboratory, Owens College.
   9 o'clock a.m. Temp. 17·5° Bar. 733·5. $CO_2 = 8·3$ vols. in 10,000.
   12 o'clock Temp. 19·5° Bar. 733·5. $CO_2 = 9·0$ vols. in 10,000.
3. Large bedroom, with invalid and attendant.
   Temp. 12°. $CO_2$ in 10,000 vols. of air = 7·4.
4. Parlour (capacity 3,000 cubic feet), four persons, three gaslights, good fire.
   Temp. 19°. $CO_2$ in 10,000 vols. of air = 13·2.
5. Ditto ditto.
   Temp, 18°. $CO_2$ in 10,000 vols. of air = 13·6.
6. Ditto ditto.
   Temp. 15°. $CO_2$ in 10,000 vols. of air = 14·5.
7. Crowded meeting-room of artisans in Penny's Mill, Gaythorn—Guardian Schools—1,000 persons.
   Temp. 18°. $CO_2$ in 10,000 vols. of air = 36·5.
   $CO_2$ in 10,000 vols. of air = 35·5.

## MICROSCOPICAL SECTION.

December 21, 1863.

JOSEPH SIDEBOTHAM, Esq., President of the Section in the Chair.

Various valuable donations were announced, among others a paper by Walter Crum, Esq., F.R.S, on Cotton Fibre, accompanied with mounted specimens, in illustration of it.

Mr. HEYS repeated his observations on the the cotton fibre, and said that there was little difference observable in cotton freshly gathered, from that as usually received in this country. His observations lead him to the conclusion that the structure of cotton fibre is as follows:—First, there is an external envelope, or tube, generally moniliform; inside, a spiral vessel which seems to prevent the collapse of the tube: inside the spiral, there is generally present another substance like a pith or core. Mr. Heys then described at length his observations on the cotton when under the influence of the solvent recommended by Mr. O'Neil, and the conclusions he drew from them. Mr. Heys's paper was illustrated by diagrams and mounted specimens.

Mr. HEYS then read a Paper on "Mounting Objects in Canada Balsam," and minutely explained the various details of the process: he strongly advocates the use of the Canada Balsam, dissolved in Chloroform, by which the trouble is lessened, and the beauty of the preparations increased.

The SECRETARY then exhibited a Drawing of the Apparatus used by Captain Baker, of the Niphon, for obtaining Soundings, free from Grease.—Description: A tube 18 inches long, 1¼ diameter, with a wooden cap, and a leather, fitted as a pump-box; this, lashed to the lead, sinks into the earth, and brings up a cylinder of mud or sand, free from grease; the surplus water being forced out through the valve at the top.

## PHYSICAL AND MATHEMATICAL SECTION.

### January 7, 1864.

J. **Baxendell**, F.R.A.S., President of the Section, in the Chair.

Two Papers were read by Mr. **Vernon**. (These were afterwards read at the ordinary meeting of the Society, January 12. See page 216.)

The following returns of the Rain Fall for 1863, were communicated to the Section.

### Old Trafford, Manchester.

By G. V. Vernon, F.R.A.S., M.B.M.S.

Rain Gauge three feet above the ground, receiving surface 106 feet above the mean level of the sea.

| Quarterly periods. | | 1863. | Fall in inches. | Average of 70 Yrs. | Differ. | Number of Days on which Rain fell in 1863. | Quarterly Periods. | |
|---|---|---|---|---|---|---|---|---|
| 1862. Days. | 1863. Days. | | | | | | 1863. | 1862. |
| | | | | Inches. | Inches. | | Inches. | Inches. |
| 45 | 45 | January... | 4·425 | 2·471 | + 1·954 | 17 | 6·170 | 6·523 |
| | | February . | 0·941 | 2·356 | — 1·415 | 16 | | |
| | | March ... | 0·804 | 2·314 | — 1·510 | 12 | | |
| 61 | 53 | April...... | 1·391 | 2·044 | — 0·653 | 17 | 7·746 | 10·259 |
| | | May ...... | 1·724 | 2·345 | — 0·621 | 14 | | |
| | | June ...... | 4·631 | 2·913 | + 1·718 | 22 | | |
| 51 | 56 | July ...... | 1·630 | 3·619 | — 1·989 | 7 | 12·216 | 11·875 |
| | | August .... | 5·027 | 3·610 | + 1·417 | 25 | | |
| | | September | 5·559 | 3·225 | + 2·334 | 24 | | |
| 61 | 61 | October ... | 6·242 | 3·845 | + 2·397 | 22 | 12·208 | 9·941 |
| | | November | 2·902 | 3·477 | — 0·575 | 18 | | |
| | | December. | 3·064 | 3·279 | — 0·215 | 21 | | |
| 218 | 215 | Total ...... | 38·340 | 35·498 | + 2·842 | 215 | 38·340 | 38.598 |

The rain fall during the past year, as will be seen above, has been 2·842 inches above the average of the last 70 years, and almost identical with the rain fall of 1862, the difference being only 0·258 inch. In 1862 rain fell on three more days, or on 218.

During the four months of February, March, April, and May, 1863, the fall of rain was only 4·860 inches, or 4·231 inches below the average of the same months for 69 previous years; in 1806, the fall for these four months was 4·278 inches, and this is the only case from 1794, in which the fall was as small for the period in question as in 1863. As compared with 1862, 1863 had a remarkably dry spring, but an excess of rain fell during the last six months of the year. Upon comparing the last three months of the year, in the table above, it will be seen that rain fell upon the same number of days in 1863 as in 1862 for these months, but 2·267 inches more fell in 1863 than in 1862. The fact just noticed shows that the number of days upon which rain falls may be quite independent of the amount of the fall, and how desirable it is to have the number of days upon which the rain actually falls, as well as the amount.

## THELWALL, near WARRINGTON.

BY JOHN ATKINSON, F.G.S.

The height of the station above the mean sea-level is 96 feet. The gauge is about one foot above the ground.

| | 1863. | Inches. | Days on which rain fell. |
|---|---|---|---|
| 5·529 | January | 3·715 | 14 |
| | February | 0·811 | 13 |
| | March | 1·003 | 13 |
| 9·307 | April | 1·389 | 13 |
| | May | 2·276 | 11 |
| | June | 5·642 | 22 |
| 11·000 | July | 1·977 | 8 |
| | August | 3·897 | 25 |
| | September | 5·126 | 21 |
| 12·624 | October | 6·801 | 21 |
| | November | 3·488 | 12 |
| | December | 2·335 | 17 |
| | | 38·460 | 190 |

In 1862 the rain fall at Thelwall amounted to 35·840 inches, distributed over 219 days. Of this quantity, 6·821 inches fell in the first three months; 9·139 inches in the second; 10·328 inches in the third; and 9·552 inches in the last three months of the year. The fall in 1863 was in excess of that of 1862 by 2·62 inches. In each of the months—January, February, April, June, August, September, October, and November, in 1863, the rain fall was in excess of that registered in the corresponding months of 1862. It will be seen that more than half the quantity of rain in 1863 (viz. 19·312 inches) fell in the four months August, September, October, and November, whereas these four months gave about one-third only of the total amount in 1862. Excessive amounts of rain fell on the following days in 1863, causing floods of greater or less extent:—January 1 and 2, 1·184 inches; June 7, 8, 9, 1·25 inches, and on the 10th and 11th, 2·642 inches (a very great flood); July 21 and 22, 1·5 inches; and October 29 and 30, 1·35 inches.

### THE FLOSH, CLEATOR, near WHITEHAVEN.

BY THOMAS AINSWORTH, ESQ.

| 1863. | Inches. | Number of Days of Rain. |
|---|---|---|
| January | 6·34 | 28 |
| February | 2·59 | 20 |
| March | 1·90 | 18 |
| April | 2·91 | 21 |
| May | 4·05 | 16 |
| June | 3·98 | 20 |
| July | 1·25 | 7 |
| August | 4·08 | 22 |
| September | 6·33 | 26 |
| October | 6·51 | 28 |
| November | 6.97 | 26 |
| December | 3·64 | 23 |
|  | 50·55 | 255 |

## ECCLES, DURING 1863, AND A COMPARISON WITH 1862.

By THOMAS MACKBEETH, M.B.M.S.

| 1862. | 1863. | | Amount in Inches. | Days it Fell. |
|---|---|---|---|---|
| Inches. | Inches. | | | |
| | | January | 3·959 | 19 |
| 8·005 | 5·794 | February | 0·872 | 16 |
| | | March | 0·963 | 14 |
| | | April | 1·254 | 16 |
| 9·533 | 7·408 | May | 1·792 | 13 |
| | | June | 4·362 | 22 |
| | | July | 1·690 | 7 |
| 10·986 | 11·702 | August | 4·736 | 25 |
| | | September | 5·276 | 25 |
| | | October | 5·393 | 23 |
| 9·140 | 11·312 | November | 3·164 | 17 |
| | | December | 2·755 | 24 |
| 37·664 | 36·216 | Total | 36·216 | 221 |

## ROYTON, near OLDHAM.

BY JOHN HEAP, ESQ.

| 1863. | Inches. | Number of Days of Rain. |
|---|---|---|
| January | 6·412 | 26 |
| February | 1·803 | 20 |
| March | 1·223 | 17 |
| April | 1·599 | 19 |
| May | 2·325 | 12 |
| June | 3·785 | 19 |
| July | 1·700 | 8 |
| August | 5·230 | 24 |
| September | 6·625 | 26 |
| October | 6·205 | 22 |
| November | 3·415 | 19 |
| December | 3·775 | 21 |
| | 44·097 | 233 |

Mean fall of 27 years, 33·151 inches.

## Ordinary Meeting, January 26th, 1864.

Dr. R. Angus Smith, F.R.S., Vice-President, in the Chair.

The Chairman showed a copy of the "Mundus Subterraneus" of Athanasius Kircher, edition of 1678, Amsterdam, written in 1662. In the first volume are five maps, on which the Nile with some of the adjacent country is delineated. The earliest is taken from an Arabian work on geography. In this the Mountains of the Moon are depicted about twenty degrees south of the equator, and running east to west: from the eastern half run five rivers which meet in a small lake; out of this small lake run three rivers which run into a large lake, the north of which is south of the equator. The same number, *i.e.* five rivers, run from the west, and passing through a small lake, run in three divisions to the same great lake, which is called Zambie. Out of this are made to flow three rivers, one the Nile, another the Zaire.

A map by Odoard Lopez makes the centre of Africa a hilly country well supplied with lakes and rivers; all these lakes are united, and send their waters in every direction. This makes the Zaire, the Zambesi, and the Nile unite, by uniting the lakes from which they spring. One of these lakes is called the Zaire. It would seem as if there were some knowledge of the existence of lakes at the time, whilst a bold guess completed the map.

A third map is ushered in with great ceremony. Kircher obtained it from the procurator of India at Rome. It is drawn by Peter Pais, a jesuit, who visited the fountains of the Nile on the 21st of April, 1618, along with the Emperor of Ethiopia. In this the Mountains of the Moon are also prominent. On a hill north of them are two fountains, "oculi Nili" or "fontes Nili." These form the Nile, which falls into the lake Bed, on the shores of Goyam, Bed, and Dambia. This seems as clear an account of the Blue Nile as one might expect from a traveller of the period. The Jesuits were, at the time mentioned, in Abyssinia, and Pais, whose name is written also Paez and Payz by other writers, is said to have gone there in 1603.

Kircher desires to inform posterity of this great discovery, which to his mind puts aside the preceding maps. We see in these three maps the Blue Nile actually gained, and the mind wandering after the other sources with much uncertainty, but apparently not without some such foundations for truth as could be obtained from the wandering reports of men ignorant of latitudes and equators, of any mode of measuring distance, and of any necessity to be very particular in describing that which so little concerned them. In order to make the fountains of the Blue Nile serve for the whole Nile, the lake Bed, on a fourth map, is put very far to the south of Congo, from which place it flows into the Zambie. This seems to be one of Kircher's attempts at ingenuity, which are certainly very often extremely bold. On this map is drawn a lake near the situation of the Nyassi, with an outlet to the east.

On another map, which however is rougher, and intended to show the general form of the world, the Nile is brought from about 15 degrees south of the equator to a large lake-like widening beginning about 10 degrees north. A branch takes somewhat the direction of the Blue Nile.

In all these the existence of lakes may be considered by some as the result of the guidance of tradition or report.

The exultation of Pais when he discovers the fountains is not without interest. Although he gives but few of the proper class of details, he does his best, and such as a man with his class of knowledge might be expected to give. He says — "I saw with the greatest pleasure that which was denied to Cyrus king of Persia, and Cambyses, Alexander the Great, and the famous Julius Cæsar himself."

## MICROSCOPICAL SECTION.

### Ordinary Meeting, January 18th, 1864.

Professor WILLIAMSON, F.R.S., Vice-President of the Section, in the Chair.

Mr. GRINDON was elected an ordinary member of the Section.

The adjourned debate on Mr. Heys's Paper on the structure of the cotton fibre then commenced.

Professor WILLIAMSON did not agree with Mr. Heys as to the moniliform character of the fibre. He conceived it to be, like other cell structures, merely an elongated cell with its usual wall and contents. The moniliform appearance observed by Mr. Heys was owing to the hard starch or gum granules in the spiral contents of the ripe cell, preventing the collapse of the tube in those places where they existed on the drying of the cell, thus giving the appearance alluded to. He thought the only way to arrive at a correct conclusion was to examine the fibre in a growing state in all stages from its earliest appearance in the flower bud until its maturity. The *twist* of the fibre so often alluded to he considered to be owing entirely to the drying and consequent curling up of the cells, as in other vegetable structures. Professor Williamson strongly urged those members who had convenience, to grow a number of cotton plants and carefully examine the fibre in all its stages. Surely Manchester ought not to allow the question as to the structure of the cotton fibre to be discussed and settled elsewhere.

The President of the Section, Mr. Sidebotham, then read a paper on mounting objects for the microscope in fluid.

The paper referred chiefly to the mounting of Desmidiaceæ and other freshwater algæ. Mr. Sidebotham recommended a cell of japan black, the covering glass fastened on with gold size, then a coating of lac varnish, and afterwards several coatings of japan black, applied each when the previous coating was dry.

Mr. Sidebotham exhibited Desmidiaceæ, &c., mounted by himself in the years 1842 to 1847, in various fluids, and concluded that for this class of objects water was the best medium, the specimens mounted in 1842 being as perfect in structure and colour as when first prepared. Many specimens had become spoiled owing to the air penetrating into the cells, from the cracking of the varnish, but with care and a coating of lac varnish before the japan black he thought this would in future be of rare occurrence.

Mr. Sidebotham then explained a plan of mounting diatomaceæ in balsam, so as to preserve their forms and colours, and also keep the frustules attached to each other.

Professor Williamson corroborated Mr. Sidebotham's experience as to water being the best medium for the Desmidiaceæ; those specimens mounted by himself more than twenty years ago, where the fluid had been preserved, were as perfect as when first prepared.

Mr. Dancer mentioned that he had specimens of *Volvox globator* mounted in water in the year 1843 by himself, quite perfect in form and colour; he preferred gold size as a material for cells to japan black.

Mr. Nevill exhibited the larva of a gnat mounted in water four years ago, showing that this mode of preparation is applicable to animal as well as vegetable structures.

## Ordinary Meeting, February 9th, 1864.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

George Harris, Esq., Barrister-at-Law, was elected an Ordinary Member.

Professor Roscoe exhibited the light emitted by burning a portion of a fine specimen of pure magnesium wire 1 mm. in diameter and 10 feet long, which had been manufactured by Mr. Sondstadt. Professor Roscoe remarked that it afforded him great pleasure to be able to state that a suggestion made by Professor Bunsen and himself in their photochemical researches, and printed in the Philosophical Transactions for 1859, page 922, was about to be practically adopted. Mr. Sondstadt is now commencing to manufacture the metal magnesium on the large scale, and the first important application of the metal is the employment of burning magnesium wire as an illuminating agent, especially for photographic purposes. In the researches above mentioned Professor Bunsen and the speaker had examined the photochemical action of the sun compared with that of a terrestrial source of light, and for the purpose of this comparison they chose the light evolved by the combustion of magnesium wire. They showed that a burning surface of magnesium wire which, seen from a point at the sea's level, has an apparent magnitude equal to that of the sun, effects on that point the same chemical action as the sun would do when shining from a cloudless sky at a height of 9° 53′ above the horizon. On comparing the *chemical* with the *visible* brightness of these two sources of light it was found that the brightness of the sun's disc as measured by the eye when the sun's zenith distance was 67° 22′, is 524·7 times as great as that of the burning magnesium wire, whilst, at the same zenith-distance, the *chemical* brightness of the sun is only 36·6 times as great. Hence the value of this light as a source of the chemically active rays for photographic purposes becomes at once apparent. The extract from the memoir referred to is as follows: —

"The steady and equable light evolved by magnesium wire burning in the air, and the immense chemical action thus produced, render this source of light valuable as a mple means of obtaining a given amount of illumination pressed in terms of our measurement of light. . . . . . . . t combustion of magnesium constitutes so definite and simple a source of light for the purpose of photochemical easurement, that the wide distribution of this metal becomes desirable. The application of this metal as a source of light may even become of technical importance. A burning magnesium wire of the thickness 0·297 millimetre evolves, according to a measurement we have made, as much light as 74 stearine candles of which 5 go to the pound. If this light lasted one minute, 0·987 metre of wire, weighing 0·1204 grm., would be burnt. In order to produce a light equal to 74 candles burning for 10 hours, whereby about 20lbs. of stearine is consumed, 72·2 grms. of magnesium would be required. The magnesium wire can be easily prepared by forcing out the metal from a heated steel press having a fine opening at bottom; this wire might be rolled up in coils on a spindle, which could be made to revolve by clockwork, and thus the end of the wire, guided by passing through a groove or between rollers, could be continually pushed forward into a gas or spirit-lamp flame in which it would burn."

Professor Roscoe stated that great credit was due to Mr. Sondstadt for the able manner in which he had brought the difficult subject of the metallurgy of magnesium into the present satisfactory position, and expressed his opinion that, even for photographic purposes, the application of the metal will prove most important. Mr. Brothers, Mr. Parry, and other photographers present, corroborated Dr. Roscoe's opinion respecting the value of such a source of light for photography. Since the meeting Mr. Brothers made an experiment upon the magnesium light, which he reports as follows:—

"The result of an experiment I have just tried is that in 50 seconds with the magnesium light I have obtained a good negative copy of an engraving—the copy being made in a darkened room. Another copy was made in the usual way in daylight, and in 50 seconds the result was about equal to the negative taken by the artificial light. The sun was shining, but there was a good deal of fog in the atmosphere."

A paper was read, entitled, "On the Tensile Strength of Cotton, as effected by various Chemical Treatments." By Mr. Charles O'Neill, F.C.S.

The author has given a great number of experiments upon the subject, made in order to elucidate some important practical and scientific points not hitherto much worked upon. By means of his apparatus for testing tensile strengths (Proceedings of this Society, No. 6, 1863-64, p. 186) he was enabled to obtain data which by any previously known method could only have been obtained, if at all, by an incredible amount of labour.

*Effect of bleaching upon strength of cotton.* These experiments were made upon printing cloth of 18 threads to the quarter inch, American cotton, and bleached for printing by the low pressure process. The process included, among other treatments,—

(1) Passing three times over a red hot copper plate.

(2) Boiling 16 hours with milk of lime.

(3) Boiling 16 hours with soda and rosin.

(4) Steeping [illegible] n of bleaching powder for several hours.

(5) Steeping in di[illegible]ydrochloric acid for several hours.

| | WARP. | WEFT. |
|---|---|---|
| Strength of thr[illegible] pressed [illegible]eads in the grey cloth S.[illegible] (mean of 30 experiments) ... | 3140 grs. | 1714 grs. |
| Ditto in the bleached cloth S.V. (mean of 30 experiments)............ | 2920 grs. | 2785 grs. |

The warp threads from two other pieces of cloth give as follow, being the means of 40 experiments:—

| | A. | B. |
|---|---|---|
| In the grey state ........................ | 3407 grs. | 3512 grs. |
| In the bleached state .................. | 3708 grs. | 4025 grs. |

The cloth S.V. became elongated in the bleaching process, and contracted in width. The contraction in width, owing to the fulling up of the weft, will explain the increase of its strength, while the elongation may explain the diminution of strength in the warp. The cloth A., and also B., were chemically treated exactly as S.V., but were washed and dried loose, and not being fulled so much by the mechanical processes did not sensibly alter in length. The increased strength may be explained by the complete bedding of the cotton hairs, so forming a more compact thread. At any rate, it seems proved that the strength of cotton is not injured by the ordinary process of bleaching for printing.

*Effect of printing, dyeing, soaping, &c.* A portion of S.V. was printed, dyed, and finished as a first-class madder purple, and 20 experiments made upon the warp and weft threads, with the following results:—

| | |
|---|---|
| Strength of printed and finished warp ...... | 3569 |
| Ditto of printed and finished weft ............ | 2669 |

Here it is seen the warp threads have gained more than they lost in bleaching, while the weft threads have lost something. The increase of strength in the warp threads is partly, if not wholly explained by the diminution of length, the two yards gained in bleaching, and rather more, being absorbed in the processes of dyeing, soaping, &c., and the thread consequently becomes thicker and stronger.

*Mordanted cloth treated with acid.* These experiments are interesting, as touching upon the chemical *versus* the physical theory of dyeing. A piece of calico was chosen printed by blocks in wide longitudinal stripes, with the usual mordants for madder or garancine dyeing; it was aged, cleared, &c., as usual, to remove all loose mordant. Portions containing mordanted and unmordanted parts were treated by hydrochloric acid to remove the mordants, then carefully washed, and the strength of the threads tested. The results are as follow:—

| | |
|---|---|
| Iron mordant for black, warp threads, 10 experiments.............................. | 3450 grs. |
| Warp threads contiguous to the mordanted threads, 10 experiments.......................... | 3715 grs. |

Same mordant, weft threads, 6 experiments... 2201 grs.

Same weft threads in the unmordanted parts, 6 experiments........................................ 2906 grs.

The above experiments show a decided diminution of strength in those threads which had received the mordant. The unmordanted threads had, of course, been submitted to the same acid and other treatments.

A repetition of these experiments upon another piece of cloth of different origin but similarly printed, and which had been dyed in madder and then treated with hydrochloric acid, gave the following results:—

Alumina mordant for red, warp threads, 10 experiments........................................ 1906 grs.

Unmordanted threads, contiguous, 10 experiments ........................................ 2031 grs.

showing a diminution of strength.

Iron mordant for black, warp threads, 10 experiments........................................ 1631 grs.

Unmordanted threads, contiguous, 10 experiments........................................ 2260 grs.

showing a considerable diminution of strength.

*Experiments on cotton hairs, taken from mordanted and unmordanted threads, which had been treated by acid.*

Alumina mordant for red, 8 experiments ...... 60·3 grs.

Not mordanted adjacent hairs, 5 experiments. 71·6 grs.

The uncertainty as to whether the mordant had actually touched the hairs in the centre of the threads caused these experiments to be discontinued and the following instituted.

*Cotton mordanted in the wool.* Small parcels of New Orleans cotton were steeped separately in ordinary iron liquor (crude pyrolignate of iron) and red liquor (crude acetate of alumina), saturated with these liquors, the excess expressed out, and then dried gently, and aged for 24 hours; afterwards well washed in lukewarm water and dried. A portion of each parcel was then treated with dilute hydrochloric acid, some of the original stock of New Orleans cotton being placed in the same acid, and going through the same treatments. The mordants being dissolved out, the cotton was well washed to free it from the acid, dried, and its strength ascertained with the following results:—

| | |
|---|---|
| New Orleans Cotton not mordanted, 10 experiments | 143·9 grs. |
| Ditto mordanted with iron, 10 experiments | 96·8 grs. |
| Ditto mordanted with alumina, 10 experiments | 94·1 grs. |

A very notable diminution of strength has occurred in the mordanted parcels.

*Gun cotton.* The only undeniable chemical compound which cotton forms with other elements, without undergoing conspicuous physical change, is in gun cotton. A sample of New Orleans was treated with equal volumes of concentrated sulphuric and nitric acids; it increased in weight 66 per cent, burned well, and was soluble in alcoholised ether. Twenty hairs being measured gave a mean length of 0·997 inch; twenty hairs before treating gave a mean length of 0·996 inch. The difference 0·001 being within the range of error, it may be said not to have changed in length. The strength of 10 hairs was taken, and gave a mean of 85·4 grs. The mean strength of 19 hairs before treatment being 138·1, there has been a considerable diminution of strength. This may not be true of all kinds of gun cotton.

*Mercerised Cotton.* It is very well known that cotton cloth treated with concentrated solution of caustic soda shrinks in length and becomes stronger. The following experiments corroborate this. New Orleans cotton was treated with soda solution sp. gr. 1·250. Twenty hairs being measured gave a mean length of 0·857 inch, being a contraction of 0·139 inch on the mean of twenty hairs measured before treatment. The mean strength of 10 hairs is 154·1 grains against a mean strength of 138·1 grains before treatment.

The reliability of these results is discussed by the author. The experiments in detail show wide discrepancies, and the maximum and minimum of a series are often at a considerable distance from the mean. This is a difficulty inherent to the subject, and can only be overcome or lessened by multiplying the experiments. Ten experiments seem to give a reliable mean, for in making twenty or thirty, the first, second, and third tens give nearly the same mean. Most of the results are means of twenty experiments; and several of them having been repeated at long intervals and by different hands, without any important difference in the means, the author is of opinion that they express the truth, at the same time that they may be open to some numerical rectification.

## Ordinary Meeting, February 23rd, 1864.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Walter Crum, F.R.S., was elected an Honorary Member of the Society.

The formation of a Photographic Section was announced. On the motion of Mr. Sidebotham, seconded by Mr. Parry, it was unanimously resolved that the Sections of the Society, with the consent of the Council, shall have power to elect sectional associates, subject to rules which have been drawn up by the Council. The arrangement to take place immediately, and to continue in force until the end of the ensuing Session.

Professor **Roscoe** stated that the question of the possibility of taking photographic portraits by means of the magnesium light was now satisfactorily settled; he exhibited some prints of a portrait which Mr. Brothers and he had taken at five o'clock p.m. on Monday the 22nd, by burning fifteen grains of magnesium in the form of fine wire, at a distance of about eight feet from the sitter. The negative thus produced was stated by Mr. Brothers to be fully equal to any obtained by sunlight in the most favourable state of the atmosphere, the distribution of light and shade was most agreeable, any harshness of the shadows being completely removed simply by slightly moving the wire whilst it is burning. During the meeting, Mr. Brothers took an excellent negative copy of Chantrey's fine bust of the late Dr. W. Henry, in the possession of the society, by burning ten grains of magnesium wire, the light lasting for 59 seconds. It is expected that the quantity of wire necessary for taking a photographic portrait can be sold at the cost of a very few pence.

Mr. G. C. Lowe described a meteor seen by him on Sunday, Feb. 7th, at 6h. 11m. p.m. Greenwich mean time. It was first seen just below the constellation Cassiopeia, and was in full view throughout its entire passage, which occupied about four seconds. It descended towards the horizon at an angle of about 40°, passing between the two stars $\eta$ Pegasi and Scheat, and disappeared without breaking up when at the altitude of $\alpha$ Pegasi. It was of an elongated pear shape, and was followed by a short train of sparks of a dull red colour, the meteor itself being a very pale blue, somewhat less than a diameter of the moon in apparent length, and about six or eight times brighter than the planet Jupiter.

This meteor was also seen by Mr. Wylde, Mr. Parry, and Mr. Pochin, who all described it as one of great brilliancy.

Mr. Sidebotham exhibited a copy by photo-lithography of one of the earliest editions of Shakespere, which he believed to be the first published work in which that process was employed.

Mr. Brockbank exhibited a bead-like fossil body which had been found in a sandstone near Stainmoor, and had been traced to full four feet in length.

The President said that a similar fossil had been described by Mr. George Tate, F.G.S., of Alnwick, in the Transactions of the Berwickshire Naturalists' Club for 1858, under the name *Eione Moniliformis*, which was found in the sandstones of the mountain limestone of Howick, Scremerston, and Haltwhistle, in Northumberland and in Yorkshire. Mr. Hindson of Kirby Lonsdale, has also found the same fossil in sandstones of a similar age in that district, and probably they would be met with in the beds of the millstone grit further south, at Dyke Nook, near Keighley, and Saltersbrook, near Woodhead, where he (the president) had observed specimens of several species of M. Loy's genus of *Crassopodia* with which they have been found associated.

Dr. Roscoe read a Paper by Mr. Edward Sondstadt, entitled, "Note on the Preparation of Calcium."

Although Davy demonstrated the existence of Calcium, and obtained it in an impure condition, by his well known method more than half a century ago, there are but two methods at present known whereby this most abundant of all the metals, excepting perhaps aluminium, can be obtained in a state of comparative purity. Matthiessen, following a method which was, I think, first indicated by Bunsen, who obtained magnesium by the electrolysis of the fused chloride of magnesium, obtained calcium by the electrolysis of a mixture of the fused chlorides of calcium and of strontium. This method, however, of obtaining the metals of lime and of magnesia is exceedingly troublesome, principally because of the floating up and burning of the metal on the surface of the salt electrolysed. The second of the two methods referred to is that adopted by Liès Bodart, and Gobin, who obtained calcium by heating iodide of calcium with sodium in an iron crucible, the cover of which was securely fastened down. The only objection to be made to this process is the troublesomeness and expense of preparing anhydrous iodide of calcium, which, like chloride of magnesium, is apt to undergo partial decomposition during ignition—and it must be remembered that partial decomposition of iodide of calcium involves the formation of lime, a substance practically infusible, which, during the reaction with sodium, must prevent, if present, the aggregation of the minute particles of reduced calcium into globules. In order to overcome this, and the before-named objection to the process, the author fuses together equivalent quantities of iodide of potassium and of chloride of calcium. The fused mass is poured into an iron crucible and covered till cool enough to handle; the mass is then dropped out, and a rather less than an equivalent quantity of sodium is put into the crucible, and the mixture of calcium and potassium salts is placed above it. The crucible is then closely covered, and heated to redness. The heat need neither be strong nor long continued. The best results are obtained when the crucible cover is fastened down, but calcium in lump may be obtained without using more pressure than that afforded by a well fitting lid. The reaction does not appear to be violent, and hence the advantage of considerable pressure. Is the slight violence of the reaction between sodium and the calcium salt owing to the near approach of the two metals as to atomic weight, and thence specific heat? It is easier to obtain calcium in lump by the modification of Liès Bodart's process, that I have described, in a small way, than it is to obtain magnesium in lump on a like scale.

A paper was read by J. C. DYER, V.P., "On the Nature of Friction in Mechanics," in which he stated that:—

Resistance to motion by surface contact arises from distinct kinds of obstruction, according to the nature of the moving bodies and to the condition of their surfaces. These present problems that have been investigated by many writers and experimenters; but no fixed law or rule has been discovered for measuring such resistances as applied to different kinds of friction. The inquiries have mostly related to the action of solid surfaces, and some approach to the measure of their resistances has been attained, by finding the relation of falling weights to those sliding over horizontal surfaces of different kinds of woods, metals, glass, marbles, &c.; but little or nothing has been accomplished towards showing the nature of the surface resistance to solids moving through water or other liquids. The experiments of Captain Beaufort and Robert Fulton afford data for the sum of such resistance, but not for its mode of action.

In the case of solids, if the surfaces in contact were perfectly level and hard, they would slide over each other without friction; but as none such are known, their resistance to motion arises from the projecting points and indentations, which grapple with more or less force, causing the moving body to rise and fall and to abrade and wear down the surfaces; thus gravity and cohesion constitute the counteracting or compound force called *friction*. In railway wheels, as small portions of the peripheries are in contact with the rails, when both are in good order but slight resistance is offered; but even in this case the common measure of the weight into the distance does not strictly apply, and the amount of tractive ower is obtained from experience only. This retarding force too is dividedbetween the friction on the rails and that of the air passed through. The action of rolling surfaces such as wheels and pinions of watches, clocks, and other machinery, is amply explained, and rules given for minimum friction, in the works of Berthoud, Camming, and Halton, on clocks and watches, by Camus on the teeth of wheels, and in D. Fairbairn's work on mill-gearing, wherein we see that if the rolling surfaces were all smooth, hard, and of curvatures adapted to such motion, they would offer no friction.

The motions of shafts on fixed bearings, if the metal surfaces come in contact, will draw in and compress the air, and by heating the shaft prevent safe working; hence lubricating matter is used to limit the contact of the metals, so that only some of their prominent parts touch, to produce attrition, or friction.

The motion of bodies through water involves several kinds of resistance of a more complex nature, the measure of which has not been given by any of the able writers on that subject, nor has any formula been applied thereto for solving the questions of the compound resistances to be overcome by ships and other bodies moving through water. Some authors have taken the measure of this resistance to be as the squares of the velocities; but this fails because the different kinds of resistance are not alike called into action in the different cases adduced. Those of cannon balls ricocheting from water, the sinking of plummets in deep soundings, and floating bodies at different depths from the surface, present each of them reacting forces too complex to admit of any simple rule of measurement, the separate nature of which need not be here repeated; but it may be said that the aggregate of the resisting forces called friction must of necessity consist of the reaction of gravity, inertia, cohesion, elasticity, and adhesion of bodies, in their varied degrees of action, so that no simple measure of them as *friction* is likely to be discovered, and we thus see that friction is a compound term, pointing to the action of several retarding forces, and to comprehend the nature of these, we must analyse these natural forces as they are respectively called into action under the common but vague term of friction.

## MICROSCOPICAL SECTION.

Ordinary Meeting, Feb. 16, 1864.

**Joseph Sidebotham**, Esq., President of the Section, in the chair.

Mr. **Brothers** presented some very beautiful photographs from microscopic objects to the Section.

Mr. **Lynde** mentioned that one of the members, Mr. Grindon, had promised to procure a supply of fresh cotton plants, in all stages of growth, beginning in June, and suggested that, as the time for their examination would be during the recess, a Committee should be appointed to examine the subject of the cotton fibre very carefully, and report thereon at the first meeting in the next Winter Session.

Mr. **Sidebotham** suggested that in using very high powers, such as the 1-25th, if slips of mica were used for mounting the objects, together with mica covers, there would be no risk of damage to these lenses, which could thus be used in many examinations where the fear of damage now prevents their employment.

Mr. **Dancer** exhibited a new and improved Oxycalcium Microscope; the lenses were adapted to the ordinary form of lantern, and consisted of new combinations of lenses, giving a large flat field, and plenty of light, along with very fine definition.—The room was darkened, and Mr. Dancer exhibited an extensive range of objects with several powers, on a large stretched paper screen, 8ft. by 5ft., showing the application of this instrument to the uses of lectures on various subjects. Besides the usual objects, such as sections of wood, objects mounted in fluid, living animalculæ, the process of crystalisation, electrical action, &c., were exhibited.

A new combination forming a one-inch object glass for the achromatic microscope was also exhibited by Mr. Dancer, and was much admired.

## Ordinary Meeting, March 8th, 1864.

J. C. Dyer, Esq., Vice President, in the chair,

Horatio Micholls, Esq. and Thos. Windsor, M.R.C.S., were elected ordinary members of the society.

Mr. Brothers presented to the members several prints from a negative photograph of Slater's fine bust of the late Professor Hodgkinson, taken by the magnesium light.

The following communication from Dr. Fairbairn was read:—"I beg to forward, for the information of the society, some singularly corroded plates, taken from two boilers that exploded and killed thirteen persons a few days since, at Aberdare, South Wales. The extraordinary effects are supposed to arise from the water, into which some mineral springs from the mines have been flowing for some time past. Mr. Fletcher our engineer, has submitted this water for analysis to Dr. Angus Smith; to whom I beg to refer. I doubt not the specimens will be interesting to the society."

The plates appeared honeycombed on the interior surface to the depth of half the thickness of the plate, and the corrosion evidently proceeded from chemical action.

Mr. Fletcher stated that the corrosion was very irregular; in some portions, especially close to a seam of rivets, the plate being completely eaten away; the part of the boiler situated immediately over the furnace was that upon which the corroding action was most noticed.

Mr. Spence drew the attention of the society to the great importance of the question of the action of acid water, especially in this district, upon boilers; he employs the water from the Rochdale Canal in his boilers, and unless he were regularly to use soda to neutralise the acid, his boilers would undergo constant corrosion. By the addition of one pound and a half of soda per diem to each boiler, the plates were found to remain perfectly uninjured.

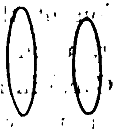
Mr. Nasmyth communicated the following letter he had received from E. J. Stone, M.A., First Assistant at the Royal Observatory, Greenwich:—

"Royal Observatory, Greenwich,
"London, Feb. 25, 1864.

"Dear Sir,

"The Astronomer Royal has placed your letter of the 20th February in my hands.

"Your discovery of the 'willow leaves' on the solar photosphere having been brought forward at one of the late meetings of the Royal Astronomical Society, my attention was attracted to the subject. At my request the Astronomer Royal ordered of Mr. J. Simms a reflecting eyepiece for our great equatorial. The eyepiece was completed about the end of January last, and at the first good opportunity I turned the telescope on the sun. I may state that my impression was, and it appears to have been the impression of several of the assistants here, that the willow leaves stood out dark against the luminous photosphere. On looking at the sun I was at once struck with the apparent resolvability of its mottled appearance. The whole disc, so far as I examined, appeared to be covered over with relatively bright rice-like particles, and the mottled appearance seemed to be produced by the interlacing of these particles. I could not observe any particular arrangement of the particles, but they appeared to be more numerous in some parts than in others. I have used the words rice-like particles merely to convey a rough impression of their form; I consider them like the fig.



"I have seen these rice-like particles on two occasions since, but not so well as on the first day, when the definition was exceedingly good.

"Yesterday (Feb. 24) I saw them for a few minutes, but with great difficulty. I use the full aperture, 12¾ inches, and a low power.

"I am, dear Sir,
"Yours very truly,
"E. J. Stone.

"J. Nasmyth, Esq.

"P.S.—On the first day I saw them I called Mr. Dunkin's attention to them. He appears to have seen them, and considers the fig. above to represent them fairly. He says, however, that he should not have noticed them if his attention had not been called to them."

Mr. Baxendell, F.R.A.S., read a paper "On Periodic Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface."

Considerations arising out of an investigation of the irregularities which take place in the changes of some of the variable stars led the author, some time ago, to regard it as highly probable that the light of the sun, and also its magnetic and heating powers, might be subject to changes of a more complicated nature than has been hitherto supposed, and that, besides the changes which are indicated by the greater or less frequency of solar spots, other changes of a minor character, and occurring in shorter periods, might also take place. In the hope of detecting these supposed changes the author resolved to undertake the discussion of a series of magnetical observations, and for this purpose he selected the observations made at the Imperial Observatory of St. Petersburg, the most northern station at which hourly magnetical observations have been made for any lengthened period. Commencing, therefore, with the year 1848, the greatest and least values of the magnetic declination for every day were extracted from the observations, and taking the differences and arranging them in order, it was found, on a careful examination, that they indicated changes of activity taking place in a period of 31 days. The daily oscillations were then arranged in a table of 31 columns, and taking the means of the numbers in these columns, their differences from the general mean for the year were found to be as follow, the unit of value being one division of the scale of the magnetometer, or 26·3″ of arc:—

| Day. | | Diff. from General Mean. pts. | Day. | | Diff. from General Mean. pts. | Day. | | Diff. from General Mean pts. | Day. | | Diff. from General Mean. pts. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | — | 8·18 | 9 | — | 7·86 | 17 | + | 0·49 | 25 | — | 1·11 |
| 2 | — | 8·25 | 10 | — | 7·99 | 18 | + | 6·35 | 26 | — | 2·38 |
| 3 | — | 3·32 | 11 | — | 0·70 | 19 | — | 3·05 | 27 | — | 1·43 |
| 4 | + | 1·18 | 12 | + | 7·45 | 20 | + | 2.65 | 28 | + | 8·41 |
| 5 | — | 4·80 | 13 | + | 14·83 | 21 | + | 4·21 | 29 | — | 6·51 |
| 6 | — | 0·21 | 14 | + | 9·22 | 22 | + | 5.80 | 30 | — | 6·51 |
| 7 | — | 1·52 | 15 | + | 1·19 | 23 | + | 8·39 | 31 | — | 5·69 |
| 8 | — | 6·66 | 16 | + | 5·99 | 24 | + | 5·08 | | | |

A projection of these numbers shows that of the *seventeen* consecutive days, 12 to 28, the amount of oscillation, or range of the magnetic needle, was *above* the mean value on *thirteen* days, and *below* the mean on only *four* days; while of the remaining *fourteen* days the range was *below* the mean on *thirteen* days, and above on *one* day only. The total amount of the differences for the seventeen days of maximum was 67·09, or 3·95 per day, and for the fourteen days of minimum it was 67·02, or 4·78 per day, the mean for the year being 39·86. The ratios of the average excess of a day of maximum, and of the average deficiency of a day of minimum to the mean value, are therefore as 1 to 10·09, and as 1 to 8.34.

On proceeding to examine the observations for the succeeding years it was found that they could not be represented by a period of 31 days. It appeared, therefore, at first sight that the period which had been obtained for 1848 was merely accidental; but, guided partly by conclusions drawn from his variable star investigations, and partly by the high degree of improbability that the results for 1848 could be due to mere accident, the Author was led to think that the period he had found for 1848 might be variable, gradually diminishing for a series of years, and afterwards gradually increasing, to diminish again when it had completed its cycle of change. Assuming, therefore, that in every year periodic changes took place in the magnetic activity of the sun, the Author proceeded to determine for each year the most probable approximate value of the period, and he obtained a series of values gradually diminishing till 1856, when the period was only about 23 days, and afterwards rapidly increasing, until in 1859, it amounted to about 32 days. A glance at these results at once suggested the idea that the variable period thus found was in some way connected with, and dependent upon, the great solar spot period, the minimum value occurring in the year of minimum frequency of the solar spots, and the maximum values in the years when the spots were most numerous.

Several series of thermometrical observations were now examined for indications of periodical changes in the element of mean daily temperature, and it was found that they exhibited, with unexpected distinctness, changes in this element occurring also in a variable period, the range of variation being, however, somewhat less than in the case of the magnetic element, although the times of maximum and minimum were almost exactly the same. The maximum and minimum values were respectively 31 and 23½ days.

A table is given showing the number of days included in maximum and the minimum portions of each mean period for the years 1848 to 1859, and the number of exceptional days, or those on which during the maximum part of the period the temperature was *below*, and during the minimum part *above*, the mean value. From this table it appears that out of a total number of 165 days of maximum only 14 were exceptional; and out of a total of 164½ days of minimum the number of exceptional days was only 16. The mean gives a ratio almost exactly as 1 to 11. Considering that the values of the period in the different years are only approximate, this result may be regarded as affording satisfactory proof of the existence of a variable period of temperature; but a comparison of the total amount of the differences of temperature from the mean, and the amount of exceptional differences which is given in another table, is much more striking and conclusive. From this comparison it appears that against a total amount of 255·61° of *plus* differences on maximum days, there were only 8·17° of *minus* differences; and against 258·61° of *minus* differences on minimum days there were only 11·79° of *plus* differences, the mean ratio of the amount of exceptional differences to the total amount being therefore as 1 to 25·7.

At St. Petersburg the average temperature of the warmer half of the period is not less than 3° greater than that of the cooler half, and as this difference of temperature is repeated at least twelve times in every year, it must necessarily exercise a powerful modifying influence over many meteorological phenomena.

With reference to the differences between the maximum and minimum values of the magnetic and the corresponding values of the temperature period, it is remarked that owing to occasional interruptions of the magnetical observations and to the enormous extent of the oscillations of the needle on particular days, the values of the magnetic period given in the paper may require some correction; still it is believed that the ranges of the two periods are not identical.

Another period of change having a mean duration of rather over eighteen months is then referred to. The Author was first led to it from a discussion of the Greenwich magnetical observations for the years 1848 to 1859; and it has been confirmed by the results of a discussion of temperature observations made at Brussels, in Europe, and at Yakoutsk, in Asia. It is obvious that this period will at times interfere sensibly with the shorter one, and it is probable that some of the cases which have been called exceptional may be due to this interference.

As it may perhaps excite surprise that in this investigation no use has been made of the fine series of observations taken at the British Colonial Observatories, it is remarked that a little consideration will serve to show that in the early stages, at least, of an inquiry like the present, little or no reliance could be placed on results derived from series of observations in which every seventh day, or fifty-two days in the year, were complete blanks. It has, in fact, been found that the omission of a single day in a year will in some cases produce a very sensible effect upon the final results for the magnetic period. Hence it is that the Author regards some of the values he has obtained for this period as being open to correction when he may have an opportunity of discussing more complete sets of observations. General Sabine, in his very elaborate discussions of the magnetical observations made at the colonial observatories, separates the larger movements of the needle from the general mass, and treats them as extraordinary disturbances, assuming apparently that the two classes of ordinary and extraordinary disturbances are due to different causes. The Author has, however, not ventured to adopt this mode of proceeding, but has preferred to regard all the movements of the magnet, whether large or small, as having a common origin, and his discovery of the temperature period, by acting upon this view, may be regarded as affording strong evidence in its favour.

With regard to the probable cause of the variability of the short period, the author remarks that the subject is one of great difficulty, for, while the facts seem clearly to belong to the domain of astronomical science, he has found it impossible to frame any hypothesis to account for them without calling in the aid of some principle which has not hitherto been applied to the explanation of astronomical phenomena. It is therefore not without considerable hesitation that he ventures to observe that the facts would perhaps be best explained by supposing—

1st. That a ring of nebulous matter exists differing in density or constitution in different parts, or several masses of such matter forming a discontinuous ring, circulating round the sun in a plane nearly coincident with the plane of the ecliptic, and at a mean distance from the sun of about one-sixth of the radius of the earth's orbit.

2nd. That the attractive force of the sun on the matter of this ring is alternately increased and diminished by the operation of the forces which produce the solar spots, being greatest at the times of minimum solar spot frequency, and least when the spots are most numerous.

3rd. The attractive force being variable, the dimensions of the ring and its period of revolution round the sun will also vary, their maximum and minimum values occurring respectively at the times of maximum and minimum solar spot frequency.

In reference to the nature of the varying attractive force, it is not improbable that the matter of the supposed ring may be highly diamagnetic, and being much nearer to the sun than any of the known planets, of much greater bulk and lightness, and being subjected to a much higher temperature, it will be very sensibly affected by the changes which take place in the magnetic condition of the sun, and when interposed between the earth and the sun, it may act not only by reflecting and absorbing a portion of the light and heat which would otherwise reach the earth, but also by altering the direction of the lines of magnetic force. The changes of temperature at the surface of the earth will thus be due partly to differences in the amount of heat received from the sun, and partly to changes in the movements of the great currents of the air produced by alterations in the earth's magnetic condition. If the larger part of the difference of temperature is due to the latter mode of action, we might expect that during the warmer half of the period, the mean direction of the wind at any given station would be sensibly different from that during the cooler half; and also, that the epochs of maximum and minimum temperature would not be the same at all parts of the earth's surface. Both of these conclusions are borne out by the results given in the paper. Thus at St. Petersburg in 1859 the mean direction of the wind on maximum days was S. 54° W., and on minimum days S. 73° W. at 19° more to the west of South; and at Sitka, on the north west coast of North America, in 1851, the mean direction on maximum days was S. 32° W., and on minimum days S. 56° W., the difference being 24°. As striking instances of the differences in the epochs at distant stations, it may be stated that in 1859 the epoch of maximum at St. Petersburg corresponded precisely with the epoch of minimum at Madras; and that at Pekin, in 1851, the epoch of minimum was exactly coincident with the epoch of maximum at Sitka.

Changes in the amount of heat received from the sun sufficient to produce the variations of temperature observed at any given station would no doubt affect the movements of the great currents of the atmosphere, though not to the extent indicated by the observations; but it is difficult to conceive that they could produce the differences in the epochs which are found to take place. We may therefore fairly conclude that the action of the supposed ring of nebulous matter is principally of a magnetic, and but slightly of a thermal character.

It is suggested that the greater range of variation of the magnetic as compared with the temperature period may be due to the inertia and elasticity of the great currents of air, the inertia tending to lengthen the temperature period and the elasticity to shorten it; but as the inertia will act with greatest effect when the magnetic period is at its minimum, while on the other hand the elasticity will be most effective when this period is at its maximum, the result will be a range of variation of the temperature period, somewhat less than that of the magnetic.

Adoping for the present the maximum and minimum values of the temperature period as being determined with greater accuracy than those of the magnetic period, the greatest and least values of the sidereal period of revolution of the ring will be 29·12 and 22·08 days respectively. From these numbers we find that the greatest distance of the ring from the sun is 0.185, the radius of the earth's orbit being taken as unity; the least distance 0.154; and the mean 0·169. Taking Mr. Hind's value of the mean distance of the earth from the sun, namely, 91,328,600 miles, we have

Greatest distance of the ring = 16,921,000 miles.
Least „ „ = 14,068,000 „
Mean „ „ = 15,494,500 „

and the range of movement to and fro in a radial direction = 2,853,000 miles. The greatest attractive force of the sun on the ring being taken as unity, the least will be 0·691. The difference is therefore nearly one-third of the maximum amount. It will be evident that this difference may be regarded as a measure of the forces which are concerned in the production of the solar spots.

The results of the elaborate investigations of the motions of the planet Mercury, made by M. Leverrier, led that accomplished mathematician to attribute a certain unexplained excess in the motion of its perihelion to the action of a disturbing body circulating round the sun within the orbit of Mercury; and from a discussion of the probable mass of the disturbing body he concluded that it could not be concentrated in a single planet, and that it consisted of a ring of small bodies similar to that which is known to exist between the orbits of Mars and Jupiter; and it is remarkable that the mean distance, which he seemed to regard as the most probable, is precisely that which the Author has found for the ring of nebulous matter whose existence he has assumed to account for the phenomena described in his paper. This unexpected and unlooked for agreement between results arrived at from considerations and by methods so totally different, seems to establish the existence of this ring with quite as much certainty as the results of the profound researches of Adams and Leverrier established the existence of Neptune before that planet had been actually seen. This ring, however, owing to its proximity to the sun, may never be seen, and, like the dark companions of Procyon and Sirius, it may only be known to us through its action on the other bodies of the system of which it forms a part Should future researches place its existence beyond doubt, this will, it is believed, be the first instance in which the conclusions of physical astronomy have been confirmed by the results of an investigation of magnetical and meteorological phenomena. Whether, however, the hypothesis which the Author has ventured to put forward be accepted or not, it is now very evident that observations of solar phenomena merit a much larger share of attention than has ever yet been devoted to them. It has long been suspected that the same causes which produce the spots on the sun's disc must in some way have an important influence on the phenomena of our own atmosphere. The facts now given convert this suspicion into a certainty; and it is perhaps not too much to say that meteorology can never take rank as a true science while our knowledge of the sun remains in its present imperfect state. Moreover, there is little doubt that many questions of high physical interest depend for their solution upon our obtaining a more intimate acquaintance than we yet possess with the operations which are going on in the great centre of our system. It is therefore much to be desired that some of the many observatories which are now established in various parts of the world should be specially devoted to observations of the sun, and solar phenomena generally; and that the principal magnetical and meteorological elements should be observed daily, without interruption, at all the regular magnetical and meteorological observatories.

It may be stated that the values of the variable temperature period, given in this paper, were derived from observations made at St. Petersburg, Wardoe, Gorki, Barnaoul, Irkoutzk, Nertchinsk, Yakoutsk, Pekin, Madras, Novo-Petrovsk, Lougan, Zurich, Geneva, Milan, Brussels, Greenwich, Jakobshavn in Greenland, and Sitka on the north-west coast of North America. The comparison of the variable temperature period with the solar spot period, extends over the twenty-seven years 1833–59, and therefore includes three maxima and three minima of solar spot frequency.

## Ordinary Meeting, March 22nd, 1864.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Messrs. Oliver Heywood, James Mudd, Leslie J. Montefiore, and C. A. Duval, were elected Ordinary Members of the Society.

Mr. HURST communicated the following letter from Capt. John Mitchell, Superintendent of the Madras Museum: —

Madras, 13th January, 1864.

To H. A. Hurst, Esq., 61, George Street, Manchester.

Dear Sir,

I have the pleasure to acknowledge the receipt of your letter of the 10th November, 1863.

There are very few microscopists here, and as I do not know any person who is likely to undertake the examination of cotton fibre in the various stages of its growth, I have resolved to do so myself, in the belief that the subject is one of sufficient importance to justify me in devoting one day in the week to the inquiry so long as may be found necessary.

I have accordingly made arrangements with Dr. Hunter, Honorary Secretary of the Madras Agrihorticultural Society (who at once promised me every assistance), to receive weekly on Saturday a few pods from the Society's grounds, where cotton of all kinds is growing.

I have already given four days to this inquiry, and although it is still in its infancy, I have obtained some interesting results, which I will at once briefly communicate.

I began the examination with pods that were supposed to have been just formed. In this, the earliest stage, I found the cotton hairs just becoming visible upon the surface of the seed as minute transparent hemispheres containing a few motionless granules—I should perhaps say translucent, for the cotton hairs do not seem ever to be transparent.

In a more advanced stage the seeds were covered with hairs which contained numerous minute granules floating in a very fluid and colourless mucus. An active rotation of the cell contents, exactly like that in Nitella, was seen in all the hairs that had not been injured by pressure, and continued for a considerable time, at least half an hour. I found it could be seen with Ross's half-inch and the higher eyepieces, but I used chiefly a $\frac{1}{8}$th.

In a pod apparently somewhat older the appearances only differed by the cell contents, which I have called colourless mucus above, becoming thickened, and the granules somewhat smaller, so that we had a fine granular mucus of a pale buff colour.

As the pod becomes older the cell contents appear to increase in density and rotation to cease; at least I have not seen rotation unless in the hairs of young seeds, *i.e.* seeds from young pods of perhaps from two or three to ten or twelve days' growth. I have not, unfortunately, been able to learn the exact age of the pods.

On Saturday last I plucked a fine pod of Queensland cotton (growing in the garden of a friend) that was supposed to be nearly full grown, and was upwards of two inches in vertical diameter. In the hairs of this I found generally, but not always, a considerable amount of secondary deposit, made evident by the thickening of the walls and by its action on polarised light. But in the hairs of younger pods there was nothing of the kind, and the walls were so thin as scarcely to afford evidence of their presence, it requiring considerable power to bring out the usual double contour line, and they had no action, singly, on polarised light, although they became a little luminous in a mass of many.

The growing cotton fibre is an elongated cone with a hemispherical apex, and, of course, a circular transverse section. Each hair is a single cell. I have sought in vain, with all powers and every kind of illumination that I thought likely to render it visible, for any section or transverse division in the hairs, and I have been equally unsuccessful in my search for spiral fibres, which Mr. O'Neill says he found in cotton by means of re-agents, and I believe I am justified in saying that spiral fibre did not exist in any cotton hairs hitherto examined by me. But I have yet to examine pods of a later growth, and spiral fibre may yet appear, but I must confess I do not expect it.

I have not seen any twist in growing fibre, and, notwithstanding the pressure to which the hairs are probably exposed, I have seen no flattening from this cause, but the hairs of course collapse and become flat when from any cause the cell contents are absent. From sections I have made and examined I believe that in the younger pods the hairs wind round the seeds; in the more advanced stages the hairs of neighbouring seeds intermingle, and this may account for the bent and twisted appearance of dry cotton, that is, in some degree; but the principal cause will doubtless be found in the desiccation of the cotton after it is exposed to the sun by the bursting of the capsule.

I must not omit to mention that when by pressure a portion of the contents is expelled from the cotton hairs, it frequently appears in the form of small spheres, in which an active molecular movement of granules is seen, just as in the mucous corpuscles from the mouth, from which, in appearance, they only differ in their larger size.

I have seen in some dry Sea Island cotton, with a $\frac{1}{12}$th and polarised light, what Mr. Sidebotham (was it he?)

took for spiral fibres, I presume; but they are only visible in places and not in all hairs. I confess that at present I am a sceptic on this point.

With apologies for this hasty letter,

I am, Dear Sir,
Your most obedient servant,
J. Mitchell, Captain,
Superintendent Madras Museum.

Professor Roscoe read the following extract from a letter which he had received from Professor Boettger, of Frankfort, respecting the occurrence of the salts of Cæsium, Rubidium and Thallium together, in the salt obtained by evaporating the mineral water of Nauheim, near Frankfort.

"The Nauheim salt is a highly valuable substance, as being perhaps the only material from which Cæsium can be obtained in quantity. According to my experiments, 1cwt. of this salt yields 1lb. of double chloride of Cæsium and Platinum, containing small quantities of the Rubidium and Thallium double salt. At my special request the "Kurfürstliche Salzamt" in Nauheim has arranged to sell this evaporated salt, packing included, at the exceedingly low rate of one thaler (3s.) per cwt. In my spectrum investigations I almost invariably employ a flame of hydrogen in place of the common Bunsen's lamp, as by this means I obtain a higher temperature, and the lines appear therefore more distinct. The spectrum of the triple platinum-chloride of Cæsium, Rubidium, and Thallium thus obtained exhibits in the first place the well-defined emerald green line of Thallium, soon afterwards the two brilliant blue Cæsium lines appear close together, then the very broad and ill-defined blue bands of Rubidium are seen, as well as the two narrow red lines characteristic of this metal.

"I have lately discovered the presence of Thallium not only in many other mineral waters, but also in the vegetable kingdom, in very minute, although perceptible quantities. Thus Thallium occurs in the ash or the charcoal from wine yeast, in molasses, tobacco, chicory root, &c. If four pounds of any of these substances are taken, a quantity of the double chloride of Platinum and Thallium is obtained sufficient for many experiments. According to my most recent experiments, Thallium occurs in the dust from the pyrites burners as Thallium-iron-alum. I have lately prepared this salt directly from the sulphates of Thallium and Iron; it is the most easily soluble of all the Thallium salts; it possesses the pale reddish amethystine colour of ammonia-iron-alum, it crystallizes like this salt in large regular octohedra, and contains 24 atoms of water. Sulphate of Thallium is isomorphous with sulphate of Potassium, and the fact of the formation of the above double salt is another proof of the close relationship between Thallium and the alkaline metals, independently of the fact that it almost invariably accompanies Potassium, and not unfrequently both Caesium and Rubidium.*

A Paper was read by J. C. DYER, Vice-President, entitled "Notes on Spinning Machines: Part 1; The Mule Jenny:" who stated that—

Two distinct principles were embraced in the inventions of James Hargreaves and Richard Arkwright, which were afterwards combined by Samuel Crompton, to form the beautiful power driven machine, called the Mule. Arkwright employed the throstle, or throated spindle, with arms or "flyers," to conduct the threads on bobbins, arranged in stationary frames; Hargreaves employed naked spindles arranged on a traversing frame or carriage, by which the threads were drawn out (about 5 feet) in horizontal lines, whilst being twisted, and were then taken up, or wound, on the spindles, to form "cops," whilst the carriage returned to the roller beam for another "stretch." This process gives to the mule twist, the peculiar soft property that distinguishes it from the throstle or water twist. When the Crompton Mule came into general use, several improvements were made in its construction, by different eminent mechanicians, of whom Mr. John Kennedy and Mr. Peter Ewart were the most eminent; but it would form a long list to point out the authors of those improvements before any notable efforts were made to render it a self-acting machine.

It was the task of the "hand-spinner" to guide and regulate the "putting up" the carriage and winding the threads on the spindles, after they had been spun, and to perform these operations, the self-acting movements were devised, to render the mule independent of his skill. The real difficulty to render the mule a self-acting machine was chiefly confined to winding the threads on the continually changing forms and size of the cops.

The first notable attempt to construct a self-acting Mule was made by Mr. Niel Snodgrass, about the year 1815, but his plan appears not to have attracted much notice. The next was the patent obtained by Mr. William Eaton, of Wiln Mill (on the Trent in Derbyshire), dated in December 1818. Mr. Eaton has laid down with great exactness, the mechanical means of giving all the movements required for a self-acting mule, which are separately and minutely described in his specification. Mr. Eaton and his sons established a spinning mill, with his patent mules, to the extent of about 3000 spindles, in Chepstowe-street, Manchester, which were in operation in the years 1821 and 1822, where they were open to general inspection.

The next self-acting mule was patented by Mr. Richard Roberts, in 1825. His winding-on process, however, being imperfect, prevented his mule from coming much into use until his second patent was brought out, in 1830, which was far superior to the former and was more extensively adopted. In the meantime, Mr. Maurice de Jongh, of Warrington, obtained a patent for his self-acting mule, dated 1827. This mule was worked with moderate success for some years. We next come to the patent of Mr. Peter Ewart, dated in 1833, in which he very accurately describes all the movements required to render "the mule independent of the attention and skill of the spinner," viz.—(1) The backing off; (2) guiding the faller wires; (3) putting up the carriage; and (4) the winding-on; which last "I consider the most important movement of the machine." Mr. Ewart's mules were established in his mill, in East-street, Lower Mosley-street, and continued in regular operation until after he left Manchester. The next self-actor was patented by Mr. James Smith, of Deanston, in 1834. There was much novelty and some good properties in Mr. Smith's mule, so that it and Mr. Robert's second mule were the chief competing machines for many years. Besides the foregoing a patent was obtained by Messrs. J. and J. Potter for a self-acting mule, dated in 1836. The specification of this patent "being out of print" at the Patent Office, no account can be here give of it.

It was not until after the expiration of the second patent of Mr. Roberts, and that of Mr. Smith, that a really good working mule was constructed, as all the former plans becoming public, any of them might be combined to produce a better machine than those before patented, and which appears to have been realised in the patent obtained, in 1847, by Mr. Matthew Curtis and Mr. Robert Lakin. Thus thirty years had elapsed from the time when Mr. Eaton gave the true principles for constructing a self-acting mule to that of their being carried into practical effect as above stated.

The advances made in the wealth-creating powers of the people are more conducive to the public welfare than even the accumulation of wealth produced by their exercise. These powers of adding to the fixed capital of the nation come mainly from the use of labour-saving machines in our manufactories, and especially in those of the cotton and other textile fabrics. Hence it seemed desirable to record some of the changes and improvements made in the principal machines employed in our cotton mills. Of these the mule jenny is perhaps the most important, and the successive changes it has undergone give a fair type of the gradual progress of mechanical inventions generally.

Great inventions may come perfect from the *head,* but seldom, or never so, from the *hand* of their authors. Thus the Crompton mule (as before stated) underwent many useful changes by the skill of others, which prepared it for the inventions to render it self-acting. In the foregoing account of the labours bestowed on the mule I have given the names and claims of the inventors as they appear in their several patents, and I may assume that the public will desire to have the simple facts relating to the parties concerned, thus plainly set forth.

### Ordinary Meeting, April 5th, 1864.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Mr. John Eastham was elected an Ordinary Member of the Society.

Messrs. H. M. Ormerod and G. C. Lowe were appointed Auditors of the Society's Accounts for the present Session.

Mr. Bottomley gave the results of some experiments upon the estimation of sulphur made according to a method devised some time ago by M. Pelouze. This method had been brought under the notice of the Society by Dr. Calvert, as being a quick and approximate one; the present experimenter had, however, obtained these approximations within such wide limits that he had not found the process available for the purpose intended. In these experiments, 20 grains of the ore, very finely powdered, were mixed with 100 grains of carbonate of soda, 100 grains of chlorate of potash, and 140 grains of common salt. The carbonate of soda and common salt were always heated previous to an experiment, to ensure their being quite dry. These materials were well mixed for about ten minutes in a porcelain mortar.

In all cases the mixture quietly deflagrated, without throwing any particles out of the platinum crucible.

The following were the results obtained:—

| | True Percentage. | Percentage by Alkalimetry. |
|---|---|---|
| 1 | 36·98 | 33·20 |
| 2 | 40·75 | 36·95 |
| 3 | 38·44 | 38·80 |
| 4 | 39·64 | 42·11 |
| 5 | 36·87 | 41·70 |

In the two first experiments the materials were kept fused at a low red heat for some time after deflagration had ceased. In the three last experiments the lamp was removed soon after deflagration had taken place.

In the filtrate from the first experiment arseniate of soda was found.

In the fourth and fifth experiments arsenic was also found, but not in quantities adequate to account for the difference between the two percentages.

These wide differences between the results in the two columns no doubt depend upon the fact that the chlorate of potash does not act merely as an oxidizing agent, but gives up some chlorine. In all cases some oxygen compound of chlorine has been evolved with the carbonic acid.

It is worthy of remark that many years ago it was stated by Vanquelin that the residue obtained after decomposing chlorate of potash was sensibly alkaline. ("Ann. de Chimie et de Physique," xcv., 101.) A few grammes of chlorate of potash were heated in a platinum dish for a few minutes, until the fluid mass began to solidify. This residue, dissolved in water gave a solution that seemed faintly alkaline, so faint, however, as to be scarcely perceptible. Prolonged heat and perfect decomposition of perchlorate of potash might perhaps have made it more marked.

The above experiments were made in Mr. Spence's laboratory. The same method had been tried in Mr. Rumney's laboratory, by Mr. Bocharoff; his results, however, did not agree with the true percentage. He also ascribed the disagreement to the same cause as that suggested in this notice.

Mr. Baxendell gave the details of some observations he had lately made at Southport to determine the velocities of rifle balls. Three kinds of rifle were used by the firing party, the Whitworth, the Henry, and the Enfield; and the distance of the target being 600 yards, it was found that the average times required by the balls to traverse this distance were—

| | Seconds. |
|---|---|
| With the Whitworth rifle | 1·59 |
| „ Henry | 1·75 |
| „ Enfield | 1·87 |

The velocities were therefore—

| | Feet per Second. |
|---|---|
| With the Whitworth rifle | 1,132 |
| „ Henry | 1,028 |
| „ Enfield | 962 |

The differences between individual results were greatest with the Enfield rifle, and least with the Whitworth; and the number of points made was greatly in favour of the Whitworth instrument, the accuracy of fire apparently increasing in a greater ratio than the increase in the velocity of the balls.

It was understood that the charges of powder used were those which experience had shown to be the best adapted for each form of rifle.

Annual Meeting, April 19th, 1864.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Capt. JOHN MITCHELL, Superintendent of the Madras Museum, was elected a Corresponding Member of the Society.

The following Report of the Council was read by one of the Secretaries:—

## REPORT OF THE COUNCIL.

In presenting their Annual Report the Council are glad to be able to congratulate the Members upon the generally satisfactory condition of the Society. During the past year fourteen new ordinary Members have been elected, whilst on the other hand the losses have been by deaths four, defaulters two, and resignations four, the present number of ordinary members being one hundred and ninety against one hundred and eighty-six of last year. The Council are also glad to call attention to the satisfactory condition of the Society's finances as seen in the Treasurer's Report annexed, the balance in the hands of the Treasurer now being £371. 8s. 1d. against £320. 18s. 7d. of last year. The two parts of the second volume of the third series of the Memoirs are now ready, the third part completing the volume, and containing papers read during the present Session, is already in hand, and will be finished before the commencement of the ensuing Session. The increasing value of the library of scientific reference is a subject to which the Council may justly refer with pride, the number of academies and scientific bodies with which the Society is in communication has steadily augmented, and now exceeds that of almost any other similar institution. The ordinary Members deceased during the last financial year are Mr. George Mosley, Mr. John Gould, Mr. John Jessie, and Dr. Edward Stephens.

Mr. Mosley was associated with the Society for some years, having been admitted an ordinary member in January, 1859, and elected a Member of the Council in April, 1861. He took a prominent part in the formation and working of the microscopical section of which he was the honorary secretary. He organised a scheme for procuring soundings from different parts of the world, for the purpose of investigating the various formations of the sea and ocean beds; his labours in this direction met with great success, and the Society now possesses a large number of specimens which are in process of arrangement and classification. As an accurate observer and indefatigable student of the microscope, his place will not be easily filled.

The list of honorary members has suffered a great loss in the death of Mr. Richard Roberts, C.E. This gentleman, now generally acknowledged to have been one of the first mechanicians of our time, was admitted as an ordinary Member of the Society in the year 1823, April 16, and on his removal from Manchester he was elected in April, 1861, an Honorary Member. The mechanical inventions of Mr. Roberts were of the most varied and important description; the best known of his inventions was the improvement of Crompton's mule, so as to render it self-acting, and his improvements in engineering tools. His first patent for the former purpose was taken out in March, 1825, and a second patent for an improved process was granted to him in July, 1830. Mr. Roberts obtained nearly thirty patents in England for the following useful subjects:—hand and power looms, machines for preparing and spinning fibrous substances, steam engines, locomotive carriages, apparatus for punching, shearing, and perforating metals, beetling and mangling machines, clocks, watches, and other timekeepers, machine for hoisting weights, telegraphic communication, weaving and cutting plushes, velvets, carpets, &c., fluid meters, pumping apparatus, screw propellers, churns for agitating and evaporating fluids, turbines, hydraulic presses, steam ships and other vessels, anchors, steam boilers, life boats, war ships and their armaments, casks, machinery for cutting paper, leather, cloth, &c., machinery for punching, drilling, and riveting, omnibus and other carriages, folding fabrics, mechanism for engraving and otherwise copying paintings and other designs on flat and carved surfaces, &c., &c.

The following list of communications made during the past year proves that the scientific activity of the Society has in no wise diminished :—

*April* 30*th*, 1863.—"On the Wave of High Water, with Hints towards a New Theory of the Tides," by Mr. Thomas Carrick.

*April* 30*th*, 1863.—"On the Number of Days on which Rain falls annually in London, from Observations made during the 56 years, 1807-1862," by G. V. Vernon, F.R.A.S., M.B.M.S.

*April* 30*th*, 1863.—"On the Rain Fall at Oldham during the years 1836-62," by John Heap, Esq. with remarks by G. V. Vernon, F.R.A.S., M.B. M.S.

*May* 12*th*, 1863.—"Note on the History of Steam Navigation," by W. J. Macquorn Rankine, F.R.S., &c.

*July* 1*st*, 1863.—"The Complete Theory of Groups, being the Solution of the Mathematical Prize Question of the French Academy for 1860," by Rev. Thos. P. Kirkman, M.A., F.R.S., &c.

*October* 6*th*, 1863.—"On Ten Ancient Spanish Coins taken in change at Malaga," by John Harland, Esq., F.S.A., communicated by Mr. R. D. Darbishire.

*October* 6*th*, 1863.—"On the Sound produced in the Electric Discharge," by Professor Wm. Thomson, F.R.S., communicated by Professor Tait.

*October* 20*th*, 1863.—"Further Observations on the Carboniferous, Permian, and Triassic Strata of Cumberland and Dumfries," by E. W. Binney, F.R.S., F.G.S.

*November* 3*rd*, 1863.—"On the Theory of Equations," by His Honour James Cockle, M.A., F.R.A.S., &c., communicated by Rev. Robert Harley, F.R.S., &c.

*November* 3*rd*, 1863.—"On Recent Researches on the Theory of Equations," by the Rev. Robert Harley, F.R.S.

*November* 3*rd* 1863.—"On Marine Shells in Stratified Drift, at High Levels, on Moel Tryfaen, Caernarvonshire," by R. D. Darbishire, F.G.S., &c.

*November* 3*rd*, 1863.—"On the Existence of the Moa of Naturalists in New Zealand," by Charles Clay, M.D.

*November* 3*rd*, 1863.—"Remarks on the Habits and Character of the Maoris of New Zealand," by Charles Clay, M.D.

*November* 12*th*, 1863.—"On the Construction of Bi-focal Lenses for Spectacles," by J. Atkinson, F.G.S.

*November* 12*th*, 1863.—"On a new Variable Star (T. Aquilæ)," by J. Baxendell, F.R.A.S.

*November* 17*th*, 1863.—"On a new method of producing Carbonic Oxide," by Dr. F. Crace Calvert, F.R.S.

*November* 17*th*, 1863.—"On an Apparatus for Measuring Tensile Strengths, especially of Fibres," by Charles O'Neill, F.C.S.

*November* 17*th*, 1863.—"Experiments and Observations upon Cotton," by Charles O'Neill, F.C.S.

*December* 1*st*, 1863.—"Note on the Meteor of February 6th, 1818," by J. P. Joule, LL.D., &c.

*December* 15*th*, 1863.—"Notes on some recent Discoveries in Elemental Physics," by J. C. Dyer, V.P.

*December* 29*th*, 1863.—"Additional Observations on the Drift Deposits and more recent Gravels around Manchester," by Ed. Hull, B.A., F.G.S.

*January* 12*th*, 1864.—"Enquiry into the Question, whether Excess or Deficiency of Temperature during part of the year is usually compensated during the remainder of the same year," by G. V. Vernon, F.R.A.S., M.B.M.S.

*January* 12*th*, 1864.—"Examination as to the Truth of the Assertion that when November has a mean temperature above the average, it is usually followed by excessive cold between the December and March following," by G. V. Vernon, F.R.A.S., M.B.M.S.

*January* 12*th*, 1864.—"Note on the Amount of Carbonic Acid contained in the Air of Manchester," by Henry E. Roscoe, B.A., Ph.D., F.R.S.

*February*, 9*th* 1864.—"On the Photochemical Effect of the Light emitted by burning Magnesium Wire," by Henry E. Roscoe, B.A., F.R.S.

*February* 9*th*, 1864.—"On the Tensile Strength of Cotton as affected by various Chemical Treatments," by Chas. O'Neill, F.C.S.

*February* 23*rd*, 1864.—"Note on the Preparation of Calcium," by Mr. Edward Sondstadt, communicated by Professor Roscoe.

*February* 23*rd*, 1864.—"On the Nature of Friction in Mechanics," by J, C. Dyer, V.P.

*March* 8*th*, 1864.—"On the Existence of the Willow Leaf-shaped Bodies on the Sun's Surface," by E. J. Stone, M.A., communicated by Mr. James Nasmyth.

*March* 8*th*, 1864.—"On Periodic Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface," by Jos. Baxendell, F.R.A.S.

*March* 22*nd*, 1864.—"On the Microscopic Structure of the Cotton Fibre," by Captain John Mitchell, of Madras, communicated by Mr. Harst.

*March* 22*nd*, 1864.—"Notes on Spinning Machines: Part I., The Mule Jenny," by J. C. Dyer, V.P.

## LIBRARIAN'S REPORT.

Since the close of last Session 396 vols., 216 parts of vols of non periodical works, 214 pamphlets, and 27 quarterly, 161 monthly and 303 weekly numbers of periodicals, have been added to the Library, of which 169 vols., 13 quarterly and 35 monthly numbers by purchase; the remainder by exchange or donation.

The periodical publications which the Librarian is authorised to procure for the Library by purchase, are:—

The London, Edinburgh, and Dublin Philosophical Magazine.
The Quarterly Journal of Pure and Applied Mathematics.
The Cavendish Society's Publications.
The Ray Society's Publications.
The Palæontographical Society's Publications.
The Natural History Review.
The Annals and Magazine of Natural History.
The Quarterly Journal of Science.
The Registrar General's Annual Reports.
Les Annales de Chimie et de Physique.
Le Journal de l'Ecole Polytechnique.
Les Annales des Sciences Naturelles.
Le Journal de Pharmacie et de Chimie.
Les Archives des Sciences Physiques et Naturelles.
Die Astronomische Nachrichten.
Die Annalen der Physik und Chemie.
Die Annalen der Chemie und Pharmacie.
Der Journal fur die reine und angewandte Mathematik.
Neues Jahrbuch fur Mineralogie, Geologie, und Palæontologie.
Die Zeitschrift fur wissenschafsliche Zoologie.

The Philosophical Transactions for 1845 and 1846, mentioned in the last Annual Report as having been taken out from the Library without being entered in the Register, have not been returned, notwithstanding repeated announcements in the Circulars convening the Meetings. It is to be feared that these volumes must be considered as lost.

## ROBERT WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

*From* 31*st March*, 1863, *to* 31*st March*, 1864.

**Dr.**

| | | | | £ s. d. | £ s. d. |
|---|---|---|---|---|---|
| 1863. April 1. | —To Balance in the Bank of Heywood Brothers and Co. | | | 314 8 8 | |
| | ,, Balance in hands of Treasurer | | | 6 9 11 | 320 18 7 |
| 1864. Mar. 31. | —To *Members' Contributions:* | | | | |
| | Arrears, 1861-2 | 1 | | 2 2 0 | |
| | ,, 1862-3 | 12 | | 25 4 0 | |
| | Members on the Roll, April 1st, 1863, 186, at 42s. | | £390 12 0 | | |
| | Deduct Compounders | 3 | | | |
| | ,, Paid in advance | 2 | | | |
| | ,, Dead | 2 | 37 16 0 | | |
| | ,, In arrear, March 31, 1864 | 11 | | | |
| | | | | 352 16 0 | |
| | Six members elected in 1863, April—Dec. | | | 12 12 0 | |
| | Eight ditto, in 1864, half subscription | | £8 8 0 | | |
| | Deduct in arrear | | 6 6 0 | 2 2 0 | |
| | Admissions paid | 8 | | 16 16 0 | 411 12 0 |
| ,, ,, | To *Sale of Publications:* | | | | |
| | Memoirs—Ser. 3, Vol. I. | | | 1 1 0 | |
| | Proceedings | | | 0 15 0 | |
| | Dalton's Meteorology | | | 0 3 0 | 1 19 0 |
| ,, ,, | To *Sundry Income:* | | | | |
| | Sect. Contrib.: Mathematical | | £2 2 0 | | |
| | ,, Statistical | | 1 1 0 | | |
| | ,, Microscopical | | 2 2 0 | 5 5 0 | |
| | Photographic Society—Attendance, Coal, &c., at Meetings | | | 4 10 0 | |
| | Interest allowed by Bankers | | | 10 3 4 | 19 18 4 |
| | | | | | £754 7 11 |
| | Library Fund | | | £86 7 0 | |
| | Compositions | | | 72 10 0 | |
| | General Balance | | | 212 11 1 | |
| | Total Balance | | | £371 8 1 | |

**Cr.**

| | | | £ s. d. | £ s. d. |
|---|---|---|---|---|
| 1863. March 31. | By *Charges on Property:* | | | |
| | Chief Rent | | 12 10 3 | |
| | Fire Insurance | | 7 17 6 | |
| | Property Tax | | 4 19 2 | 25 6 11 |
| | ,, *House Expenditure:* | | | |
| | Water, Gas, Candles, and Coals | | 16 5 9 | |
| | Cleaning and Petty Expenses | | 2 13 10 | |
| | Furniture | | 0 4 3 | |
| | Repairs | | 0 14 6 | |
| | Tea and Coffee at Meetings | | 16 13 11 | 36 12 3 |
| | ,, *Administrative Charges:* | | | |
| | Salary—Keeper of Rooms | 60 0 0 | | |
| | Wages—Assistant ditto | 12 8 0 | 72 8 0 | |
| | Attendance on the Photographic Society and Sections | | 7 10 0 | |
| | Postage and Parcels | | 18 6 7 | |
| | Stationery, Printing Circulars, &c., and Receipt Stamps | | 22 18 7 | 121 3 |
| | ,, *Publishing:* | | | |
| | Memoirs—Printing, Engraving, &c. | | 96 18 0 | |
| | Printing Proceedings | | 44 17 0 | 141 15 0 |
| | ,, *Library:* | | | |
| | Periodicals, Binding, &c. | | 53 15 10 | |
| | Memoirs Purchased | | 0 6 0 | |
| | Subscription to Cavendish Society | | 1 1 0 | |
| | ,, Ray Society | | 1 1 0 | 56 3 10 |
| | ,, *Sundry Expenditure:* | | | |
| | Solicitors, for Registration of Alteration of Rules | | | 1 18 8 |
| | Total Disbursements | | | 382 19 10 |
| | Balance in Bank of Heywood Brothers and Co. | | | 371 8 1 |
| | | | | £754 7 11 |

ROBERT WORTHINGTON, Treasurer.
1st *April*, 1864.

Examined and found correct, 8th April, 1864.

HENRY M. ORMEROD, } Auditors.
GEORGE CLIFF LOWE, }

On the motion of Mr. ORMEROD, seconded by Mr. PARRY, the Annual Report was unanimously adopted.

The following Gentlemen were elected Officers of the Society for the ensuing year:—

### President.

ROBERT ANGUS SMITH, PH.D., F.R.S., &c.

### Vice-Presidents.

JAMES PRESCOTT JOULE, LL.D., F.R.S., &c.
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# PROCEEDINGS

OF THE

# LITERARY AND PHILOSOPHICAL SOCIETY

OF

# MANCHESTER.

## VOL. IV.

SESSION 1864–65.

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## NOTE.

The object which the Society have in view in publishing their Proceedings, is to give an immediate and succinct account of the scientific and other business transacted at their Meetings, to the members and the general public. The various communications are supplied by the Authors themselves, who are alone responsible for the facts and reasonings contained therein.

# INDEX.

Airy G. B., M.A., F.R.S., Astronomer Royal.—On Auroral Arches, p. 170.

Alcock Thomas, M.D.—On Specimens of Foraminifera from Roundstone, Connemara, p. 52. Remains of an Ichthyosaurus, p. 99. Notes on a Visit to Walton Hall, p. 100. On Marine Entomostraca, p. 174. Southport Natural History, p. 188. Notes on Natural History Specimens from Connemara, p. 192.

Bates Rev. J. Chadwick, M.A., F.R.A.S., F.G.S.—Raingauge and Anemometer Observations, 1864, p. 143.

Baxendell J., F.R.A.S., Hon. Sec.—Earthquake of September 25, 1864, p. 1. New Star, p. 22. Period and Changes of the Greenwich Variable Star in Vulpecula, p. 54. Observation of an Auroral Arch, p. 133. On a Thermometer constructed by Dr. Dalton, p. 133. Note on Mr. Bates's Raingauge and Anemometer Observations, p. 145.

Binney E. W., F.R.S., F.G.S., V.P.—Strokes of Lightning, p. 25. Remarks on Marine Shells found at Macclesfield, p. 43. Spores of Plants in Splint Coal, p. 45. Remains of the Elephant found in Derbyshire and Cheshire, p. 49. Internal Structure of Stigmaria, p. 87. Further Observations on the Permian and Triassic Strata of Lancashire, p. 134.

Breguet M.—Construction of Dumas's Lamp, p. 101.

Brierly Henry.—Spinning Machines, p. 2.

Brockbank W.—Discovery of the Mammoth (Elephas Primigenius) at Waterhouses, near Leek, p. 46.

Brothers A., F.R.A.S.—Photograph of the Moon, p. 13. Portraits by the Magnesium Light, p. 65. Picture of the Blue John Mine taken by the Magnesium Light, p. 112. Was Daguerre a Discoverer? p. 113.

Buxton E. C., Junr.—Photographic Experience in India, p. 120.

Calvert F. Crace, F.R.S., F.C.S.—Action of Sea Water upon certain Metals and Alloys, p. 115.

Caro Heinrich.—Injurious Action of Alkalies on Cotton Fibre, p. 149.

Carrington Benjamin, M.D.—On an Annelidan Larva, and On the Embryology of Annelids, p. 100. On the Chætopod Annelides of the Southport Sands, p. 176.

**Clifton** Prof. R. B., M.A., F.R.A.S.—On an Acoustical Electric Telegraph, p. 53.

**Cockle** Chief Justice, M.A., F.R.A.S., F.C.P.S.—On Differential Equations, p. 38.

**Dancer** J. B., F.R.A.S.—Contrivance for Regulating the Amount of Light in Using the Microscope, p. 34. Exhibition, Stereoscopically, of Photography on a Large Scale, p. 111. The Opaque Microscope not New, p. 120. Microscopical Appearances of Cotton Hair during Dissolution in the Ammoniacal Solution of Copper, p. 152. On Pseudoscopic Vision through Prisms, p. 157. On Viewing Photographic Pictures taken with Lenses of different Foci, p. 190.

**Dancer** William.—Injurious Action of Alkalies on Cotton Fibre, p. 149.

**Darbishire** R. D., F.G.S.—Notes on Marine Shells found in Stratified Drift at Macclesfield, p. 41.

**Dyer** J. C., V.P.—Spinning Machines, p. 12.

**Heap** John.—Note on the Rainfall of the last Twenty-nine Years at Royton, Oldham, p. 147.

**Heelis** Thomas, F.R.A.S.—Account of a Fireball, p. 84.

**Herschel** Alexander S., B.A.—On the Auroral Arch of March 20, 1865, p. 171.

**Heyes** W. H.—Markings on Leaves of the Vegetable Marrow, p. 36. Structure of Cotton Fibre, p. 63. Preparation of Canada Balsam for Mounting Microscopic Objects, p. 173.

**Hunt** G. E.—Discovery of Potamogeton Nitens in Loch Ascog, Rothsay, p. 52. Notes on Mosses, p. 141.

**Johnson** J. R.—Pantascopic Camera, p. 111.

**Johnson** R., F.C.S.—Action of Sea Water upon certain Metals and Alloys p. 115.

**Joule** J. P., LL.D., F.R.S., V.P.—Hardening Steel Wires for Magnetic Needles, p. 28. New Magnetic Needle for showing Rapid and Minute Changes of Declination, p. 37. Testing Boilers by Hydraulic Pressure, p. 89. New Camera, p. 113. Instrument for showing Rapid Changes in Magnetic Declination, p. 131. Improvements in a Camera, p. 192.

**Kirkman** Rev. T. P., M.A., F.R.S.—Relation of Force to Matter and Mind, p. 14. Corrigenda, p. 28. Theory of Groups, Corrigenda and Addenda, p. 171.

**Knott** George, F.R.A.S.—Observations of the Greenwich Variable in Vulpecula and its Companion Stars, p. 55.

**Latham Arthur G.**—Examination of a Shell of Helix Nemoralis, p. 62.

**Linton James.**—Stigmas of Poterium Sanguisorba; Calyx of Gum Cistus, p. 36.

**Lund Edward**, F.R.C.S. *Ex.*—On the Points of Resemblance and Difference between the Skeletons of the Gorilla and Man, p. 57.

**Mitchell John**, Captain.—Experiments on Cotton Fibre, p. 51.

**Mudd James.**—A Photographer's Dream, p. 191.

**Mylius A.**—Action of Caustic Soda on Ethylic and Methylic Alcohol, p. 106.

**Nasmyth James**, C.E.—Antiquity of the Features and Details of the Lunar Surface, p. 29. On a Large Group of Solar Spots, p. 78.

**Nevill T. H.**—Foraminifera from Gorteen Bay, Connemara; and a New Method of Mounting Specimens, p. 63. New Sensitive Paper, p. 67.

**Parry John.**—Section of a Shell of Helix Aspersa, p. 62. On Mr. Sidebotham's Camera, p. 67. Pressure Apparatus for Mounting Microscopic Objects, p. 174.

**Robinson John.**—Alloy to resist the Action of Sea Water, p. 119.

**Roscoe** Prof. H. E., F.R.S., Hon. Sec.—Mr. Rutherford's Photographs of the Fixed Lines in the Solar Spectrum, and of the Moon, p. 69. Preparation of Bulbs for exhibiting the Chemical Combination of Chlorine and Hydrogen Gases, p. 101.

**Schunck E.**, F.R.S., V.P.—On some Products derived from Indigo Blue, p. 70.

**Sidebotham J.**—Trees damaged by Lightning, p. 25. Address to Microscopical Section, p. 33. Seed of Sanicula Europea, p. 36. Printing Transparencies for the Stereoscope and Magic Lantern, p. 65. On the Wothlytype Process, p. 68. Notes on the Development of the Wings of Lepidopterous Insects, p. 98. Proper Focus of Lens to be used in taking Photographic Landscapes, and on some Modes of Measuring the Size of Objects therein depicted, pp. 113 and 190.

**Smith R. Angus**, F.R.S., P.—Dancer's Aspirator, p. 26. Composition of the Atmosphere, p. 30. On some Physiological Effects of Carbonic Acid and Ventilation, p. 79. On the Meteorological Instruments invented by Dr. Joule, p. 132. Minimetric Method of Analysis, p. 159.

**Sonstadt Edward.**—New Reagent for the Separation of Calcium from Magnesium, p. 90.

**Taylor James.**—Rainfall at Oldham, Strinesdale, and Brushes Clough, p. 19.

**Thom John.**—New Form of Roof for Dyehouses, p. 103.

VERNON G. V., F.R.A.S.—Note on the Rainfall of 1864, p. 85.

WARDLEY GEORGE.—On Glass Transparencies, p. 192.
WATSON JOHN.—On the Plumules or Battledore Scales of the Lycænidæ, p. 98.
WILLIAMSON Prof. W. C., F.R.S.—Address to the Photographical Section, p. 64. Difficulties in Determining Specific Distinctions in the Lower Forms of Animal Life, p. 140.
WORTHINGTON S. B., C.E.—Bridge with Girders made of Bessemer Steel Plates, p. 77.

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*Meetings of the Physical and Mathematical Section.*—Annual, p. 143. Ordinary, pp. 19, 53, 84.
*Meetings of the Microscopical Section.*—Annual, p. 189. Ordinary, pp. 33, 51, 52, 62, 97, 99, 140, 141, 173, 174, 188.
*Meetings of the Photographical Section.*—Annual, p. 191. Ordinary, pp. 64, 67, 111, 112, 120, 190.
*Report of the Council.*—April 25, 1865, p. 163.

# PROCEEDINGS

OF

# THE LITERARY AND PHILOSOPHICAL SOCIETY.

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Ordinary Meeting, October 4th, 1864.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., Vice-President, in the Chair.

Amongst the donations announced was a bust of Dr. Dalton, presented by Mrs. Samuel Fletcher.

On the motion of Dr. ROSCOE, seconded by Dr. JOULE, the best thanks of the Society were unanimously voted to Mrs. Fletcher, for her very valuable present.

The CHAIRMAN, in announcing the severe loss which the Society had sustained by the death of their late lamented Honorary Librarian, adverted to the very important services he had rendered. By Mr. Ekman's able management and indefatigable industry the library had become one of the most valuable in the country.

THOMAS WINDSOR, Esq., M.R.C.S., was unanimously elected Honorary Librarian of the Society.

Several members stated their experience of the earthquake which occurred on the 26th ult. Mr. Baxendell described a remarkable derangement of the sidereal clock belonging to the corporation, which he had mounted at his house, which has suggested a new method of registering the occurrence of earthquake shocks.

A Paper was read entitled "Remarks on Mr. Dyer's Paper entitled 'Notes on Spinning Machines,' by Mr. Henry Brierly"; communicated by E. W. BINNEY, F.R.S.

Mr. Dyer, in the abstract of his paper (*Proceedings*, vol. iii. p. 265), commences by saying that "two distinct principles were embraced in the inventions of James Hargreaves and Richard Arkwright, which were afterwards combined by Samuel Crompton, to form the beautiful power-driven machine called the mule. Arkwright employed the throstle, or throated spindle, with arms or 'flyers' to conduct the threads on bobbins arranged in stationary frames; Hargreaves employed naked spindles arranged on a traversing frame or carriage, by which the threads were drawn out (about five feet) in horizontal lines, whilst being twisted, and were then taken up, or wound, on the spindles, to form 'cops,' whilst the carriage returned to the roller beam for another 'stretch.'"

As I understand Mr. Dyer, he attributes to Arkwright the invention of the *spindle and flyer*, and seems to consider that as the leading "*principle*" of Arkwright's invention, as distinguished from Hargreaves's invention, the main "*principle*" of which he mentions as consisting in the employment of *naked spindles* mounted on a *traversing frame* or *carriage*. Mr. Dyer is in error on both these points; and it is only fair to a very ingenious inventor who preceded both Hargreaves and Arkwright that these matters should be properly understood.

As to Hargreaves's "Jenny," there is no doubt of the originality of the invention, and that Hargreaves is entitled to the sole merit of whatever was valuable in that invention; but I shall show that as regards the inventions commonly attributed to Arkwright the case is far different. Before doing so, however, I must point out the mistake committed by Mr. Dyer as regards the nature of Hargreaves's invention. Hargreaves did not employ spindles mounted upon a *moving carriage*, nor was there any "*roller beam*" in his machine at all. Mr. Dyer seems to be here confounding the invention of Hargreaves with the subsequent invention of Crompton. An inspection of the specification of the patent taken out by Hargreaves in 1770 (No. 962) will show that in his "Jenny" the *spindles* revolve in *stationary bearings*, while instead of *rollers* for drawing out the material to the requisite fineness a "clasp" was used, which was arranged to move backwards and forwards in suitable framing. Thus, before beginning to spin a "stretch," the spinner had the "clasp" near to the spindles, with a certain length of roving between the clasp and each spindle. He then with one hand drew the clasp gently away from the spindles, thus elongating the portions of roving between them, while with the other hand he turned round certain apparatus by means of which the spindles were caused to revolve and twist the threads, and having thus drawn away the clasp for about five feet from the spindles he then returned it back towards them, at the same time guiding the threads by means of a "presser," or faller, and turning the spindles round so as to wind the spun threads upon *bobbins placed upon them*, and not upon the *spindles themselves*. This is a very different operation from that of the mule, in which *rollers* moving at different velocities draw the material to the required fineness, while the spindles *recede* from them in a *moving carriage*, at the same time revolving so as to twist the threads, which are afterwards wound upon the *spindles themselves* so as to form "cops." It is necessary to bear these distinctions in mind in following the progress of development which resulted in the production of the highly effective spinning machinery in use at the present day.

In coming to the consideration of the inventions usually attributed to Arkwright, it is only proper that due notice should be bestowed upon those of a very ingenious man who preceded him; viz., Lewis Paul. The invention of "spinning by rollers," as it is familiarly termed, and for which Arkwright usually obtains the credit, most undoubtedly originated with Paul. It is only necessary to read the specification of the patent granted to Paul on the 24th June, 1738, to be satisfied on this point. It is true that there has been some dispute as to whether Paul himself really originated the invention described in that specification, a person of the name of Wyatt being mentioned by some as having communicated the invention to Paul, and another person of the name of Highs having laid some sort of claim to the invention. The claims of these parties, however, only rest upon some very vague assertions made by themselves or persons connected with them, such, for instance, as that contained in a letter written by Wyatt to Sir Leicester Holt, and quoted at page 124 of Baines's *History of the Cotton Manufacture*, in which Wyatt says, referring to the machine known as Paul's—"I am the person that was the principal agent in compileing the spinning engine," &c. Such assertions as these, however, cannot be taken as satisfactory proofs of the claims of these parties, and when in opposition to them we have the specification of a patent granted to Paul for this application, *in which specification the name of Wyatt actually appears* as that of a witness to the signature, &c., of the document by Paul, it is difficult for an unprejudiced person to come to any other conclusion than that Paul was really the inventor of the contrivance for which the patent was granted. However that may be, it is quite clear that this invention does not belong to Arkwright, whose first patent relating to spinning machinery was taken out in 1769, or above thirty years after the patent granted to Paul. And that Arkwright was aware of the existence of Paul's patent is beyond all doubt, and is proved by documents which are quoted in the work to which I have already referred, viz., Baines's *History of the Cotton Manufacture*, as well as by other documents to which I need not now particularly allude.

In speaking of the "peculiar soft property" possessed by the yarn spun upon the mule, as distinguishing it from the yarn spun by the machine called the "throstle," Mr. Dyer is quite correct. I believe it is not so generally known as it should be that the "throstle," as the term is now generally understood, consists mainly of a combination of the leading features of Paul's invention of 1738, which I have already mentioned, with those of a later invention, for which he obtained a patent in 1758. Mr. Dyer mentions the *spindle and flyer* as *Arkwright's invention*, but they had been in use upwards of *one hundred and sixty years* before the date of Arkwright's inventions. This is proved by several of the works now in the library of the Patent Office, especially an Italian work, entitled "Novo Teatro di Machine et Edificii," &c., and a pamphlet published in 1681, entitled "Some Proposals for the Employment of the Poor." In Paul's specification of 1738 we have the application of rollers moving at different velocities for drawing the material, and in that of 1758 the *combination* of *rollers* (of which only one pair are shown in the specification of the latter patent) with the *spindle* and *flyer*, by means of which the thread was twisted and wound upon a bobbin. So far as I have been able to ascertain, this is the first instance in which we have the material passing through *rollers* in its way to a *spindle and flyer* by which it is twisted and wound upon a bobbin, and we have here, clearly, the origin of the "throstle." Paul's specification of 1758 describes these operations very minutely, but spinners acquainted with this class of machinery will at once perceive that the machine described in Paul's specification, though displaying great ingenuity, was defective in one important particular, which was, that there was no arrangement by which the "traverse" of the bobbin, as it is now termed, could be performed. The manner in which the yarn was distributed over the bobbin was peculiar. The flyer was *tubular*, and only reached downwards to just within the head of the bobbin, and the latter was of *conical* form, the lower end being the smallest. The yarn was thus wound constantly at the *top* of the bobbin by the flyer, but as it accumulated there, the *conical form* of the bobbin caused the coils of yarn to *slip downwards*, and thus a kind of distribution of yarn over the bobbin was effected. This part of the arrangement was evidently very defective, and it is highly probable that the want of some more efficient method of distributing the yarn over the bobbin was one if not the sole cause of the want of success which undoubtedly attended the efforts of Paul to bring his machine into general use. The addition to the machine of the "traverse motion," as it is called, or the arrangement by which the bobbin is made to ascend and descend on the spindle, was unquestionably a vast improvement, and of this Arkwright may fairly have the credit. But even here Arkwright had been partly anticipated,* for in the specification of a patent granted to Comah Wood, in 1772, No. 1018, there is the description of a mode of causing the bobbin to be moved on the spindle for the purpose of winding the thread upon all parts of it in succession, this being done by a movable rail, capable of being shifted from time to time by hand. This, however, was only a slight approximation to what was required, and Arkwright, in applying the "heart motion,"† as it is now termed, may be said to have brought the throstle well nigh to the condition in which it is used at the present day.

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* Arkwright's specification of 1769 does not describe any arrangement for this purpose.

† The date of this application appears to be uncertain.

Crompton's invention, in which he combined the drawing rollers of Paul with the use of spindles mounted in a movable carriage, and which was thence called the "mule," as partaking to some extent of the principles of both the jenny and the throstle, formed an immense improvement on all previous inventions. Of course the mule has been greatly improved since Crompton's time; but in speaking of Mr. Kennedy and Mr. Peter Ewart as being among the *most eminent* of those who have improved that machine, Mr. Dyer gives no reason for his opinion. I pass from this, however, to Mr. Dyer's observations on the self-acting mule.

Mr. Dyer says the "real difficulty" connected with the construction of a self-acting mule was chiefly confined to the winding of the threads on the cops; but, as I can testify from some twenty years' actual experience, this is by no means the greatest difficulty, especially as regards the construction of *large mules*. The backing off, or unwinding of the loose coils of yarn from the spindles, which are above the cops, so as to allow the depression of the "faller," or guiding wire, is connected with mechanical difficulties of a most formidable nature, and the reaction produced by the sudden stoppage and reversal of the motion of the spindles of a large mule, with their attendant pulleys and geering, &c., has been a source of trouble and annoyance which none but those who have experienced it can understand. Mr. Dyer mentions a Mr. Snodgrass as having made the first "*notable*" attempt to construct a self-acting mule; but beyond the attempt being a notable *failure*, I know of nothing to entitle it to that distinction. Mr. Eaton's invention of 1819, which Mr. Dyer mentions as next in succession to that of Mr. Snodgrass, was undoubtedly a very ingenious invention, and was used *by himself* for some little time. Mr. Eaton's specification shows that his knowledge of the subject was very accurate, but the *details* of the invention, *practically considered*, were in many respects very defective. I will not here go into all these details, but will point out two defects, either of which were sufficient to prevent the successful application of the invention. One of these was the arrangement of the apparatus for effecting the "backing-off," (the difficulties attending which movement I have already noticed,) and the manner in which the "sector used by Mr. Eaton for this purpose was made to pass into geer with the wheel upon the" run "shaft," or shaft which turned the spindles, was most objectionable, as creating a constant tendency to derangement and breakage of both the sector and wheel; and that this *was* the result of the arrangement I have been informed by a most respectable engineer now living in Manchester, and who saw Mr. Eaton's machines during the short time they were in existence.

Another defect lay in the arrangement for effecting the winding of the yarn upon the spindles. For this purpose Mr. Eaton employed two conical pulleys, one of which gave motion to the other by means of a strap, the latter being traversed along the pulleys at intervals so as to vary the rapidity of motion of the spindles according to the varying size of the cops. It is obvious that in this case the liability of the strap to slip upon the pulleys must have led to great irregularity in the working of the machine, and in fact I do not believe that a *large machine* could be worked at all on such a system. These defects, along with others which might be mentioned, led to such trouble and annoyance in connection with the attempt to work these machines, that they were abandoned after a trial extending over a few months. In fact the attempts of Mr. Eaton to construct a self-acting mule, as well as those of Mr. De Jongh and of Mr. Ewart, to which Mr. Dyer also alludes, can only be looked upon as so many *experiments*, none of which were attended with any reasonable amount of practical success; all being abandoned after a fruitless struggle, exhausting the patience both of the inventors and of those connected with them.

In speaking of Roberts's invention of 1825, Mr. Dyer seems either not to understand of what that invention consisted, or to greatly undervalue it. Although it is quite true that this invention has been greatly improved upon since the date of the patent granted for it, those who are practically conversant with these matters know well that it contained the germs of what was afterwards developed into one of the most successful and extensively used machines ever produced. In fact it may truly be said that although great improvements in the construction of the self-acting mule have been made since the date of Roberts's invention, and especially during the last few years, still, in all or nearly all the self-acting mules now used there are embodied some of those excellent mechanical arrangements of which he was undoubtedly the originator, and which must always be looked upon as imperishable monuments of that splendid mechanical genius with which Roberts was beyond all question endowed. For example, the arrangement of the "cam-shaft" for producing the changes of motion of the machine; the arrangement of the double fallers; the method of causing the faller to be depressed by the reverse movement of the apparatus which turns the spindles in backing-off; and the system of regulating the "winding-on" by the gradual approximation or otherwise of the faller and counter faller, are all contrivances displaying the highest mechanical skill and ingenuity, and have all been most extensively brought into use, and so continue to the present day, while on turning to the patent obtained by Roberts in 1830, for the employment of the well known radial arm and drum as a means of effecting the "winding-on," we find that scarcely a self-acting mule is now constructed which does not contain in some form or other a modification of this famous invention.

In speaking thus of Roberts's inventions, I consider that I am only doing justice to a highly meritorious engineer, whose ingenious contrivances, although undoubtedly attended with some defects, which have led to certain of his arrangements being superseded by others of a more perfect character, must yet be admitted as having furnished some of the brightest examples of the mechanical genius of the present age. Mr. Roberts was the inventor of the first practical self-acting mule.

Mr. Dyer is correct in attributing to the inventions of Mr. Smith, of Deanstone, "much novelty and some good properties," and in stating that Mr. Smith's and Mr. Roberts's mules were the chief competing mules for many years. I really, however, cannot understand Mr. Dyer when he comes to speak of *Mr. Potter's* inventions. The patent of 1836, to which Mr. Dyer evidently refers, was not taken out by Messrs. John and James Potter, but by *Mr. James Potter only;* that gentleman being, as I know personally, an extremely ingenious mechanic. Mr. Potter's mules have had no inconsiderable amount of success. Mules to the amount of many thousands of spindles have been made and sold under Mr. Potter's patents, as I know personally. A large firm of cotton spinners, with whom I was formerly connected, purchased and used a number of them, and I have no hesitation in saying that they possessed qualities which rendered them for *some purposes* preferable, *on the whole*, to either Roberts's or Smith's mules. Such, indeed, was the admitted merit of Mr. Potter's invention, that on the approach of the termination of the fourteen years for which his patent was originally granted, the Lords of the Privy Council granted a prolongation of the patent for five years.

I am much surprised at what Mr. Dyer says in the latter part of his paper, in which he states that it was not until after the expiration of the second patent of Mr. Roberts and that of Mr. Smith that a *really good working* *mule* was constructed, "and which," says he, "appears to have been realised in the patent obtained, in 1847, by Mr. Matthew Curtis and Mr. Robert Lakin. Thus thirty years had elapsed from the time when Mr. Eaton gave the true principles for constructing a self-acting mule to that of their being carried into practical effect as above stated." Now, I am acquainted with the specification of the patent taken out by Mr. Curtis and Mr. Lakin, in 1847. The invention described therein consists almost wholly, so far as it relates to the mule, of improvements, or *alleged* improvements, in some of the details of Smith's and Roberts's machines, and there is no reason to give it the character of the first really good working mule ever constructed. I have the pleasure of knowing Mr. Curtis and others connected with his firm, and I should think if they were inclined to claim such a character for any of their inventions, it would certainly not be for the one Mr. Dyer mentions.

Nor is there any better foundation for the assertion that Mr. Eaton "gave the true principles for constructing a self-acting mule," &c. As I have already mentioned, Mr. Eaton's invention only survived a few months, and there is no single particular of Mr. Eaton's invention to be found in the self-acting mules now used, while, on the other hand, Roberts's inventions form the basis of the great bulk of the self-acting mules now made, *not excepting even those made by Messrs. Curtis and Co. themselves.* It was Mr. Roberts, and not Mr. Eaton, who "gave the true principles for constructing a self-acting mule."

The highest test of the efficiency of a self-acting mule is its capability of spinning the finer numbers of yarn, as, from the delicacy and accuracy of manipulation required in producing the very fine threads, it has been found very difficult to adapt the self-acting mule to that class of spinning. Mr. Roberts himself failed in this, as I have heard him admit in the witness box of a court of justice; but if this test were applied to the invention of Messrs. Curtis and Lakin, of 1847, it would be found *lower down in the scale than even Roberts's.* Many persons have, indeed, until a comparatively recent period, entertained the opinion that the spinning of fine yarns would never be successfully accomplished by the self-acting mule; and had I not already extended these observations to a much greater length than I at first intended, I could mention some interesting circumstances connected with the progress of this branch of invention. That would carry me, however, into a discussion with reference to inventors and inventions not mentioned in Mr. Dyer's paper, and I will therefore reserve what I have to say upon that part of the subject for some future occasion.

Mr. **Dyer**, in reply, said that he had supported his statements about the patented inventions noticed in his paper (the abstract of which only was commented upon by Mr. Brierly), by literal citations from the printed specifications. His Notes were entitled "Part the First, on the Mule Jenny," and that therefore he had only to make the distinction of this class of spinning machines from those of the throstle class, and his description of the latter went no further than to show how much of it Crompton took to make up his composite mules.

## Ordinary Meeting, October 18th, 1864.

J. P. Joule, LL.D., F.R.S., &c., Vice-President, in the Chair.

Mr. Brothers, F.R.A.S., exhibited a photograph of the moon, which he had made from a negative by Warren De la Rue, F.R.S. The original negative is one inch in diameter, and from this a positive two inches in diameter was first made. This was placed within the rays of a nine-inch condenser of the solar camera, and an enlarged negative on a plate 36in. by 24in. was produced. The print exhibited was on a single sheet of paper, and thus the disadvantage of joining several sheets together as in other large prints of the moon was avoided. Various effects from the same negative could be produced by providing either for the finer details of the strongly illuminated side of the moon, or for the more rugged parts at the side near and at the termination of parts in shade. During the conversation which followed, Mr. Brothers stated that the negatives were the property of Messrs. Smith and Beck. It was suggested by Mr. Baxendell that more accurate micrometrical measurements could be made from such a photograph than direct from the moon's surface in the telescope—in the one case the object being still, and in the other in constant motion; and Dr. Roscoe remarked that if a series of the several phazes of the moon on the same scale as the one shown were published, he believed they would be of great value for educational and scientific purposes.

A Paper was read, entitled "On the Relation of Force to Matter and Mind." Part I. By the Rev. **Thomas P. Kirkman**, M.A., F.R.S., Hon. Member.

There are at least three schools of thinkers among us, about the relation of force to matter. The first and most popular one conceives of matter as really existing and occupying space, whether it be or be not the seat or subject of force, so that if all force were withdrawn, matter would remain, quiescent and unexciting. The second school, not, it is to be hoped, a numerous one, affirms that matter exists of necessity, and is of necessity endued with force; that no power can divorce matter and force; that every form of chaos or order is but one of the combinations that eternal matter must assume by the eternal play and collision of eternal force; and that the notion of an Author and Preserver of the Universe is but the dream of ignorance. This is the school of the Materialists. A third and I think a growing school denies the existence of matter as distinct from force; this may be called the school of the Immaterialists. The Immaterialists are not the idealists of Berkeley's type, for the former affirm space, and an external world of force having a real existence in space, which the latter are far from affirming.

If any leading philosopher will boldly preach this Immaterialist gospel, and proclaim a crusade against matter, I am prepared to promise him one humble follower.

Of *force* no man is required to give either a demonstration or a definition. We cannot converse without agreeing in affirming ourselves in space, which is simply the affirmation of existence or force in this external world, which is not ourselves. But I have a right to demand from a disputant both a definition of *matter* and a proof of its existence. It appears to me that this existence is either an unproved inference from the experience of resistance, or is a delusion attached to a word of our infancy. Why cannot I close my hand upon a brick? The answer of a child is, because the brick is there, inactive and inert there, a piece of stuff which is doing nothing but just lying there: this is the notion of sluggish quiescent matter. The thinker perceives that he is prevented from closing his hand by force perpetually in action there, and that the brick is what it is by virtue of intense cohesion, and other resistances that defeat his pressure and the force of his will. Thus he gets the compound notion of matter having its seat in space, and of force having its seat in the matter.

But what need is there of the matter? Why may not the forces have their seats in space? How can you construct a demonstration of the presence of this matter?

You appeal first to the evidence of our senses. But we are all agreed that our sight and hearing can teach us nothing about forces, resistances, and motions, except through the interpreter touch; that the telegrams which reach the mind by sight and hearing are rapidly translated into nothing but the memory of touch. Now touch teaches us absolutely nothing but *resistances*, except lessons of pleasure and pain that may be here left out of consideration. Thus the evidence of our senses comes to nothing more than the consciousness and the memory of *forces* which resist our own force of volition.

You say that this table is a geometrical locus of material points in space, and at the same time a locus of forces having their seats in those material points. Let us suppose that the Author of the Universe, or some lower power competent to do it, were to remove all the matter from this locus, and leave a system of force exactly filling the locus of the removed matter, each force having its seat not in a point of matter but in a point of space: suppose that these pure force-points in pure space should present the same resistance to my hand or my tools, should receive the same vibrations from, and communicate the same vibrations to, all that surrounds the locus, as the force did before the matter was removed. How could our senses detect the absence of the vanished matter? The table would feel and look the same, sound, smell, cut, burn the same, as before. Then it appears that our senses help us to no evidence either of the presence or the absence of this mysterious matter.

Do you appeal next to the mathematician? You refer to his moments, moving forces, living forces, &c., in all which the *mass* is before us in his formulæ. It is true that the letter *m* is there; but to the mathematician it is in all cases simply a number, a constant obtained by experiment only, experiment which is independent of all hypothesis about the existence or non-existence of matter. Newton, in the opening of his *Principia*, says, *Virium causas vel sedes non expendo.* He neither troubled himself about the cause, nor about the seat of force. It was sufficient for him to have the exact positions in space of the centre and points of departure of his forces with their directions and the numerical constant, given by experiment, which determine their intensities. No mathematician cares anything about material seats for his forces: he throws the *matter* invariably away, he contents himself with forces given in position in space, with the requisite numerical multiples, and he investigates simply the relations and mutual actions of pure *force-points*, such as centres of gravity and the like: then he contracts and predicts the facts of the universe.

The Materialist gains nothing by the evidence of the mathematician. But he returns to the charge, thus you have been talking nonsense about that table, in pretending that the matter can be removed while all the forces remain; for matter exists in ultimate atoms, each constituted by an indefinite force of cohesion of its parts. Wherefore, if the atoms are supposed to be removed, their internal forces are removed from the locus along with them.

So here are the dear little atoms—their fairy troops are brought into the field. There is some poetry in the superstitions of the credulous Materialist; but poetry is not quite science. I deny the existence of atoms, and demand proof of it. Experimental proof will be difficult in the case of such small commodities; can we expect it from analysis? Will there ever be a proof of an indivisible absolute minimum of matter? Do you expect a set of formulæ to force up, demonstrating the impossibility of the existence in the universe of a bit of matter of less than a certain definite weight and diameter? That may do for the Materialists, but not for the mathematicians.

If I must believe in matter, I must also believe, not that it is infinitely *divisible*—for that is absurd; but that it is, like the space which it occupies at this moment infinitely *divided;* so that every mathematical point, or mere volume of space, is occupied in a material locus by a mathematical point, or zero mass, of matter. The difficulty of affirming an actual infinite division is no greater in the case of matter than in the case of space; and we are compelled to affirm this in space. Wherefore I did not talk nonsense about the table. If there is matter, its ultimate atoms can be nothing but zero masses, each occupying a zero vacuum of space, which masses, having no parts, can be constituted by no internal forces of cohesion.

In conclusion, I venture to defy the Materialist to deduce any experimental or logical demonstration of the existence of his matter the seat of force, or bring proof of it beyond his own unsupported assertion. In these notions there is nothing new; nor am I aware that their adoption can make any change in the habits or symbols of our scientific thought, except only with reference to the greatest topic in philosophy, the cause of the Universe. I think that I have cleared abundant room for this observation: that, while it is confessedly difficult for the Theist to prove, by a merely intellectual process, the existence of his God,—it is *not* a whit less difficult for the Materialist to prove, by any process whatever, the existence of his matter.

## PHYSICAL AND MATHEMATICAL SECTION.

### October 13th, 1864.

Joseph Baxendell, F.R.A.S., President of the Section, in the Chair.

Mr. **Vernon**, F.R.A.S., read a letter from Mr. James Taylor, Secretary of the Oldham Corporation Gas and Water Works, enclosing the following returns of Rainfall at Oldham, Strines Dale, and Brushes Clough. Mr. Taylor states that "the gauge at Oldham is fixed at the Gas Works, about four hundred yards from the centre of the town. Strines Dale is one of our reservoirs about two miles north-east of Oldham. Brushes Clough is the site of another of our reservoirs about three miles north of Oldham."

### RAINFALL AT OLDHAM.

Gauge—6 feet above ground, and 600 feet above the sea.

| | 1860. | | | 1861. | | |
|---|---|---|---|---|---|---|
| | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. |
| | Inches. | | Inches. | Inches. | | Inches. |
| January ...... | 2·55 | 25 | 21st—0·38 | 0·27 | 8 | 24th—0·06 |
| February...... | 0·89 | 16 | 8th—0·21 | 1·99 | 17 | 8th—0·57 |
| March ......... | 3·77 | 25 | 28th—0·50 | 4·46 | 28 | 2nd—0·70 |
| April ......... | 1.37 | 11 | 8th—0·32 | 0·87 | 6 | 1st—0·36 |
| May............ | 3·18 | 18 | 26th—0·50 | 1·02 | 8 | 25th—0·28 |
| June ......... | 6·30 | 25 | 2nd—0·92 | 2·14 | 16 | 21st—0·36 |
| July............ | 2·71 | 13 | 29th—0·50 | 3·95 | 27 | 11th—0·53 |
| August ...... | 4·48 | 28 | 10th—0·53 | 2·63 | 20 | 22nd—0·55 |
| September ... | 2·52 | 20 | 16th—0·66 | 4·33 | 17 | 8th—0·72 |
| October ...... | 4·47 | 24 | 10th—0·72 | 1·16 | 15 | 24th—0·23 |
| November ... | 2·51 | 17 | 21st—0·53 | 4·28 | 24 | 25th—1·04 |
| December ... | 1·63 | 16 | 6th—0·86 | 1·56 | 15 | 6th—0·51 |
| | 36·38 | 238 | | 28·66 | 201 | |

| | 1862. | | | 1863. | | |
|---|---|---|---|---|---|---|
| | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. |
| | Inches. | | Inches. | Inches. | | Inches. |
| January ...... | 2·52 | 20 | 31st—0·49 | 5·81 | 21 | 1st—1·81 |
| February...... | 0·73 | 10 | 1st—0·38 | 1·46 | 16 | 4th—0·31 |
| March ......... | 3·95 | 18 | 24th—1·13 | 1·01 | 16 | 7th—0·32 |
| April ......... | 3·53 | 15 | 2nd—0·40 | 1·57 | 17 | 5th—0·35 |
| May............ | 5·70 | 22 | 7th—1·08 | 1·94 | 16 | 11th—0·62 |
| June ......... | 3·49 | 25 | 13th—0·44 | 4·41 | 22 | 10th—1·46 |
| July............ | 4·43 | 21 | 31st—0·94 | 1·76 | 8 | 21st—0·83 |
| August ... .. | 3·22 | 16 | 8th—0·94 | 4·96 | 24 | 31st—0·89 |
| September ... | 5·17 | 17 | 3rd—1·22 | 6·41 | 26 | 21st—1·03 |
| October ...... | 5·78 | 20 | 12th—0·82 | 6·05 | 26 | 30th—1·17 |
| November ... | 1·68 | 15 | 9th—0·31 | 3·43 | 21 | 3rd—0·73 |
| December ... | 3·36 | 23 | 9th—0·78 | 3·22 | 21 | 2nd—0·72 |
| | 43·56 | 222 | | 42·03 | 234 | |

## RAINFALL AT STRINES DALE.

Gauge—6 feet above ground, and 800 feet above the sea.

| | 1859. | | | 1860. | | |
|---|---|---|---|---|---|---|
| | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. |
| | Inches. | | Inches. | Inches. | | Inches. |
| January ...... | 1·53 | 14 | 17th—0·52 | 2·81 | 26 | 19th—0·28 |
| February...... | 2·06 | 19 | 26th—0·34 | 0·55 | 10 | 8th—0·16 |
| March ......... | 3·45 | 22 | 14th—1·12 | 2·80 | 23 | 28th—0·49 |
| April ......... | 2·79 | 18 | 25th—0·42 | 0·75 | 14 | 1st—0·21 |
| May............ | 0·48 | 3 | 7th—0·27 | 3·09 | 18 | 12th—0·47 |
| June ......... | 2·44 | 16 | 5th—0·52 | 7·73 | 27 | 2nd—1·38 |
| July............ | 2·56 | 13 | 22nd—0·65 | 2·63 | 16 | 29th—0·48 |
| August......... | 6·91 | 17 | 7th—2·26 | 5·10 | 28 | 5th—0·72 |
| September..... | 5·62 | 26 | 30th—0·94 | 3·10 | 21 | 29th—0·48 |
| October........ | 3·48 | 19 | 25th—0.91 | 4·47 | 24 | 15th—0·94 |
| November..... | 2·31 | 16 | 1st—0·51 | 3·35 | 22 | 21st—0·75 |
| December...... | 3·05 | 17 | 4th—1·02 | 2·14 | 20 | 6th—1·07 |
| | 36·68 | 200 | | 38·52 | 249 | |

| | 1861. | | | 1862. | | |
|---|---|---|---|---|---|---|
| | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. |
| | Inches. | | Inches. | Inches. | | Inches. |
| January ...... | 0·84 | 16 | 16th—0·28 | 2·39 | 22 | 31st—0·38 |
| February...... | 2·11 | 17 | 8th—0·59 | 0·78 | 13 | 1st—0·17 |
| March ......... | 4·84 | 28 | 2nd—0·96 | 3·80 | 16 | 24th—1·13 |
| April ......... | 1·08 | 9 | 1st—0·39 | 2·81 | 18 | 6th—0·47 |
| May............ | 1·14 | 11 | 24th—0·29 | 5·41 | 23 | 7th—1·24 |
| June ......... | 2·55 | 16 | 23rd—0·47 | 3·29 | 24 | 13th—0·47 |
| July ......... | 3·93 | 27 | 21st—0·59 | 4·53 | 20 | 15th—0·44 |
| August ...... | 2·16 | 21 | 22nd—0·45 | 2·81 | 18 | 7th—0·88 |
| September ... | 4·13 | 20 | 8th—0·75 | 4·74 | 16 | 3rd—1·26 |
| October ...... | 1·70 | 17 | 11th—0·44 | 4·59 | 23 | 15th—0·58 |
| November ... | 4·32 | 23 | 25th—0·85 | 1·27 | 18 | 14th—0·23 |
| December ... | 1·81 | 17 | 6th—0·67 | 2·65 | 25 | 9th—0·76 |
| | 30·61 | 222 | | 39·07 | 236 | |

| | 1863. | | |
|---|---|---|---|
| | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. |
| | Inches. | | Inches. |
| January ...... | 5·02 | 25 | 1st—1·54 |
| February...... | 0·99 | 15 | 4th—0·18 |
| March ......... | 1·16 | 18 | 7th—0·28 |
| April ......... | 1·47 | 19 | 5th—0·36 |
| May............ | 1·87 | 15 | 14th—0·33 |
| June ......... | 4·32 | 22 | 10th—1·46 |
| July............ | 1·87 | 13 | 21st—0·95 |
| August ...... | 5·27 | 24 | 31st—0·86 |
| September ... | 5·78 | 24 | 21st—0·90 |
| October ...... | 5·99 | 24 | 30th—1·08 |
| November ... | 2·60 | 22 | 3rd—0·40 |
| December ... | 2·53 | 22 | 8th—0·46 |
| | 38·87 | 243 | |

## RAINFALL AT BRUSHES CLOUGH.

Gauge—6 feet above the ground, and 950 feet above the sea.

| | 1862. | | | 1863. | | |
|---|---|---|---|---|---|---|
| | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. | Rain Fall. | No. of Days' Rain. | Maximum Daily Rain Fall in each Month. |
| | Inches. | | Inches. | Inches. | | Inches. |
| January ...... | 3·19 | 19 | 31st—0·53 | 6·41 | 25 | 1st—1·56 |
| February...... | 1·01 | 16 | 1st—0·19 | 2·12 | 19 | 1st—0·34 |
| March ......... | 4·29 | 21 | 24th—1·12 | 1·43 | 19 | 7th—0·29 |
| April ......... | 4·04 | 18 | 5th—0·75 | 1·93 | 19 | 5th—0·54 |
| May............ | 6·49 | 22 | 7th—1·41 | 2·80 | 18 | 11th—1·01 |
| June........... | 4·31 | 27 | 12th—0·50 | 4·40 | 17 | 11th—1·02 |
| July........... | 5·69 | 23 | 31st—1·05 | 1·97 | 9 | 21st—0·95 |
| August......... | 3·33 | 17 | 7th—0·95 | 5·23 | 22 | 27th—0·58 |
| September ... | 5·45 | 19 | 3rd—1·39 | 7·31 | 27 | 21st—1·33 |
| October ...... | 6·56 | 23 | 12th—0·83 | 7.40 | 25 | 30th—1·38 |
| November ... | 1·85 | 17 | 9th—0·42 | 3·39 | 22 | 3rd—0·75 |
| December ... | 4·66 | 26 | 9th—0·96 | 3·80 | 21 | 2nd—0·85 |
| | 50·87 | 248 | | 48.19 | 243 | |

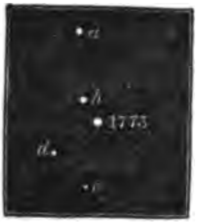
Mr. **Baxendell** read the following "Note on a New Star near the Greenwich Variable, No. 1773 of the 12-year Catalogue."

On the 13th of June, 1861, I made a diagram of the Variable Star No. 1773 of the Greenwich 12-year Catalogue, and three small companion stars as seen with Mr. Worthington's 13-inch Newtonian Reflector, and estimated the magnitudes of the companions to be $a=13{\cdot}2$, $b=13{\cdot}2$, and $c=13{\cdot}7$. No other stars were visible at that time within an area round 1773 having a radius equal to the distance of the companion $a$, the limit of visibility being the 14·5 magnitude; and the accuracy of the diagram was verified on several subsequent nights.

On the night of the 2nd of October instant, while on a visit with my friend Mr. B. D. Naylor, F.R.A.S., at Bowdon, Cheshire, I turned his newly-mounted achromatic equatorial of 6·2 inches aperture, by Cooke and Sons, of York, on 1773, and observed three small stars near it which I took to be the objects I had laid down in my diagram of June 13, 1861; but it afterwards occurred to me that the star *c* was not in the position shown in the diagram, and therefore on the night of the 6th instant, I directed Mr. Worthington's 13-inch reflector upon 1773, when I at once saw that the star I had taken to be *c* was in reality a new object, all the three stars seen on the 13th June, 1861, being in the positions shown in the diagram, though, strangely enough, *c* was about half a mag. less bright than I had estimated it to be at the time the diagram was made. The magnitude of the new star *d* was estimated to be 13·3. Using the 13-inch reflector again on the night of the 7th instant, careful estimations gave the magnitude of the four small stars $a=13{\cdot}2$, $b=13{\cdot}1$, $c=14{\cdot}1$, $d=13{\cdot}3$.

The sky, for a short time before midnight on the 9th instant, was unusually clear to the west, and the three companions *a*, *b*, and *d*, were seen with the 5-inch achromatic, and their magnitudes, determined photometrically, were found to be, $a=13{\cdot}1$, $b=12{\cdot}8$, $d=13{\cdot}2$.

The relative positions of 1773 and its companion stars are shown in the annexed diagram. The distance of the new star *d* from 1773 is about 48,″ and its angle of position about 315°.



The place of 1773 for 1865 is

| | H. | M. | S. |
|---|---|---|---|
| R.A. | 19 | 42 | 51·6 |
| Dec. | 26° | 57′ | 8·6″ N. |

From some observations made with the 5-inch achromatic in September, 1863, it appears that *b* was at that time at least *three-tenths* of a magnitude less bright than *a*, which was then estimated to be of the same magnitude as at present. The variability of 1773 was discovered at the Royal Observatory, Greenwich, in 1837, by Messrs. Rogerson and Glaisher; and as *d* was certainly invisible, or at least below the 14·5 magnitude in June, 1861, and *c* is now about half a magnitude less bright than it was at that time, while *b* on the other hand is a full half magnitude brighter than it was in September, 1863, we have here *four* variable stars within an area of little over one square minute, and it is to be remarked that the place of Anthelme's new star of 1670 precedes this singular and interesting group with a difference of right ascension of only about 48 seconds.

## Ordinary Meeting, November 1st, 1864

R. Angus Smith, Ph.D., F.R.S., &c., President, in the Chair.

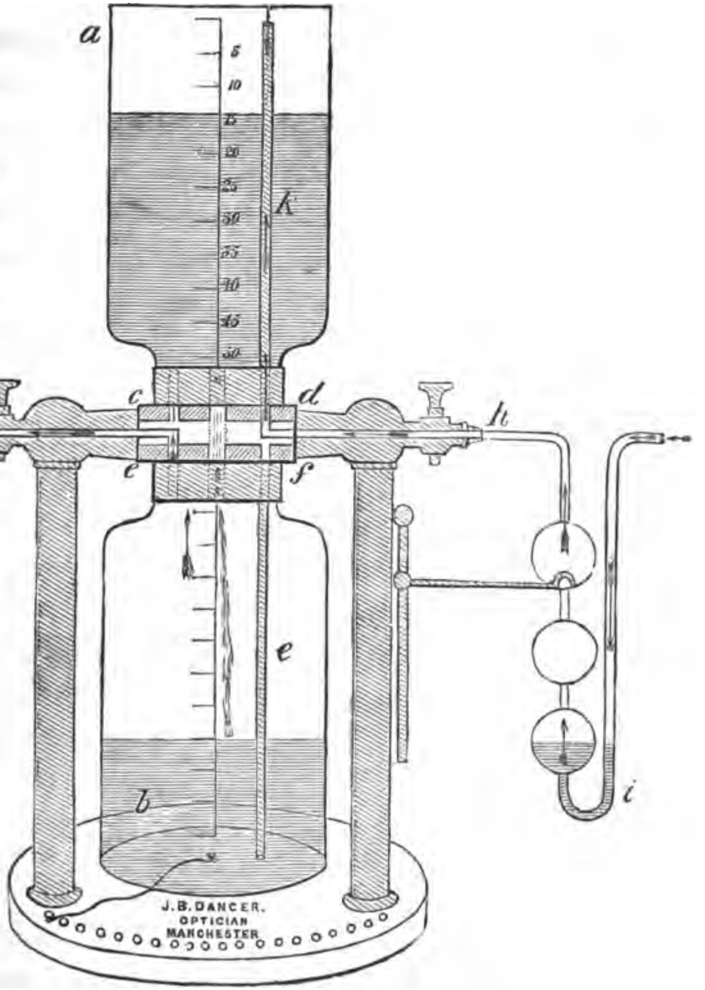
The following gentlemen were elected Ordinary Members of the Society:—Mr. William Cort Wright, F.C.S.; Mr. George Heppel, M.A.; and Mr. William Mather.

Mr. **Sidebotham** said that he had noticed the common statement that Beech Trees were never damaged by lightning. He had been induced to collect facts on the subject, and had found that out of 28 instances, the trees struck were—Oak, 9; Poplar, 7; Ash, 4; Willow, 3; Horse Chestnut, 1; Chestnut, 1; Walnut, 1; Thorn, 1; Elm, 1, respectively.

Mr. **Binney** remarked that strokes of lightning were generally determined by the nature of the subsoil, and that in certain localities thunderstorms were very destructive, while in others they were comparatively harmless, and damage by lightning hardly ever occurred. The Beech, it was well known, was generally found growing upon dry, sandy soils, which were bad conductors of electricity, and which therefore acted as protectors against destructive lightning discharges.

The PRESIDENT gave an account of an aspirator which had been contrived for him by Mr. J. B. Dancer, of Cross-street, to be used in the analysis of mixed gases, &c. A drawing accompanies this description. Two jars graduated into parts of a cubic foot or according to pleasure, are mounted in brass, and placed mouth to mouth on an axis *g*, *h*, or the white portion enclosed by *c*, *d*, *e*, *f*. The upper jar is filled with water and the taps opened; then the water flows from *a* to *b*, the air or gas entering by *h*, and passing previously through any solution that may be used, as at *i*. The gas enters *a* by the tube *k*, the air in *b* goes out by *c*. When *a* is emptied, it is simply turned round by the hand, and *b* the filled bottle stands uppermost. The shaded part of *c*, *d*, *e*, *f*, revolves with the jars, and the openings are so made as to form continuations of the openings in the axis *g*, *h*. The same conditions exist, no matter which jar is uppermost. This apparatus is very convenient in a laboratory, and has an advantage over other aspirators in measuring the gas. The measurements on the jars are made at a definite pressure of water, and this ought of course to be maintained when the numbers are read off. The apparatus is certainly very elegant, and is an ornamental as well as useful addition to a chemist's work table. It may be called Dancer's Aspirator, or the Swivel Aspirator.

Dr. Boswell Reid describes one somewhat resembling this, but instead of having the swivel movement, it was necessary to lift the whole apparatus and to invert it. It was in reality two aspirators; one emptied itself into the other.


a
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J. B. DANCER,
OPTICIAN
MANCHESTER

Dr. Joule described the process he employed to harden steel wires for magnetic needles. The wire was held stretched between the ends of two iron rods bent into a semicircular shape. The free ends of the iron rods could be placed in connexion with a voltaic battery by means of mercury cups. Underneath the steel wire a trough of mercury was placed. When the ends of the iron rods dip into the cups the current passes through the wire, heating it to any required extent. When these ends are lifted the current is cut off, while at the same instant the heated wire is immersed in the trough of mercury.

## CORRIGENDA.

Mr. Kirkman begs the reader to make the following corrections in the last No. of these Proceedings:—

Page 14, line 11, for 'unexciting,' read *unresisting*.
" 15, line 1, for 'existence,' read *resistance*.
" " line 22, for 'forces,' read *forms*.
" 16, line 27, for 'centre,' read *centres*.
" 17, line 2, for 'multiples,' read *multipliers*.
" " line 4, for 'then he contracts,' read *thus he constructs*.
" " line 21, for 'force,' read *turn*.
" " line 3 from bottom, for 'mere,' read *zero*.
" 18, line 5, for 'vacuum,' read *volume*.
" " line 8, for 'deduce,' read *adduce*.
" " line 10, for 'bring,' read *any*.

## Ordinary Meeting, November 15, 1864.

R. Angus Smith, Ph.D., F.R.S., &c., President, in the Chair.

The following letter from Mr. James Nasmyth, C.E., addressed to Mr. Joseph Sidebotham, and dated November 8th, was read:—

I had intended to have sent the Manchester Literary and Philosophical Society "a Paper," embodying some ideas I entertain in regard to the vast antiquity of the features and details of the lunar surface, but in attempting to put my views on this subject into the formal shape of "a Paper," somehow or other my pen won't say what I want it to express, so I am fain to get out of the difficulty by sending you an abstract of my views on this subject in the form of a letter.

The views I entertain on the subject in question are these, namely, that as a direct consequence of the small mass of the moon, and its comparatively large surface, it must have parted with its original cosmical heat with much greater rapidity than in the case of the earth, and consequently the moon must have assumed a final condition of surface structure ages before the earth had ceased from its original molten condition. And as the moon had in all reasonable probability never possessed an atmosphere or water envelope (it certainly has none such now), while the earth has both, the action of the earth's atmosphere, and especially that of its ocean when it existed in the first instance as a vast vapour envelope, ere the earth had cooled down so as to permit the ocean taking up its final position *as an ocean*, this mighty vapour envelope must have retarded the escape into space of the cosmical heat of the earth millions of ages after the moon had assumed its final condition as to temperature.

Therefore it is from such considerations I am led to the conclusion that the surface features and details of the moon present to us a sight of objects the antiquity of which is so vast as to be utterly beyond the power of language to express, and scarcely less so for the mind to conceive.

But yet at the same time such considerations appear to me to enhance so vastly the deep interest which ever attends the examination and contemplation of the moon's wonderful surface, that I would earnestly urge those who agree with the soundness of these views to bear them in mind next time they have an opportunity to behold the marvellous details of the lunar surface, as I am fain to think that in doing so the interest of what is there revealed to them will be rendered vastly more impressive.

The PRESIDENT read a paper "On the Composition of the Atmosphere." He believed that his inquiry proved that the oxygen test was a very valuable one, as indicating the condition of the atmosphere. The oxygen was diminished in many cases, and indeed in all cases where the air was known to be inferior. He said the objection to such air may perhaps be found not so much in the absence of oxygen as in the gases which take its place. That place was not wholly supplied by carbonic acid. He believed it needful to examine the composition to the second decimal place in the case of oxygen, and to the third or even fourth in the case of carbonic acid, as extremely small amounts of some gases affect us.

Hitherto we have had the composition of the air given in numbers varying a tenth per cent—specimens have generally been taken from rooms on streets or open places indiscriminately. It is the author's wish to show that variations are dependent on the conditions of soil, situation, wind, &c., and that the oxygen and carbonic acid together may with very minute analysis guide us in our sanitary inquiries. The paper cannot easily be given in abstract, further than by adding a table of the analyses showing the average numbers obtained in various places:—

### ANALYSES OF ATMOSPHERES VARYING IN OXYGEN.

| | Oxygen per cent. Avge. Nos. |
|---|---|
| N.E. sea shore and open heath (Scotland) | 20·999 |
| Tops of hills (Scotland) | 20·98 |
| In a suburb of Manchester in wet weather | 20·98 |
| Ditto ditto ditto | 20·96 |
| Front of street ¾ths of mile from Exchange, Manchester | 20·945 |
| At the back part of the house | 20·936 |
| Low parts of Perth | 20·935 |
| Swampy places (favourable weather) | 20·922 to 20·95 |
| In fog and frost in Manchester | 20·91 |
| In sitting room which felt close, but not excessively so | 20·89 |
| In a small room with petroleum lamp, well ventilated | 20·84 |
| Ditto after six hours | 20·83 |
| Pit of Theatre, 11 30 p.m. | 20·74 |
| Gallery, 10 30 p.m. | 20·36 |
| In large cavities in mines | 20·77 |
| In currents | 20·65 |
| Under shafts | 20·424 |
| In sumps | 20·14 |
| When candles go out about | 18·5 |
| The worst specimen yet examined in the mine | 18·27 |
| Very difficult to remain in for many minutes | 17·2 |

## ANALYSES OF ATMOSPHERES VARYING IN CARBONIC ACID.

| | Avge. of Carbonic Acid per cent. |
|---|---|
| Manchester streets, usual | 0·0403 |
| During fogs | 0·0679 |
| About middens | 0·0774 |
| Average | 0·0442 |
| Fogs excepted | 0·0424 |
| Fogs and middens excepted | 0·0403 |
| Where the fields begin | 0·0369 |
| In close buildings | 0·1604 |
| Minimum of suburbs | 0·0291 |
| Over North Scotland (towns excepted) | 0·0336 |
| Candle goes out | 1·8 to 2·5000 |
| Lowest found in mines | 2·5000 |
| Lowest entered | 4·0000 |

The greater part of this is given in Dr. Angus Smith's Report "*On the Air of Mines.*"—Appendix to Report of the Royal Mines Commission, 1864.

## MICROSCOPICAL SECTION.

### First Ordinary Meeting, Session 1864-5.

17th October, 1864.

**Joseph Sidebotham**, Esq., President of the Section, in the Chair.

The **President** stated that he regretted to have to inform the members of the total failure of the efforts made during the last summer to provide them with fresh cotton pods for the purpose of investigating into the structure of the cotton fibre. This was partly owing to ravages of the common greenhouse pest, the red spider, and partly unaccounted for as the plants had flowered but not fruited. He called the attention of the meeting to the compact form of microscope made by Mr. Dancer to facilitate seaside and other investigations, where portability, combined with means of using the higher powers, was the chief desideratum. A specimen was on the table, and he and other members could bear testimony to its advantages.

He also called the attention of the members to the many beautiful forms of insects and vegetable life which were frequently neglected as being too small to be examined by the unaided eye, and yet too large for the ordinary powers of the microscope. He assured the members the use of the present three or two inch object glasses would reveal to them many objects of surpassing beauty, which had hitherto only been studied in detail.

With regard to the use of such powers as the $\frac{1}{16}$th or $\frac{1}{25}$th, he thought they seemed to have reached the limits of the available power of microscopic object glasses, as it appears impossible to separate or define lines more numerous than ninety thousand in an inch, on account either of the decomposition of light or some other cause. It therefore seems beyond our power ever to discover more of the ultimate composition of matter by aid of the microscope, even were we not prevented by the material composition of our lenses and organs of vision.

We have, however, penetrated to the very confines of organic life, if not beyond, inasmuch as no organisms appear to exist smaller than those we can already see.

It is, moreover, a curious fact that the smaller creatures are composed of fewer elements than the larger ones, and that the number of elementary bodies composing them decrease in number as the organisms themselves decrease in size. It becomes therefore a matter for speculation whether the reason of this may not be, that the ultimate atoms of some elementary bodies are larger than others, and that these, from their size, cannot be used in the composition of the more minute forms of organic bodies, and that smaller organisms than those about $\frac{1}{78000}$th of an inch do not exist, because the ultimate atoms of all solid bodies are too large to be economically used in their formation. The telescope appeared to have infinite fields of distance to explore, but it would seem the microscope had nearly reached the limits of its possible power.

Mr. J. B. DANCER, F.R.A.S., then read a paper "On a contrivance for regulating the amount of light transmitted from the source of illumination to the mirror of the microscope."

When viewing certain objects by transmitted light, and particularly with oblique illumination, a very slight alteration in the quantity and direction of the light produces a marked difference in the appearance of the object, especially in Diatomaceæ, where a proper management of the light shows lines or markings invisible under ordinary direct illumination.

The apparatus now exhibited is one easily made at a trifling cost, and consists of a circular disc of blackened tin or cardboard ten or twelve inches in diameter, with a number of perforations of various shapes and sizes—circular, cross-shaped, wedge-shaped, &c.—the centres of which are about 3½ inches from the centre on which the disc, placed perpendicularly, rotates. The form of perforations found generally most useful are parallel slits—slits at right angles to each other—wedge-shaped and circular openings.

The object under view must be well illuminated in the direction required, and then the disc, supported by a pillar, is placed between the source of light and the concave mirror, when a few trials will determine the best form of aperture.

The markings of Pleurosigma fasciola, angulatum, &c., may be seen by its aid under powers which would not show them with any arrangement of achromatic condensers, and it also has the good property of shading all but the amount of light required from the lower portion of the microscopic stage and stand.

The disc might be attached to the lamp, but it appears to work better on a stand, and is susceptible of various modifications which will readily suggest themselves to the microscopist.

Mr. W. H. **Heys** exhibited specimens of leaves of the vegetable marrow, showing reticulated markings somewhat similar to those of Symphytum, and presented a slide to the cabinet.

Mr. **Sidebotham** stated that in sweeping over herbage for Coleoptera and other insects, he had found some very curious seeds, to one of which, that of Sanicula Europea, he thought attention had not hitherto been drawn, though well deserving of it. Those of Henbane and Daucus were also most singular.

Mr. **Linton** exhibited the elegant tufted stigmas of Poterium sanguisorba, and the very singular calyx of the gum Cistus, which might almost be mistaken for the skin and scales of a fish.

## Ordinary Meeting, November 29th, 1864.

R. ANGUS SMITH, Ph.D., F.R.S., &c., President, in the Chair.

Mr. E. C. Buxton was elected an Ordinary Member of the Society.

Dr. JOULE exhibited a magnetic needle for showing rapid and minute alterations of declination. It consisted of a piece of hardened and polished watch spring, an inch long and one-tenth of an inch broad, suspended vertically by a filament of silk. The steel was magnetised in the direction of its breadth. He remarked that Professor Thomson had long insisted upon the advantages which would attend the use of very small bars in most magnetical investigations, and had employed excessively minute needles in his galvanometers with great success. Dr. Joule stated his intention to fit up his needle so as to be observed by light reflected from its polished surface, or otherwise by viewing a glass pointer, attached to the bottom of the steel, through a microscope. He believed that by the latter plan he should be able to observe deflections as small as 1″ of arc.

A paper was read "On Differential Equations," by His Honour Chief Justice Cockle, M.A. F.R.A.S., F.C.P.S., &c., President of the Philosophical Society of Queensland; communicated by the Rev. Robert Harley, F.R.S., &c.

Importing an arbitrary constant C into a result which Mr. Robert Rawson, an Honorary Member of the Manchester Literary and Philosophical Society, in a letter dated Portsmouth, June 14th, 1862, communicated to me, we may say that the differential resolvent of a (trinomial) cubic, whereof the coefficient of the second term vanishes and that of the third is constant, is of the form

$$\frac{d^2y}{dx^2}-\frac{f''(x)}{f'(x)}\frac{dy}{dx}-\mathrm{C}\left\{f'(x)\right\}^2y=0\ldots\ldots\ldots(1)$$

Put

$$f'(x)=\frac{df(x)}{dx}=\mathrm{X},\quad \mathrm{C}=\mathrm{K}^2,$$

then, (1) divided by $\mathrm{X}^2$ becomes

$$\frac{1}{\mathrm{X}^2}\cdot\frac{d^2y}{dx^2}-\frac{\mathrm{X}'}{\mathrm{X}^3}\cdot\frac{dy}{dx}-\mathrm{K}^2y=0\ \ldots\ldots\ldots\ldots(2)$$

Now, if

$$\mathrm{L}=\pm\,\mathrm{K},\ \mathrm{M}=\mp\mathrm{K},$$

then

$$\left(\frac{1}{\mathrm{X}}\cdot\frac{d}{dx}+\mathrm{L}\right)\left(\frac{1}{\mathrm{X}}\cdot\frac{d}{dx}+\mathrm{M}\right)y=0\ldots\ldots\ldots(3)$$

is the symbolical decomposition of (1) or (2). When L and M are any constants whatever, (3), when developed, gives rise to a linear differential equation of the second order reducible to an equation with constant coefficients by changing the independent variable from $x$ to $t$, where

$$t=\int\frac{dx}{\mathrm{X}^{-1}}=\int\mathrm{X}dx$$

In deducing from (3) its development, the order of the symbolical factors is indifferent, but the two particular integrals of the development are, I think, obtainable by reversing in (3) the order of the symbolical factors. The differential resolvent of every such trinomial cubic as that discussed by Mr. Rawson is soluble by a change of the independent variable, and belongs moreover to a comparatively simple form of equation soluble by such change.

The theory of Transcendental Solution has led me to the following proposition (theorem):—

*If an irreducible algebraical equation of the degree $n$ have a homogeneous linear differential coresolvent of the order $m$, then any root whatever of the algebraical equation can be expressed as a linear and homogeneous function of any other $m$ of its root.*

The general demonstration would not be much more difficult than or very different from the particular demonstration of the case $m=2$. The converse of this theorem, I believe, is true.

In such case let $a$ and $b$ be the particular integrals of the differential resolvent which (since $m=2$) is by hypothesis of the second order only. Let $\alpha$, $\beta$, and $\gamma$ be any three of the roots of the algebraical equation. Then, since among the values that can be assigned, by means of the arbitrary constants, to the general integral, the roots of the algebraical equation are included, we have three such relations as

$$\mathrm{A}a+\mathrm{D}b=\alpha,$$
$$\mathrm{B}a+\mathrm{E}b=\beta,$$
$$\mathrm{C}a+\mathrm{F}b=\gamma,$$

wherein A, B, .., F are constants. Multiplying the first, second, and last of these equations into the arbitrary multipliers $\lambda$, $\mu$, and $\nu$, and adding the results, we have

$$(\lambda A + \mu B + \nu C)a + (\lambda D + \mu E + \nu F)b = \lambda\alpha + \mu\beta + \nu\gamma \ldots\ldots(4)$$

Hence if the ratios of any two of the quantities $\lambda$, $\mu$, $\nu$ to the third be so assigned as to satisfy the equations

$$\lambda A + \mu B + \nu C = 0,$$
$$\lambda D + \mu E + \nu F = 0,$$

then the sinister of (4) will vanish independently of $a$ and $b$, and the homogeneous linear relation

$$\lambda\alpha + \mu\beta + \nu\gamma = 0 \ldots\ldots\ldots(5)$$

will subsist among the roots $\alpha$, $\beta$, and $\gamma$ of the algebraical equation. When $n = 3$ we (since the differential resolvent is homogeneous) have without reference to, but consistently with, the theorem,

$$\alpha + \beta + \gamma = 0 \ldots\ldots\ldots(6)$$

Combining the above theorem with one given by Abel and Galois, we conclude that:—

*If an algebraical equation have a differential resolvent of the second order, the algebraical equation is resoluble algebraically.*

Before closing I would add that, as it seems to me, it would be more consonant with the notation and practice of the rule of three, and, therefore, with convenience and the analogies of arithmetic, if by the ratio $p : q$ there were universally understood (not the fraction $p \div q$, but) the fraction $q \div p$.

Mr. R. D. **Darbishire**, F.G.S., read a paper entitled "Notes on Marine Shells found in Stratified Drift at Macclesfield," and exhibited a series of specimens.

The specimens were chiefly collected by Mr. W. J. Sainter and Mr. Lowe, of Macclesfield, from sand and gravel exposed in the formation of the new Cemetery on the north side of the town, at an elevation of between 500 and 600 feet above the level of the sea. Unfortunately the buying of specimens had caused the intrusion of many spurious fragments, casting suspicion on several that might after all prove to be genuine.

The beds in question were exposed on a south-easterly face, but are now defaced by ballast tips; consist of fine (running) sand, fine and coarse shingle, and very coarse gravel with large pebbles unscratched; and, while stratified, in general horizontally, exhibit in their great irregularities of extension, level and false bedding, characteristically marine aspect, as of a sea bottom under the influence of tidal and other varying currents.

Below appears the "lower boulder clay" of the Ordnance geologists.

The shells are nowhere numerous. Mr. Lowe speaks of finding some in layers. Unfortunately the shells from particular beds have not been distinguished.

In the list specimens obviously spurious have not been noticed. The following species had been identified:—

Pholas crispata.
Pholas candida.
Mya truncata.
Mya arenaria.
Psammobia ferroensis.
Donax anatinus.
Tellina solidula.
Mactra solida.
Lutraria elliptica.
Cytherea chione.
Venus striatula.
Artemis lincta.
Cyprina islandica.
Astarte elliptica.
Astarte arctica.
Cardium echinatum.
Cardium aculeatum (?)
Cardium rusticum.
Cardium edule.
Cardium Norvegicum.
Mytilus edulis.
Modiola modiolus.
Nucula sp.
Arca lactea.
Pectunculus sp.
Pecten opercularis.
Ostrea edulis.
Patella vulgata.
Dentalium entale.
Dentalium Tarentinum.
Trochus cinerarius.
Littorina littorea.
Littorina rudis.
Littorina littoralis.
Turritella communis.
Aporrhais pes pelicani.
Natica nitida.
Natica monilifera.
Murex erinaceus.
Purpura lapillus.
Nassa reticulata.
Nassa incrassata.
Buccinum undatum.
Fusus gracilis.
Fusus antiquus.
Trophon clathratum.
Mangelia turricula.
Mangelia rufa.
Mangelia nebula.
Cypræa Europæa.

Total, 50 species.

Mr. Darbishire compared this list with those of the Moel Tryfaen drift (Caernarvon), and the Kelsey Hill (Hull) fossils, and with several lists of recent faunas of the British

Eastern and Western seas, and of seas North and South of the British Isles.

The present list was specially remarkable for including *Cytherea chione, Cardium rusticum, Cardium aculeatum* (?), and *Arca lactea;* all of them shells reaching their highest northern range in the extreme south or west of England and Ireland, a circumstance believed to be new to the history of the so-called "Drift."

The Macclesfield list rendered probable the deposit of those beds from the westward, after the period had commenced during which the physical conditions of the Western Sea have differed as they now do from those of the Eastern.

A depression of 600 feet would leave only a few islands where Ireland is, would allow of a great extension of the tidal current now narrowed in St. George's Channel, and would probably carry the warmer influences which are now checked on the West of Ireland to the shores of the Derbyshire hills. Are there any traces of the sea bottom or shores on which the shells of the drift seas *lived?*

Mr. Darbishire further mentioned his identification in a bed of gravel discovered by Mr. Prestwich, F.G.S., at about 1,200 feet above the sea, on the east side of Macclesfield, of nine species of shells, *including Cytherea chione.*

Mr. Binney, F.G.S., remarked on the extent of the list, the great distance eastward from the present shores of the deposit, the elevation of Mr. Prestwich's patch, and the peculiarly temperate aspect of the group of shells. This drift could not be called "arctic." He also referred to the difficulty of recognising distinct "upper" and "lower" boulder clays; and illustrated the partial removal of the drift gravels and the underlying till over a large tract of western country during its elevation, and the re-distribution of portions of the same beds as eroded by water courses after the land had risen above the sea.

Dr. ALCOCK had examined the fossils produced, and especially the southern forms, and confirmed the identification of species.

## Ordinary Meeting, December 13th, 1864.

R. ANGUS SMITH, Ph.D., F.R.S., President, in the Chair.

Mr. Edward Sonstadt was elected an Ordinary Member of the Society.

Mr. BINNEY, F.R.S., exhibited some spores of plants found in the splint coal of Methill, Fifeshire. He said that many years ago he read a paper on some similar fossils before the Geological Society of London, and it was printed in the Journal of the Society for May, 1849. Those specimens were from a nodule found in the King Coal seam at Wigan. They also were met with in the Wigan Four Feet Coal in greater abundance. Since that time Professor Balfour, F.R.S. of Edinburgh, had described, in a paper printed in vol. xx of the Transactions of the Royal Society of Edinburgh, some similar fossils from the Fordel splint coal. The specimens exhibited were small lenticular bodies of a chestnut colour, about a line in diameter. They occurred in countless numbers, indeed forming a very considerable portion of the seam of coal itself. He stated that he had found them in equal abundance in the Eight Feet, Main, Wood, and Pirnie Well seams, but always in the splint or bone part of the coal. Dr. Hooker had proved that similar spores belonged to the Lepidodendron. The thick coating of the spore has doubtless afforded some protection to it as well as the peculiar process of bituminisation to which splint coal has been subjected, and different from that which soft or cherry coal has undergone. He said that when we considered the great abundance of these small fossils in all splint coals, and the immense number of the roots of Sigillaria found in the floors of such seams of coal, it was almost certain that they had some connection with that plant. This tended to confirm M. Adolphe Brongniart's opinion, expressed many years ago, that Sigillaria and Lepidodendron were plants very nearly allied to each other. Mr. Binney also exhibited some larger spores in a nodule of clay ironstone given to him by Mr. Ward, of Longton, and found in the Derbyshire coal field. These were in a most beautiful state of preservation, and exhibited the tesselated character of the outside of the spore.

A paper was read by Mr. WILLIAM BROCKBANK, "On the Discovery of the Bones of the Mammoth (Elephas primigenius) in a Fissure of the Carboniferous Limestone at Waterhouses, near Leek."

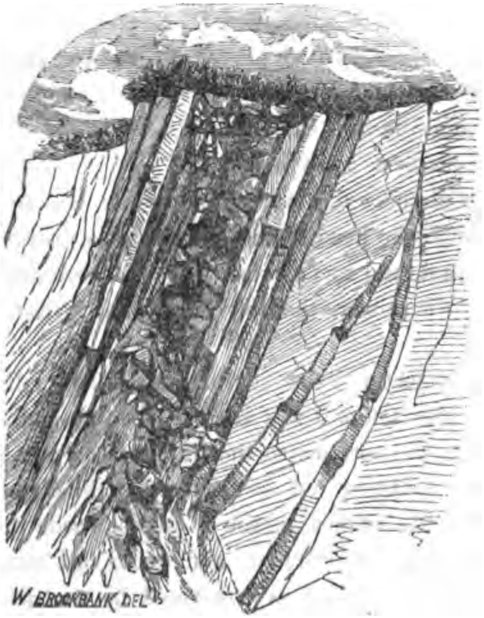
A considerable number of bones were found at Waterhouses some weeks since, but through ignorance of their real character they became dispersed without attracting attention, a good many having been used to manure the land by a neighbouring farmer.

A few of these bones reached the author through Mr. Smith, of Cheddleton Mills, and were at once identified as belonging to the skeleton of an elephant. A further search was determined upon, and the author, accompanied by Messrs. Thomas Wardle of Leek, and J. Walsh and W. Smith of Manchester, visited Waterhouses on the 9th instant, and succeeded in finding a large number of bones.

Mr. Wardle, and Mr. Green of the Geological Survey, again visited the place on the 12th instant, and found very decided fragments of teeth. A further search is being made at the present time.

A large number of bones were submitted to the Society, all of which were considered to be those of the Elephas primigenius, amongst which were one humerus nearly complete. and part of the second; parts of the pelvis and scapula; one ulna; several carpal and metacarpal bones; the head of the tibia; several fragments of tusks and two fine fragments of teeth, showing very clearly the peculiar narrow transverse plates and ridges of the dentine and enamel, by which the teeth of this elephant are distinguished.

The fissure in which these bones were found occurs in the upper beds of the carboniferous limestone, and has been exposed by the workings of a quarry. The limestone strata at Waterhouses are much dislocated, and fissures frequent, the river Hamps disappearing in dry weather through fissures very near to the above quarry, and other streams in the immediate neighbourhood were described as sinking in the same manner. The face of the limestone in the quarry is nearly parallel to the general direction of the valley, or nearly east and west, and the fissure follows the dip and direction of



OSSEOUS FISSURE AT WATERHOUSES.

the strata, being nearly vertical, or inclining about 10° to the north. It is about six feet in width from face to face of the solid rock, and is filled up with angular blocks of limestone, cemented together at the sides of the fissure into a solid breccia, the stones being coated with stalagmite, whilst the centre is filled in with angular rubble and damp ochreous clay. The whole had evidently been filled in from above.

The bones were recovered in good condition from the breccia on the dryer side of the fissure, but those occurring amongst the damp clay and rubble were so friable that it was quite impossible to save them. Large numbers af ivory flakes were found, which proved to be the remains of the teeth, and one large fragment of tooth was obtained which was decomposing into these flakes.

At the furthest point reached, a very interesting group of bones was discovered, viz., a humerus in the socket of the scapula, with the head of another humerus resting upon it at the other end, and two cervical vertebræ were found near the scapula. These were the only bones found in their relative positions.

It was conjectured that the mammoth had fallen into the narrow fissure before it was filled in, its huge bulk preventing its reaching the bottom, so that it remained jammed in until by natural decay it fell to the bottom, bit by bit. By this supposition the absence of the head was accounted for, as it would probably fall off first, and would roll lower down the chasm. This surmise is confirmed by the fact that Mr. Wardle found several fragments of the teeth on his second visit, fifteen or twenty feet below the point where the bones occurred.

The author had not been able to find any record of the occurrence of the remains of the mammoth in any work on the geology of Derbyshire or Staffordshire; and Mr. Wardle, who has recently published an interesting account of the geology of the neighbourhood of Leek, believed it to be an entirely new discovery. A considerable part of the fissure remains to be explored, and a further search is being prosecuted by Mr. Wardle and the author.

Dr. WHITE considered it probable that among the bones exhibited there were the remains of more than one animal.

Mr. BINNEY said that Mr. Brockbank was mistaken in supposing that no remains of the elephant had hitherto been found in Derbyshire. They had been met with in two localities in the county of Derby and one in the county of Chester. The late Mr. White Watson, at page 58 of his "Delineation of the Strata of Derbyshire," says:—"About the year 1663 a large cavern was discovered in sinking for lead ore upon a hill at Balleye, within two miles of Wirksworth; in which a large skeleton was found, which in the original account of its discovery is said to be 'that of a man, that his brain pan would have held two bushels of corn, and that it was so big they could not get it out of the mine without breaking it.' Several of its teeth were distributed in the neighbourhood, one of which, with the author's account of the discovery, is in the writer's possession. The tooth is ivory, and when compared with the *dentes molares* of an elephant, no difference can be found; from this circumstance it is evident that the skeleton found could not have been that of a man or giant, so called by the miners, who are ever prone to the marvellous, but must be indisputably that of an elephant, and its capacious brain pan a corresponding proof; for after the miners had conferred on it the appellation of the giant's tooth, the brain pan must naturally follow the proportions of its bulky owner. The fangs, though perfect at the time of the discovery, are now broken, and no change appears to have taken place from its original substance. Several of these teeth were brought out, but the skeleton left behind, which, it is to be lamented, cannot now be viewed, that part of the mine having run in, rendering it impracticable without much trouble and expense." About twenty-five years since the late

Mr. James Meadows, of the Ashton Canal Company, presented to the Manchester Geological Society a portion of the tusk of an elephant which he had found in a limestone fissure at Doveholes, near Chapel-en-le-Frith. This specimen is now in the museum of the Society in Peter-street. The late Mr. F. Looney, F.G.S., in his List of Organic Remains, published with Mr. Elias Hall's Geological Map, in 1836, in alluding to the fossils found in the "superficial gravel," says: "Part of a molar tooth of the Asiatic elephant was found at Adlington, near Macclesfield, and is now in the possession of William Clayton, Esq." With these three exceptions he had never heard of the remains of the elephant having been met with in the counties of Derby and Chester, and he felt much obliged to Mr. Brockbank for taking the trouble to collect the fine series of remains on the table, and exhibit them to the members of the Society.

## MICROSCOPICAL SECTION.

November 21st, 1864.

JOSEPH SIDEBOTHAM, Esq., President of the Section, in the Chair.

Mr. WATSON exhibited and presented to the Section a dozen slides showing the battledore scales of several species of Polyommatus or Lycæna; he also illustrated the subject by drawings of the scales made by Mr. Sidebotham. He showed that the scale of Bæticus has manifestly no relation to the others, though this insect is placed at the head of the list of the genus Polyommatus by Boisdudal, H. Schaeffer, Dr. O. Standinger, and W. F. Kirby. Many lepidopterists have thought that it is a Thecla, and he proposed to examine various species of the genus Thecla and to report the result.

A letter from Captain MITCHELL, of Madras, was read. He stated that he had examined fresh but full grown cotton treated with Mr. O'Neill's strong and weak solutions of copper, and had failed to detect any spiral fibres. He communicated some observations on a Lepidopterous larva, probably that described by Mr. Saunders in vol. iii. of the Transactions of the Entomological Society, which attacks the cotton pod. Wherever it was found, the cotton was entirely destroyed.

The SECRETARY read Mr. Edwards's directions for collecting Diatomaceæ.

## November 28th, 1864.

JOSEPH SIDEBOTHAM, Esq., President of the Section, in the Chair.

### *Specimens Exhibited.*

Physcomitrium sphoericum, by Mr. G. E. Hunt, found sparingly in three places on the borders of Mere Mere, this being only the second occasion on which it has been gathered in Britain. The first was thirty years ago, when it was detected in the same place by Mr. Wilson in the autumn of 1834.

Four species of Curculionidæ, new to Britain, by the President, as exhibited and examined at the Entomological Society, London; Lixus filiformis and Sibynes canus, Devizes; Ceuthorhynchidius poweri, Silverdale; and Peritelus griseus, Ventnor, Isle of Wight. Three of the species were captured by himself. [See *Zoologist,* December, 1864.]

A martin, entirely white, from Urmstone, by Mr. Linton.

### *Communications.*

Mr. G. E. HUNT announced his discovery of Potamogeton nitens in Loch Ascog, Rothsay. This plant was first observed as British by Mr. David Moore, Dublin, in a lake near the sea, at Castle Gregory, county of Kerry, in July last, [See *Seeman's Journal of Botany*, November, 1864.]

THOMAS ALCOCK, M.D., read a paper on specimens from Roundstone, Connemara. He exhibited many specimens, including forty-three species of Foraminifera, two of which were forms of Entosolenia, hitherto undescribed; also, young shells of Patella vulgata and P. pellucida, with the larval shells still attached and distinctly spiral, evidence not before recorded.

## PHYSICAL AND MATHEMATICAL SECTION.

November 10th, 1864.

**Joseph Baxendell**, F.R.A.S., President of the Section, in the Chair.

Professor **Clifton** exhibited an acoustical electric telegraph, by which a note, sounded at one end of the line, is reproduced at the other end.

He also pointed out the principles involved in the construction of this telegraph, viz.:—

1st, The production of a sound whenever a current of sufficient strength commences to circulate round an electro magnet, or ceases so to circulate.

2nd, The vibration of a stretched membrane in accordance with a note sounded near it.

With respect to the second principle, Professor Clifton drew attention to the fact, that the researches of MM. Bourget and Bernard, in agreement with the mathematical investigations of Poisson and M. Lamé, show that a given square membrane will *not* vibrate in accord with *any note*, as stated by Savart. As the same is probably true of circular membranes, such as that used in this telegraph, it follows that only certain notes are capable of being transmitted by one instrument.

## December 8th.

Robert Worthington, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. **Baxendell** read a "Note on the Period and Changes of the Greenwich Variable in Vulpecula, No. 1773 of the Twelve-Year Catalogue."

In No. 1500 of the *Astronomische Nachrichten*, Dr. Schönfeld, Director of the Observatory at Mannheim, expresses a doubt as to the variability of several of the stars in Mr. Chambers's Catalogue of Variable Stars. One of the objects which he thus points out as doubtful is No. 1773 of the Greenwich Twelve-Year Catalogue, which was entered in Mr. Chambers's list on my authority, as I had satisfied myself from occasional observations made since 1861, that its light was subject to periodical changes, though not to the extent indicated by the observations of Messrs. Rogerson and Glaisher, made in 1837. I have since reduced my observations, and have obtained from them the following times of maximum and minimum brightness:—

| Maxima. | Minima. |
|---|---|
| 1862, Oct. 20. | 1862, Nov. 24. |
| 1863, Sep. 12. | 1863, Oct. 25. |
| „ Nov 19. | „ Dec. 25. |
| 1864, June 15. | 1864, May 13. |
| „ Aug. 21. | „ July 21. |
| „ Nov. 8. | „ Oct. 7. |

Treating the equations formed from these data by the method of least squares, we have, from the observed maxima,

Period=67·97 days.
Epoch=1864, Feb. 1·37.

And from the observed minima,—

Period=67·88 days.
Epoch=1864, January 1·59.

The mean of the two values of the period is 67·92 days. The interval, from minimum to maximum brightness, is 30·8 days, and from maximum to minimum 37·1 days. This variable, therefore, like many others, increases in brightness more rapidly than it diminishes. Its magnitude at maximum is 8·8 and at minimum 9·8, the range of variation being, therefore, one magnitude. It is one of the highly-coloured stars, both Mr. Hind and myself having always noted it as being *very red*.

Mr. BAXENDELL also communicated the following "Observations of the Greenwich Variable in Vulpecula and its Companion Stars. By GEORGE KNOTT, Esq., F.R.A.S., of Woodcroft Observatory, Cuckfield, Sussex."

1864, Nov. 5.—I have this evening examined the Greenwich Variable in Vulpecula and its neighbours. The state of the atmosphere was not by any means favourable, and I could not make out *c*; but *a*, *b*, and *d* were well in view, and *b* decidedly the greatest of the three. I inclined to estimate *b* about 12½ magnitude; and by the method of reduced apertures I found ·

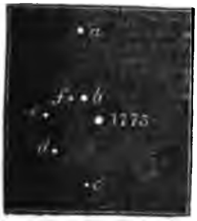
$a$=12·8 mag.    $d$=13·0 mag.
$b$=12·6 „    1773= 9·1 „

The star 1773 is the Greenwich variable; and *d* is the new star announced by Mr. Baxendell in his communication in the Society's Proceedings, No. 2, Session 1864-5.

Nov. 22.—I secured a hasty, and therefore comparatively valueless observation. After a hasty glance clouds came up, and I could not gauge the magnitudes as I was preparing to do. My rough results were—

$a$=13·0 mag.    $c$, invisible.
$b$=12·9 mag.    $d$=13·2 mag.

Nov. 26.—On this evening I entered in my journal—"*b*, 12·9; *a*, 13·0; *d*, 13·2. Is there a minute speck at *e*?"



Dec. 1.—This evening it is very clear, and I not only see most clearly a small star in the place indicated, but also that the star *b* has a small and pretty close *comes!* In the annexed diagram *e* and *f* are the new stars. I am not sure of their exact position, but have no doubt of their existence. The following are my rough estimations of magnitude:—*a*, 12·9; *b*, 12·8; *c*, 14±; *d*, 13·1; *e*, 13·7 (?); *f*, about = *e*.

The above observations were made with an equatorially mounted achromatic of 7⅓ inches aperture, constructed by Mr. Alvan Clarke, of Boston, U.S.

## Ordinary Meeting, December 27th, 1864.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

Mr. John Robinson and Mr. Joseph Spencer were elected Ordinary Members of the Society.

The bones of an adult gorilla from the Museum of the Natural History Society were exhibited, and some remarks on them were made by EDWARD LUND, Esq., F.R.C.S. *Ex.*, his object being to point out the characteristics of the gorilla skeleton by a direct comparison with the bones in man.

He said that when the bones of this gorilla were shown to him at the Museum, the first he took up happened to be the highest bone of the neck, the atlas, and he was much struck by its close resemblance to the same bone in man; it was however larger, while the hole in the transverse process for the passage of the vertebral artery was very much smaller, indicating a less supply of blood to the brain. With regard to the age of the specimen, he had no doubt that the skeleton was that of an adult, for all the bones were thoroughly ossified and the teeth were much worn down and filled by a secondary deposit of cementum. As to sex, he could not speak positively, for he did not know how far the differences in the pelvis observable in the human subject would hold good in the gorilla, but he pointed out the angular shape of the pelvic arch, which would lead to the supposition that the animal was a male.

He said he would now take the bones in detail, grouping them as the head, the trunk, and the limbs, and would indicate their peculiarities by showing how they differ from the same human bones whether by deficiency or excess of parts. The skull would attract attention at once by its very peculiar shape, which all seemed to agree in calling helmet-shaped; if however the prominent ridges which give it this appearance were left out of consideration, it would be found to be well formed and fitted to receive a well-developed brain. The front was indeed singular; for the orbits, instead of being excavated as it were out of the regular dome of the skull as in man, were outstanding, and that to such a degree that a perpendicular section which would remove them entire would scarcely lay open the cavity of the skull. It would be seen however that they formed perfect chambers, well walled in on all sides, showing that great care had been taken to protect the organs of sight. He pointed out the great depth and extent of the temporal fossæ, and remarked that they had something of the character observable in the Felinæ, the temporal muscles were very large and were so much expanded over the sides of the skull that they went completely up to the middle line. These large temporal muscles, which close the jaws with a powerful snapping action, had to do, he said, with the very strong and large canine teeth, which would be capable of exerting great force in tearing anything on which the animal might feed. The number of teeth was the same as in man, and they were of the same kinds, namely, in each jaw four incisors, two canines, four premolars, and six molars. The characters of the nasal fossæ indicated a moderate, not an acute sense of smell. In the foramen magnum he observed this difference, that while in man it is oval, in the gorilla it was almost circular or squarish. The base of the skull was convex as in man, corresponding with a concave internal surface upon which the brain would rest. The mastoid processes. which phrenologists considered to indicate destructiveness, but which are really a part of the organ of hearing, were not quite so well developed as in man. In the present case one of them happened to be broken so as to expose the interior, and, if we might form the same conclusions from it as we should in the human subject, its condition would strengthen the opinion that the animal was of mature age, for in early life these processes consist of solid bone, but as age advances are gradually excavated into larger and larger cells, and in the specimen exhibited these cells were very large. The lower jaw was remarkably large and strong, and the ramus formed a right angle with the body of the bone as it does in middle life in man. There was one peculiarity which he had observed in the condyle, namely, that the smooth articulating surface extended very far round the back of it, indicating that the mouth could be opened to a great extent, and this, together with the formidable teeth, would no doubt give the animal, to say the least of it, an alarming appearance if suddenly encountered in its native wilds.

The bones of the neck were, as usual in the mammalia, seven in number; they were very well joined together, the upper surface of their bodies being concave from side to side to receive the convex lower surface of each one above. They had well marked additional processes or rudimentary ribs, but the most remarkable feature was the great length and strength of the spinous processes, which would give advantageous attachment to very powerful muscles capable of exerting immense force in twisting the head from side to side as in the action of tearing anything with the teeth. The general appearance of the dorsal and lumbar vertebræ agreed with the human bones; the ribs were twenty-six in number instead of twenty-four, and as several of them remained in their natural position, connecting the sternum with the spine, it could be seen that the sternum, instead of having a nearly vertical position as in man, was very oblique, the lower part projecting forward and showing a great capacity of chest. The sacrum was unlike the human sacrum, being much longer and narrower. The haunch bones were also long in the direction of the length of the body, and were but slightly roughened for muscular attachment, a proof that the gluteal muscles were not much developed. The tuberosities of the ischia, however, were very strongly marked, indicating that the animal is well qualified for the sitting posture.

The upper extremities were very largely developed in proportion to the lower; in this specimen the length between the tips of the fingers of the outstretched arms is said to have measured eight feet. The shoulder blade had a peculiarity which anatomists formerly thought characteristic of man, namely, that the vertebral border was the longest. The arm bone looked immensely large when placed beside that of man; the roughnesses near the upper part, for the attachment of muscles going to the chest and back, indicated that these muscles were of extraordinary power, and when acting together would enable the creature to hug an enemy with terrible effect. The articulating surface at the lower end of this bone was oblique as in man, so that the bending of the elbow carried the hand not to the shoulder, as it would if the hinge-joint was directly transverse, but inwards to the mouth, furnishing a curious little illustration of the prehensile character of the upper extremity, and its essential use as an organ for obtaining food. The deep hollows at the front and back of the bone for the reception of projections of the ulna when the elbow is completely flexed or extended, were in the gorilla joined so as to become a hole, and thus gave even a greater extent of motion to this joint than in man. The bones of the forearm were very large, and a peculiarity in them worth noticing was that they were curved, giving them great power of resisting fracture by allowing a slight spring of the bones when subjected to a direct shock, and also adding immensely to the strength of the wrist in pronation and supination, by increasing the distance between the bones and therefore allowing of a greater length of muscle and better leverage. (The bones of the hands and feet were not shown, being at present with the skin.) The femur was shorter than the humerus, the reverse of what we find in man. The leg bones were also short, and the lower extremities had altogether the appearance of being less fully developed than the upper. The fibula was considerably shorter than the tibia, while in man the two bones are as nearly as possible the same length. The lower end of these bones formed a very imperfect socket for the ankle joint compared with the same parts in man, where it is most perfectly adapted for sustaining the erect position.

As to the habitual attitude of the gorilla deduced from an examination of the bones, the characters of the ankle joint just alluded to show that though the creature might stand erect it could have but little stability in that position, and progression on the hind legs alone would be difficult and could not be long sustained. Important evidence on the same point was also furnished by the character of the lower part of the vertebral column and the position of the pelvic bones with regard to it, for, as might be seen, there was no deep hollow behind, as in man, for the attachment of the erector spinæ muscles, the sacrum in this case being even placed further back than the iliac bones; the form of the pelvis too, and its direction with regard to that of the backbone, showed that it was fitted for the support of the contained viscera in the horizontal rather than in the erect position; and all these points led to the same conclusion, namely, that the gorilla habitually uses the front as well as the hind legs in progression.

## MICROSCOPICAL SECTION.

December 19th, 1864.

J. Sidebotham, Esq., President of the Section, in the Chair.

### *Exhibitions.*

The skeleton of an adult gorilla, by Thomas Alcock, M.D. The more striking peculiarities of the gorilla skeleton were explained by comparison with the human bones, by Edward Lund, Esq., F.R.C.S. *Ex.*

Specimens of comatula rosacea, from the Cove of Cork.—Thomas Alcock.

A jay, with the beaks crossed and very much overgrown.—A. G. Latham, Esq.

Mounted foraminifera, from Dogs Bay, coast of Galway.—Thomas Alcock.

### *Communications.*

Mr. Latham gave the result of his examination of a shell of Helix nemoralis brought by Mr. Glover, from the shore of Gorteen Bay, Connemara. The weight of the shell was 56 grains, while that of an ordinary specimen was found to be only 16 grains. By drying it lost a grain of weight, and by calcination, two grains. It was then powdered and washed with a loss of one grain of soluble salts, the whole loss of weight being 4 grains. The residue was perfectly soluble in muriatic acid. Mr. Parry showed a section of a shell of Helix aspersa, brought by Mr. Glover from the same locality; before cutting, it weighed 126 grains, and the section exhibited very clearly its unusual thickness and solidity.

Mr. W. H. HEYS read a communication on the structure of cotton. He said the twisted appearance so often alluded to is caused simply by the collapse of the cell-walls in drying, and is not an actual twisting. Captain Mitchell's experiment with the ammoniacal oxide of copper was not conducted in the manner necessary for showing the spiral threads which have been observed within the outer cell-walls, for in his letter read at the last meeting, he speaks of having put cotton into the solution four months ago, examining it at intervals, frequently at first, and then daily for some time, whereas the action of this solvent is so rapid that the microscope requires to be prepared beforehand, as in observations on crystallization, and it is sometimes difficult even then to follow the changes which take place. Mr. Heys exhibited the action of the solvent under the microscope, and also showed many drawings of cotton which had been similarly treated. He said that if a single fibre of the cotton be selected and watched it will be seen first to become inflated, not uniformly, but with many constrictions, giving it the appearance of an irregular string of beads. After a time the external envelope will entirely disappear, leaving only fragments of the spiral thread, of which the portions corresponding with the constrictions will be seen to have the form of rings. Having exhibited these appearances, he said he would leave the members to form their own opinion upon the structure of cotton, but repeated the conclusion he had arrived at, that it is composed of an external envelope or tube, within this a spiral thread, preventing its collapse, and a pith-like substance in the centre.

Mr. NEVILL showed mounted specimens, and handed in a list of 43 forms of Foraminifera, found in sand from the shore of Gorteen Bay, Connemara. Some of the kinds, he said, were not figured in Professor Williamson's Recent Foraminifera, and among these he particularly mentioned a form of Miliolina, with an appendage like a tail at the end opposite the septal orifice. He specially called attention to his mode of mounting the specimens in many small cells upon a single glass slip, by which much room is saved, and other advantages are gained; in those shown, ten small cells were punched out of an oblong piece of card, in two rows of five each, and the whole covered with a single glass He remarked on the extraordinary assemblage of species found in this Connemara sand, which, Mr. Dancer suggested, might be due to the influence of the Gulf Stream.

## PHOTOGRAPHICAL SECTION.

### Ordinary Meeting, Nov. 3, 1864.

Professor W. C. Williamson, F.R.S., &c. &c., Vice-President of the Section, in the Chair.

The following gentlemen were elected associates, viz.—Messrs. William Pegg, T. D. Thorpe, George Wardley, E. G. Hughes, William Lockett, Thos. Heywood, G. W. Mosley, F. C. Tobler, A. Lees, and W. G. Coote.

In the absence of the President, the Lord Bishop of Manchester, the Chairman delivered an inaugural address, in the course of which he referred to the three aspects in which photography naturally presents itself to our notice—as an amusement, as a science, and as an art.

Mr. A. **Brothers** exhibited some portraits which he had taken by means of the magnesium light. He had discarded the use of all reflectors; the experiments made were generally with three strands of wire, two flat ones bound together with a round one. Mr. Brothers also submitted a panoramic picture taken in the new revolving camera, and which was perfectly sharp and distinct in every part.

Mr. **Wardley** presented to the Album twelve very beautiful photographs of scenery in Cumberland, Westmorland, &c., taken by the Taupenot process.

Some photographs printed by the Wothlytype process were exhibited and much admired.

Mr. **Joseph Sidebotham** then read his Paper "On Printing Transparencies for the Stereoscope and Magic Lantern," in the course of which he called attention to the fact that we were indebted to Mr. Dancer for inventing the binocular camera, without which the stereoscope and its attendant pictures would not have been, as at present, found in almost every home. Mr. Dancer's idea, that the pictures should be taken only at a distance of three inches apart, was at first ridiculed; now, however, that distance is almost universally adopted.

Early in the history of the stereoscope the French transparent views, on albumen, were eagerly purchased, although the price was very high, and was still so.

Mr. Sidebotham hoped to prove that glass transparencies might be produced at a cost little over that of paper prints. He then described the various processes at present in use for producing them, pointing out the several disadvantages under which each process laboured. Copying in the camera has been used, and is still used by many with various combinations of lenses—some with one, some with a pair of lenses; but, as far as Mr. Sidebotham has noticed, none of the cameras for the purpose are satisfactory or suited for the wants of amateurs.

Mr. Sidebotham then described the little camera which he had made for the purpose, and which seemed to answer in every way. The principal is to have the negative in front, in an adapting frame to slide right and left; the distance is adjustable by two screws; the frame which holds the plate also slides right and left, and stops in two places with a drop catch, the lens (a short focussed double steresocopic one) being in the middle. The camera is adjustable with screws for focussing or for enlarging or reducing the image. The camera is then tilted towards the sky, the image is focussed, and the picture taken on wet collodion, moving the negative plate for each half of the picture, developing with iron, and, if requisite, deepening or toning with proto-iodide of mercury. One focussing and adjustment of the screws will be sufficient, and any number of negatives can be copied, if taken with the same camera, without any alteration. The negatives require no varnishing, so that all their original beauty is there to be copied, and it is copied faithfully. Prints by this plan may be produced as fast as the plates can be prepared—much faster than on paper if a single negative be used, as the exposure varies trom five to thirty seconds for each side.

Mr. Sidebotham stated that much of the beauty of the pictures he exhibited were doubtless due to the quality of the negatives, and which had been taken by Mr. Buxton, when in India last year. Mr. Sidebotham trusted that the plan here described would induce amateurs to print their stereoscopic pictures on glass instead of paper, and professionals to lower the price of those they place in the market for sale.

## PHOTOGRAPHICAL SECTION.

Ordinary Meeting, December 1st, 1864.

JOSEPH BAXENDELL, F.R.A.S., in the Chair.

Mr. Samuel Cottam was elected an Ordinary Member.

Mr. MUDD presented four large and very beautiful photographs to the society's portfolio,—two taken in Dunham Park, and two in North Wales, by the colodio-albumen process. These landscapes were remarkable for their peculiar softness and delicacy.

Mr. PARRY called attention to a letter in "The British Journal of Photography," by Mr. Hislop, claiming the invention of the camera exhibited and explained by Mr. Sidebotham at the last meeting. He also produced the Journal in which was the drawing and description on which Mr. Hislop founded his claim. After an examination of these it was unanimously decided by the members present that there was no similarity whatever between the two cameras, and that a drawing ought to be published of Mr. Sidebotham's camera.

Mr. NEVILL exhibited some good prints produced by making paper sensitive with a solution of salts of nitrate of uranium and nitrate of silver, and when the image began to appear, developing with sulphate of iron; the prints were only exposed fifteen seconds.

Mr. SIDEBOTHAM exhibited several specimens taken by himself, by the Wothlytype process, on different kinds of paper. He had experienced considerable difficulty in finding paper to which the collodion would adhere in the washing process. Silver prints from the same negatives were exhibited, and considered superior to those by the Wothlytype process.

Mr. DANCER exhibited on the screen, by the aid of the oxy-hydrogen light, a series of photographic pictures by members and others. The series consisted of transparencies, taken by various processes, and was a trial of their relative merits for exhibition in this manner.

Many beautiful pictures were exhibited printed on albumen, collodio-albumen, Fothergill, tannin, oxymel, and syrup processes, and also taken on collodion in the camera. The two latter processes were decided to be the most suitable; the photographs taken in India by Mr. Buxton were much admired. Several photographs of the moon were exhibited by Mr. Brothers, one of which was taken by himself.

## Ordinary Meeting, January 10th, 1865.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

Dr. ROSCOE exhibited some very interesting photographs of the fixed lines in the solar spectrum made by Mr. Rutherford of New York. These photographs exhibit groups of thousands of lines extending from near the line *b* in the green to beyond H in the violet, and serve as a most valuable confirmation of the accuracy of Kirchhoff's maps. Each line in these maps can be easily and distinctly traced in the photograph, whilst many bands drawn as single ones by Kirchhoff are seen in the magnified photograph to consist of bundles of fine lines. These photographs were prepared with three 60° bisulphide of carbon prisms.

Dr. ROSCOE also exhibited two fine photographic prints of the moon, enlarged by Mr. Rutherford from negatives taken by him in New York with an 11¼in. object glass of 14ft. focal length, which he had ground with special reference to the highly refrangible rays, and which is therefore unfit for ordinary telescopic purposes.

Mr. BAXENDELL, and Mr. WILKINSON, F.R.A.S., expressed their opinion that Mr. Rutherford's prints were decidedly sharper than any photographs of the moon they had seen.

A Paper was read "On Some Products Derived from Indigo Blue," by Dr. E. Schunck, F.R.S., Vice-President.

By his experiments on the formation of indigo blue, an account of which was laid before the Society several years ago,* the author was led to make some inquiries regarding the processes employed in tropical countries for the production of indigo from plants. All authorities, it appears, agree that the process of fermentation, which is the one usually adopted for the purpose of extracting the colour, requires to be conducted with the greatest care in order to lead to a successful result. Unless certain precautions are adopted the colouring matter may be entirely lost. This phenomenon may be easily accounted for. Though indigo blue, when once formed is a very stable body, the substance existing in the cells of the plant from which it originates and which the author terms *Indican*, is decomposed with the greatest facility, indigo blue being only one of its products of decomposition, which may be formed or not, according to the nature of the process employed.

There are, however, other facts connected with this subject which cannot so easily be explained. It is well known to those dyers who employ the so-called *woad vat*, in which the reduction of the indigo blue is effected by means of various organic matters, such as woad, madder, and bran, together with lime, that if the process be not carefully managed it may change its character entirely—a change which results in the total destruction or disappearance of the colouring matter. This phenomenon cannot be explained in accordance with what is at present known regarding indigo blue, which is considered by chemists to be a body of such a stable character as not to be decomposed by any except very potent agents, such as chlorine, bromine, or nitric acid. It has not hitherto been supposed possible to effect its decomposition by means of fermentation or putrefaction.

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* See Memoirs, Vol. XIV., p. 181.

Then, again, the author found that when very small quantities of indigo blue are reduced according to Fritzsche's method, which consists in acting on it with alcohol, grape sugar, and caustic soda, the colouring matter does not make its appearance again when the solution is exposed to the atmosphere. The liquid yields no deposit and remains yellow and transparent. This fact is also difficult to account for, since it is usually supposed that by the combined action of reducing agents and alkalies indigo blue merely takes up an atom of hydrogen and then dissolves, and by the action of oxygen is again precipitated unchanged and undiminished in quantity. By the continued action of a large excess of alcohol and grape sugar, together with caustic soda, the author succeeded in causing several grammes of indigo blue to disappear entirely. That the effect was due to the combined action of alcohol and grape sugar, not to that of one or the other only, was proved by subjecting a small quantity of indigo blue to the action of grape sugar and caustic alkali in watery solution, and another portion to the action of alcohol, protoxide of tin, and alkali. Reduction of course took place in both cases; but, though the solutions were boiled for some time, the colouring matter was in each case precipitated again on exposure to the air, apparently undiminished in quantity. Since, by the action of caustic alkalies on grape sugar, acetic and formic acids are formed, it occurred to the author that the effect produced by the sugar in this process might in reality be due to the presence of one or both of these acids rather than to the sugar itself. This supposition was completely verified by experiment. The colouring matter disappeared quite as rapidly when acetate or formiate of soda was employed in the place of grape sugar. The use of the latter was therefore abandoned in the subsequent experiments. In the present communication the author confines himself to an account of the combined action of alcohol, acetate of soda, and caustic soda on indigo blue.

The process adopted was quite simple. Pure indigo blue was introduced into a large quantity of ordinary spirits of wine, and, after being well agitated, the mixture was raised to the boiling point. A quantity of pure acetate of soda, previously deprived of its water of crystallization, and a little caustic soda were then added, and the boiling was continued for several hours. A reduction of the indigo blue took place in the first instance, as was evident from the deep red colour of the liquid. On agitating with air this red colour disappeared for a moment, the indigo blue being precipitated in powder, but after some time the liquid acquired a dark brown colour and deposited nothing on exposure or agitation. The process was then completed. In order to obtain the products formed, the brown liquid was evaporated, and, when the evaporation was nearly completed, water and an excess of sulphuric acid were added, which threw down a brown insoluble mass, consisting partly of resinous, partly of pulverulent substances. From the liquid, which was of a light brown colour, a crystallized acid was obtained, which after being purified was found to consist of anthranilic acid. From the mass, insoluble in water, the author obtained five distinct substances, which were separated from one another by means of various solvents, such as alcohol, ether, ammonia, and carbonate of ammonia. These substances were all brown and amorphous. Some of them resembled resins, others were powders. In general they were found to possess very few characteristic properties, and as they presented very little that could be of interest to the chemist, if their origin and their mode of formation be excepted, the author refrained from bestowing names on them and thus adding to the already unwieldy mass of terms with which chemical science has to deal, but preferred to distinguish them by the letters of the alphabet, as A, B, C, D, and E. The body A is easily soluble in cold alcohol and ether, but quite insoluble in alkalies. B is easily soluble in alcohol and ether, as well as in alkalies, both caustic and carbonated. These two have the appearance of resins of a rich brownish-yellow colour. C is very little soluble in alcohol and ether, and insoluble in alkalies. D closely resembles C, but is distinguishd by its solubility in alkalies. E is remarkable for being soluble in a boiling solution of acetate of soda. These three are brown powders. That portion of the mass soluble in alcohol and alkalies, but insoluble in ether, was not examined, as it was sure to contain some of the peculiar resinous product of decomposition, which is always formed by the action of caustic alkalies on alcohol, and which is supposed to be identical with the so-called "aldehyde resin."

The author's analyses of these five bodies led to the following formulæ:—

A.........................$C_{62}H_{39}NO_8$
B.........................$C_{52}H_{35}NO_8$
C.........................$C_{28}H_{11}NO_4$
D.........................$C_{56}H_{24}N_2O_{10}$
E.........................$C_{28}H_{11}NO_6$

The author attaches no importance to these formulæ except in so far as they furnish a means of explaining the mode in which these bodies are formed. It will be seen that they all contain the elements of indigo blue, alcohol and acetic acid in various proportions. Taking as an instance the body C, which is the simplest in constitution, it is apparent that it has been formed by the union of 1 atom of indigo blue, 1 atom of alcohol, and 2 atoms of acetic acid, 8 atoms of water being at the same time eliminated, since

$$C_{28}H_{11}NO_4 + 8HO = C_{16}H_5NO_2 + C_4H_6O_2 + 2C_4H_4O_4.$$

In like manner E originates from the combination of 2 atoms of indigo blue, 1 atom of alcohol, and 5 atoms of alcohol, for

$$2(C_{28}H_{11}NO_6) + 14HO = 2(C_{16}H_5NO_2) + C_4H_6O_2 + 5C_4H_4O_4.$$

The formation of B will be easily understood by a glance at the following equation:

$$C_{52}H_{35}NO_8 + 18HO = C_{16}H_5NO_2 + 6(C_4H_6O_2) + 3(C_4H_4O_4).$$

In the case of A, which is the most complex of all, it is necessary to assume that carbonic acid comes into play, since

$$C_{62}H_{39}NO_8 + 26HO = C_{16}H_5NO_2 + 8(C_4H_6O_2) + 3(C_4H_4O_4) + 2CO_2.$$

It is difficult to say whence this carbonic acid is derived, but the author supposes it may originate in the decomposition of that portion of the indigo blue which yields anthranilic acid.

Hence it appears that all these products, except anthranilic acid, are formed by a very simple process which consists merely in indigo blue taking up alcohol and acetic acid in various proportions and forming compound bodies in which none of the constituents, as such, can be detected. It is, therefore, not a process of decomposition, but a synthetical process, a building up of complex bodies from others of a simpler constitution. This is proved by the fact of water being given up during the process, whereas in all cases in which complex organic substances are decomposed into simpler ones, water is absorbed. Regarding the real constitution of these bodies, the author hazards no speculations. It might be supposed that they belonged to the class of conjugated compounds, of which organic chemistry furnishes us with so many examples, and that by decomposition we should be able to obtain from them some of the simpler bodies which are known to have entered into their composition, but the author's experiments, as far as they have gone, do not countenance this view. He was unable to obtain from any one of them, either indigo blue, alcohol, or acetic acid.

The occasional disappearance of the indigo blue in the woad vat in consequence of mismanagement now admits, the author thinks, of any easy explanation. By the fermentation of the sugar contained in the madder employed, alcohol is formed, which in its turn may yield some acetic acid and alcohol, acetic acid and a base (lime) being present, nothing further is required for the development of the process described by the author.

Professor ROSCOE suggested that some of the bodies described by the author might possibly be represented as substitution products, one or more of the atoms of hydrogen of the indigo blue being replaced by one or more organic radicles.

A Paper was also read "On Some Physiological Effects of Carbonic Acid and Ventilation," Part I., by Dr. R. ANGUS SMITH, F.R.S., President.

## Ordinary Meeting, January 24th, 1865.

R. Angus Smith, Ph.D., F.R.S., &c., President, in the Chair.

Mr. William B. Johnson was elected an Ordinary Member of the Society.

Mr. S. B. Worthington, C.E., stated that it might be of some interest to the engineering members of the society to know, that he had lately constructed a swing bridge for carrying a railway over the Sankey canal, in which the girders are made of Bessemer steel plates. The object of using steel instead of wrought iron, was to reduce the weight of the girders. The girders are four in number, about fifty-six feet long, with bearings varying from thirty to forty feet, and two feet deep. They were manufactured by Messrs. Benjamin Hick and Sons, of Bolton, from steel plates made by the Bolton Steel and Iron Company; and were tested with loads of a ton to the foot, or more than double the weight which they could possibly be called upon to bear. The deflection varied from $\frac{1}{2}$-inch to an inch, according to the length of the girder, and there was no permanent set on removal of the testing load.

The plates used varied from $\frac{1}{4}$-inch to $\frac{7}{16}$-inch in thickness; and the average tensile strength of a considerable number of plates tested, was upwards of thirty-six tons to a square inch.

The weight of the girders was about $\frac{6}{8}$ths of the weight which they would have been if wrought iron had been used.

The contract for this bridge was made in November, 1863, and the bridge was erected during the past Summer.

Mr. Worthington also exhibited a piece of cast iron lately taken out of the Sankey canal. Its exterior, from one-eighth to a quarter of an inch in depth, was so soft as to be easily cut with a dull knife. From the form of the casting he thought it very probable that it had not been in the canal more than five or six years. He stated that the water of the canal was strongly impregnated with liquids discharged from the alkali works in the neighbourhood of St. Helens.

Mr. Sidebotham read the following communication from Mr. James Nasmyth, C.E., Corresponding Member of the Society, and exhibited the large and beautiful drawing to which it refers:—

On the 5th of June, 1864, between the hours of 11 a.m. and 2 p.m., I had the good fortune to observe under unusually favourable circumstances, a large and remarkable group of solar spots.

The willow-leaf shaped objects forming the structural element of the entire photosphere, as also forming the details of the penumbral portions and bright margins of the photosphere overhanging the spots, were occasionally revealed with perfect distinctness; but the favorable moments for pure definition had to be watched for, by keeping the eye steady at the eye piece and the hand on the focal adjustment, so as to be ready to make those minute focal adjustments that I find are requisite to meet the constantly varying condition of the atmosphere, which, especially during bright sunshine, is so fertile a source of defective definition, as is well known to practical observers.

Some of the willow-leaf shaped objects were very favourably situated in an insulated position over the dark centres of the spots, and in that situation yielded me excellent opportunity for carefully noting their exact form and proportions. In this respect I can with the utmost confidence pledge myself to the correctness of what I have represented in the drawing which accompanies this communication.

I continue to employ the term "willow-leaf" shaped objects, as sufficiently exact to convey to any one a general idea of their form, and so to identify them. I might perhaps have hit upon some other term that would have been more exact and descriptive, and in that respect I was much pleased with that which was employed by Mr. Stone, of the Royal Observatory, Greenwich, when he first beheld these remarkable objects by the aid of the great equatorial refracting telescope of the Royal Observatory, on which occasion Mr. Stone described them as "bright rice-like particles." Perhaps the more general term "lenticular shaped objects" would be better than either, as admitting of a certain latitude in respect to proportions, that may admit of a classification of any variety in these respects that future observations may supply.

Any observer, who with due means at his command is fortunate to obtain a satisfactory view of these truly-remarkable structural details of the solar photosphere, can, of course, please himself as to the term that appears to him best to convey a correct idea of them. But the grand fact of their existence is now proved beyond all doubt; and they, as "a great fact," will ever remain so long as the sun exists.

Dr. R. Angus Smith, F.R.S., read a paper "On some Physiological Effects of Carbonic Acid and Ventilation," of which the following is the substance.

That bad ventilation produces effects which are unpleasant, unwholesome, dangerous, or deadly, according to circumstances, has been long known; it has also been well known that the effects of breathing carbonic acid are of a similar kind. We have not, however, been able to say distinctly that the evil effects of bad ventilation are due entirely to carbonic acid. We have had, and not without good reason, a strong belief that the organic matter was a grave offender.

The author had endeavoured to show elsewhere that the organic matter is in early stages of ventilation the most observable; but that in later stages the effect of the carbonic acid is unmistakeable.

When enquiring into the state of the air in mines he found it needful to make experiments in close places, and had a leaden airtight room built, containing about 170 cubic feet of air. On examining the effect of carbonic acid on the burning of candles, he remained in the chamber until that gas was poured in to the extent of 3·9 per cent. He then found that the pulse fell so low that it was difficult to count the beats, whilst they diminished in number. This effect was rapid, as he was not long in the room. Since there was no time for the accumulation of organic matter, nearly the whole effect must have been due to carbonic acid. Similar results were observed frequently: a few may be given. The first here adduced will show the fall of the pulse and the increase of the respiration more clearly than the others as the details are appended. The carbonic acid increased by means of respiration only. The number of beats diminishes with a regularity equal to the increase of the carbonic acid, whilst the breathing quickens with equal steadiness.

### ONE PERSON IN THE LEAD CHAMBER.

*Respiration and beats of the pulse taken every ten minutes.*

| Time. | Pulse. | Respiration. | Temperature, Celsius. | Carbonic acid in the same periods. |
|---|---|---|---|---|
| h. m.<br>10 55 | 73 | 15·5 | 18°·2 | 0·04 |
| min.<br>After 10 | 73 | 16 | 18·2 | 0·114 |
| „ 20 | 72 | 16 | 18·2 | 0·187 |
| „ 30 | 71 | 17 | 18·4 | 0·261 |
| „ 40 | 71 | 16 | 18·4 | 0·335 |
| „ 50 | 70 | 16 | 18·5 | 0·408 |
| „ 60 | 68 | 16 | 18·6 | 0·482 |
| „ 70 | 67 | 16·5 | 18·7 | 0·556 |
| „ 80 | 67 | 17 | 18·8 | 0·629 |
| „ 90 | 66 | 17 | 18·9 | 0·703 |
| „ 100 | 65 | 18· | 19·0 | 0·777 |
| „ 110 | 65 | 18·5 | 19·0 | 0·850 |
| „ 120 | 64 | 19 | 19·0 | 0·924 |
| „ 130 | 63 | 19 | 19·2 | 0·997 |
| „ 140 | 62 | 19·5 | 19·1 | 1·071 |
| „ 150 | 62 | 20 | 19·1 | 1·145 |
| „ 160 | 62 | 20 | 19·1 | 1·218 |
| „ 170 | 61 | 20 | 19·1 | 1·292 |
| „ 180 | 60 | 21 | 19·1 | 1·366 |
| „ 190 | 60 | 22 | 19·2 | 1·439 |
| „ 200 | 59 | 23 | 19·2 | 1·513 |
| „ 210 | 58 | 24 | 19·4 | 1·587 |
| „ 220 | 57 | 24 | 19·4 | 1·661 |
| „ 230 | 57 | 24 | 19·4 | 1·734 |

Five persons sat for 80 minutes in the room. In all cases there was great irregularity of breathing. They were all conversing with each other, which occupation, as was found, somewhat modified the effect. In two cases the rise of the pulse was considerable, viz., from 60 to 79, and from 84 to 91; but the numbers soon fell down to the natural amount, and would apparently have fallen much lower if one of the two persons had not felt too unwell to remain. The pulses in all cases were difficult to count, being excessively feeble, and the most delicate of the fingers was sought for the operation. No deficiency of strength was found in any of the trials, and the amount of carbonic acid rose equally from first to last.

As the pulse fell and rose according to the individuality, the author fixed on one yôung man on whom the effect was most regular for further experiment. It must be remembered, however, that on no person was the effect small or uncertain.

The objects of the farther experiments were these, 1st, to inquire if the influence could be observed when the amount of carbonic acid was small; and 2nd, to separate the effects of the carbonic acid entirely from those of organic matter.

With 3 per cent of carbonic acid evolved in the chamber itself, the pulse fell in 27 minutes from 67 to 62, the breathing rose from 17 to 23; the pulse so low that it was barely perceptible. The exposure was not full 27 minutes, as the gas took some time to evolve.

With 2 per cent the pulse fell in 70 minutes 4, the breathing rose from 18 to 23½. On coming out the pulse rose 8 in five minutes.

With 1 per cent the pulse fell 4 in the hour.

The following results were obtained by breathing air with carbonic acid entirely free from organic matter, the inspirations being taken from a prepared reservoir, and the expirations not being allowed to mix with them:—

With 1 per cent $CO_2$ a rise of 2, then a fall of 5 beats of the pulse in 26 minutes.

„ 0·5 pulse fell 5 in 40 minutes, respiration rose 7.

„ 0·25 carbonic acid pulse rose 3, and fell 4 in 30 minutes, respiration rose 4.

„ 0·1 carbonic acid rose 1 and fell 1, in 45 minutes, breathing rose 1.

Ordinary air was breathed in the same way, so as to eliminate the effect of the apparatus. Pulse rose 1 and fell 1, but no greater change occurred during a whole hour. Breathing continued unchanged except at one interval, when it fell 1 and then resumed its usual number. In no experiment during the whole period did the breathing of the same experimenter ever fall 1 when there was as much as one tenth per cent of carbonic acid present.

In one person, a youth, the pulse rose on every trial. On entering into air with 3 per cent of carbonic acid and no organic matter, his pulse rose 6 in two minutes and his breathing fell 4. The pulse was so feeble that he could not count it; some one helped in the process. He found the air very unpleasant. Another young man in two minutes in the same air, found that his pulse rose 6 and his respirations 4.

The action of carbonic acid seems, therefore, in all cases to enfeeble the pulse; at first sometimes to cause a rise, but finally to lower the number of the beats.

This effect is instantaneous or nearly so with air having 3 per cent of carbonic acid, but diminishes with the amount of impurity. It is, however, perceptible with an amount of carbonic acid as low as 0·1 per cent, and probably by taking long periods the effect would be found even with smaller quantities. This amount is often exceeded in private houses and public meetings, where it rises to 0·2 or even 0·3.

The second effect of carbonic acid is in the breathing, which it hastens rapidly, although in some cases it causes a diminution of the inspirations. The effect approaches either a gasping or a panting.

The author added that he must leave to physiologists to speak of the ultimate effect of such a condition of things, and would only observe, that in Dr. Peacock's inquiries into the state of health of the Cornish miners, he found that a feeble pulse was one of the peculiarities, a proof that the temporary results found in these experiments may be rendered permanent.

On coming into fresh air the pulse and breathing recovered in a few minutes, showing the value of ventilation.

## PHYSICAL AND MATHEMATICAL SECTION.

### January 12th, 1865.

JOSEPH BAXENDELL, F.R.A.S., President of the Section, in the Chair.

Mr. HEELIS, F.R.A.S., communicated the following account of a fire ball which he had observed:—

Tuesday, 13th December, 1864, 0 hour 7′ a.m. Fire ball with sensible disc, and brilliant train from middle of seat of Cassiopeia's chair, to a point half-way between *a* Andromedæ and *a* Cygni. Disappeared at an altitude a little greater than that of the latter star (in other words about 10° above horizon); duration about 2″. Train extended from point of appearance to that of disappearance, but disappeared almost immediately after the meteor itself. Color, bluish white, with sparks from head, especially when near the point of disappearance. Did not see it burst. Light intense, and well defined disc. It was nearly calm on the earth at the time, but heavy scud was going rapidly over the moon from the S.E.; and to the best of my judgment the meteor fell directly before the scud. Laying off the course, &c., on a celestial globe, the results are—altitude of point of first observation or origin about 50°; altitude of disappearance 10°; length of track 40°; direction N.W.

There was so much scud that the meteor must have passed behind several clouds, and so much moonlight that the time by watch could be read. Hence the disc could hardly have been the effect of irradiation.

9 a.m. Wind S.E.; light, clear weather.

The only point upon which I have any doubt is the time, which might have been a little later. I took the time by my watch, but it had been wrong for a day or two, and I could not get to any good clock to test it for some days after.

Mr. Vernon, F.R.A.S., communicated the following "Note on the Rainfall of 1864."

The fall of rain for the past year has been 4·758 inches below the average of the last 71 years. The fall was also 7·700 inches below that of 1863, and fell upon 44 less days than in that year.

The principal deficiency occurred during the last six months of the year, especially in July and October. The only month having a fall in excess out of the last six months of the year was September, and rain fell upon more days that month than any month during the year.

The falling off in July and October was no doubt owing to the excessive amount of easterly winds. In July easterly winds occurred upon 11 days, whereas the average of 13 years gives only 5·9. In October easterly winds prevailed on no less than 18 days, the average being only 9·5.

On examining the monthly means for the 71 years, there appears to be evidence of periodicity in the distribution of the rainfall: a minimum occurring in April just after the vernal equinox, and a maximum in October just after the autumnal equinox. In September, however, there appears to be an exception to the gradually incrcasing values from April to October.

If this law really exists as it appears to do, the rainfall will evidently bear a close relation to the sun's course in the ecliptic, the minimum rainfall occurring at a time when the gradually increasing temperature of the air enables it to absorb a larger amount of moisture, and consequently tending to prevent precipitation. The maximum rainfall also occurring at the time, the temperature begins to fall rapidly, and consequently tending to precipitate the large amount of moisture absorbed during the heat of summer.

## RAINFALL, 1864.

## OLD TRAFFORD, MANCHESTER.

By G. V. VERNON, F.R.A.S., M.B.M.S.

**Rain gauge 3 feet above the level of the ground, and 106 feet above the sea.**

| Quarterly Periods. 1863. Days. | Quarterly Periods. 1864. Days. | 1864. | Fall in Inches. | Average of 71 Years. Inches. | Difference. | Number of days Rain fell in 1864. | Quarterly Periods. 1864. Inches. | Quarterly Periods. 1863. Inches. |
|---|---|---|---|---|---|---|---|---|
| 45 | 38 | January..... | 1·684 | 2·460 | —0·776 | 12 | 7·722 | 6·170 |
| | | February ... | 4·027 | 2·379 | +1·648 | 13 | | |
| | | March....... | 2·011 | 2·310 | —0·299 | 13 | | |
| 53 | 39 | April ...... | 1·602 | 2·038 | —0·436 | 9 | 7·722 | 7·746 |
| | | May......... | 3·175 | 2·351 | +0·824 | 10 | | |
| | | June........ | 2·945 | 2·913 | +0·032 | 20 | | |
| 56 | 45 | July......... | 1·687 | 3·569 | —1·882 | 9 | 8·063 | 12·216 |
| | | August...... | 2·367 | 3·592 | —1·225 | 15 | | |
| | | September.. | 4·009 | 3·235 | +0·774 | 21 | | |
| 61 | 49 | October..... | 1·908 | 3·818 | —1·910 | 13 | 7·133 | 12·208 |
| | | November .. | 3·255 | 3·473 | —0·218 | 19 | | |
| | | December... | 1·970 | 3·260 | —1·290 | 17 | | |
| 215 | 171 | | 30·640 | 35·398 | —4·758 | 171 | 30·640 | 38·340 |

## Ordinary Meeting, February 7th, 1865.

R. ANGUS SMITH, Ph.D., F.R.S., &c., President, in the Chair.

Among the donations announced was a framed photographic portrait of the late Thomas Hopkins, Esq., formerly a Vice-President of the Society, presented by James Mooney, Esq.

On the motion of Dr. JOULE, seconded by Mr. ROBERT WORTHINGTON, the thanks of the Society were unanimously voted to Mr. Mooney for his valuable donation.

Mr. BINNEY, F.R.S.: After the researches of Lindley, Geoppert, Brongniart, Prestwich, Hooker, and others, it really seemed that we had obtained almost a complete knowledge of the internal structure of *Stigmaria*. It is true that only Geoppert had seen the isolated bundles in the pith, all the specimens of the other observers having been imperfect in that portion of the plant, and giving no indication of structure there. In my own researches I have rarely met with a *Stigmaria* shewing any structure in the central axis, even where the small stems of *Sigillaria vascularis* displaying all the structure in that part are found in great abundance.

Many years since, after the examination of a great number of specimens of *Stigmaria* in my collection, it occurred to me that an outer as well as an inner radiating cylinder would be discovered. In my remarks on *Sigillaria*, published in

Vol. IV., part 1, p. 20, of the *Quarterly Journal of Geology*, is the following passage: "That part of *Stigmaria* which intervened between the vascular axis and the bark, appears to have consisted of two kinds of tissue. These have, in most cases, been unfortunately destroyed, so that we cannot positively know their true nature; but they appear to have been of different characters, for there generally appears to be a well marked division. This is often shown in specimens composed of clay ironstone which have not been flattened, and the boundary line is about quarter an inch from the outside of the specimen. Most probably the outer zone has been composed of stronger tissue than the inner one, as is the case with well preserved specimens of *Lepidodendron*." It is singular that such acute observers as those above named had not noticed this line of division, but it was no doubt owing to the imperfect specimens which they had examined. After the discovery of the outer radiating cylinder in *Lepidodendron* by Witham, and the same arrangement in *Sigillari elegans* by Brongniart, it was to be expected that such outer radiating cylinder would be found to exist in *Stigmaria*, if it were the root of *Sigillaria*. After an examination of a great number of specimens, the cabinet of Mr. James Russell, of Chapel Hall, Airdrie, has afforded me four or five different *Stigmaria*, which give clear evidence of the existence of this outer radiating cylinder. They are all in clay ironstone, and have not been much compressed. He has kindly allowed me to slice two of the specimens, which afford decisive evidence of the existence of both an inner and an outer radiating cylinder. The space on the outside of the inner cylinder does not shew distinctly the bundles of vessels communicating with the rootlets, although there is some evidence of their former occurrence. The bell shaped orifices from which the rootlets sprang are well displayed, and the space between them is occupied by wedge shaped masses of elongated tubes or utricles arranged in radiating series, not to be distinguished in any way from those which I have shewn occur in *Sigillaria vascularis* (*Quarterly Journal of the Geological Society* for May, 1862, Plate 1, fig. 6). Indeed, the transverse section of the specimen there figured would almost do for a representation of *Stigmaria,* if the latter had the central axis preserved, which it unfortunately has not. There is the same internal radiating cylinder and the same space formerly occupied by lax cellular tissue, which gradually passes into the elongated tubes or utricles arranged in radiating series, and parted by large bundles of vessels next to the bark; thus clearly proving from structure alone that *Stigmaria* is the root of *Sigillaria,* each of them having an inner radiating cylinder composed of barred vessels, a space occupied by lax cellular tissue, and an outer radiating cylinder composed of elongated tubes or utricles.

Mr. Binney also exhibited some of the specimens to the meeting, and said that he was in possession of clear evidence both from structure and branching to prove that *Sigillaria,* which was the chief plant from which our beds of coal had been formed, was a tree which branched like the *Lepidodendron,* and was not the strange stunted-topped plant which some authors had represented it to be.

The PRESIDENT drew attention to the late fatal explosion at Peterborough, and asked whether the easy method of testing steam boilers, described some years ago by Dr. Joule, was forgotten or found to be impracticable?

Dr. JOULE said that he had taken pains to give his method, by which the testing by hydraulic pressure could be applied with the utmost facility by simply filling the boiler with water and then raising its temperature a few degrees, a very extended publication. He believed that the objection raised by some to its use, was the absurd one that hydraulic pressure injured the boiler. The very object of a test was to detect weak boilers for the purpose of strengthening or rejecting them. He was at a loss for terms strong enough to express his opinion of the reckless disregard of life, or the ignorance which resulted in the deplorable catastrophes which were constantly occurring; and he believed that the only method of cure would be that proposed by Mr. Binney in the case of the explosion of firedamp in mines, namely, that the parties to blame should be compelled to support the widows and orphans of their victims.

Mr. Alderman Pochin stated that he had made use of Dr. Joule's plan, and found it quite practicable and easy of application.

A paper was read "On a New Re-agent for the Separation of Calcium from Magnesium," by Edward Sonstadt, Esq.

When, in the ordinary course of qualitative analysis, carbonate of ammonium is used to separate calcium from magnesium, unless the former is present in notable proportion to the latter, a very insoluble double carbonate of magnesium and ammonium always accompanies the carbonate of calcium, if this is allowed sufficient time to form. If much magnesium and *no* calcium is present, the magnesium precipitate still falls after a while. Both metals are precipitated by this re-agent, the only difference being that the calcium precipitate forms somewhat earlier than the magnesium precipitate. This fact is cursorily mentioned by Fresenius, more fully by Gmelin, and has recently been made the subject of special notice by Dr. Dyer. Calcium, therefore, can only be separated from magnesium by this re-agent, by fractional precipitation, which necessarily involves loss of substance; and, in qualitative examination, the method is sure to mislead when the proportion of calcium present is small, unless it is controlled by other methods. The same remarks apply in substance to the two other methods of precipitation by sulphuric acid and alcohol, and by oxalate of ammonium. When a moderately strong solution of Epsom salts is treated with sulphuric acid and alcohol, the solution is mostly converted into a crystalline magma; and if it is desired to separate a small proportion of calcium, which we will suppose to be present, the magma must be filtered, dissolved, and subjected to the treatment again and again to separate the sulphate of calcium, when, if the quantity of that salt present be very minute, it must be wholly lost. Of course these remarks do not apply to solutions of calcium and magnesium salts containing much of the former, except in a modified degree. What is true of the sulphuric acid and alcohol process, is true in a more extended sense of the oxalate of ammonium process. I have precipitated within a trace the whole of the magnesium present in a considerable quantity of solution of chloride of magnesium, simply by successive additions of oxalate of ammonium,—the solution being concentrated to its original bulk after the last addition of the re-agent. Yet, in working with this re-agent the rule is, that enough of it must always be added to transform all the magnesium salt into oxalate, since oxalate of calcium is soluble in solution of chloride of magnesium. That some magnesium salt must precipitate with the lime salt under such conditions is obvious; and that it does so is well known, and is, though incompletely, provided for by the process being directed to be repeated upon the precipitate first obtained. This process, therefore, is also one of fractional precipitation, and for it to approach success, the operator must know pretty nearly beforehand how much calcium, in proportion to the magnesium present, he has to deal with. Nevertheless, it is unquestionable that in skilled hands, either of the two last processes is capable of giving close approximations to the truth, when the quantity of calcium present amounts to a few per cent of the mixed salts. When the quantity of calcium is less than 1 per cent, I do not think it is possible to estimate it accurately by any of these processes; and when the proportion is larger, the processes are at least more troublesome, have a wider limit of experimental error, and are more apt to fail in less experienced hands, than the analytical processes in use for estimating most of the other commonly occurring elements.

In common tungstate of sodium we possess a test for calcium which is probably equal in delicacy and in certainty to that of chlorine for silver, or of sulphuric acid for barium.

The action of this test, in a preliminary examination, requires to be ascertained—

(1.) With calcium solutions alone.

(2.) The presence of magnesium.

(3.) The presence of magnesium and ammonium salts, and of these with free ammonia.

(1.) *The behaviour of tungstate of sodium with solutions of calcium salts.* A saturated solution of sulphate of calcium, taken at 13° C. remains perfectly clear on addition of an equal volume of a saturated solution of tungstate of soda for a short time. On warming, when the solution attains the temperature of 42° C, it becomes turbid, deposits a film upon the containing glass vessel, and soon after a dense precipitate falls. To ascertain the limit of the action of the test, the solution of sulphate of calcium was successively diluted to various degrees, and precipitates obtained, until the solution was so dilute that it contained but one part of sulphate of calcium in 114,000 parts water. A few drops of solution of tungstate of sodium were added, the solution warmed, and at 56° C, the solution became distinctly opalescent. An experiment was then made on the distilled water used for the dilution, but it gave no reaction. It was evident that it was possible to push the attenuation much further, and yet get indications of calcium. But this proportion ($\frac{1}{114000}$) is near the limit at which sulphate of calcium may be rendered *distinctly* visible. A solution of chloride of calcium behaves similarly. Sulphate of magnesium is not precipitated by tungstate of sodium, unless the solutions of the two salts are strong. The experiments were made with a solution of pure sulphate of magnesium, of specific gravity

1·114, and containing 11·283 per cent of the anhydrous salt. The solution of tungstate of sodium was saturated (at common temperature), and contained about one-third its weight of dry salt. A mixture of equal parts of these solutions gave no precipitate in the cold, but quickly crystallized when warmed, the crystals being difficultly soluble, and leaving a very slight residue of an insoluble variety of tungstic acid, or of some compound of that acid. But when the mixed solutions above described were very little diluted, the solution remained perfectly clear at any temperature, until the fluid was concentrated by evaporation, when no precipitate, but clear crystals appeared. It is only, therefore, in very concentrated solutions that tungstate of sodium gives—not then a precipitate—but crystals, with sulphate of magnesium. The chloride of magnesium solution behaves similarly, though it was not so closely examined.

(2.) *The behaviour of tungstate of sodium with solutions containing calcium and magnesium.* The earlier experiments seemed to indicate that the presence of magnesium did not at all interfere with the precipitation of the calcium. But on continually diminishing the quantity of the calcium salt while that of the magnesium salt was kept constant, it was found that the latter exercised a very appreciable solvent power. The limiting experiment was as follows:—To 5 cc. of a solution containing 7 parts in 100,000 of sulphate of calcium, were added 3 cc. solution of sulphate of magnesium, containing 11·283 per cent anhydrous salt, 12 cc. water, and a few drops of tungstate of sodium. There were thus, in 2,000,000 parts of fluid, 35 parts sulphate of calcium, and 33,849 parts sulphate of magnesium—the remainder being water, except the small quantity of tungstate of sodium. The reaction was not visible till the fluid reached the temperature of 70° C, when it became apparent, and, on putting it aside to cool, a perfectly distinct film formed on the glass. A similarly attenuated solution of the lime salt, but containing no magnesium, was exposed to the same conditions with the re-agent, and the reaction in the latter case occurred earlier, at a lower temperature, and was more distinct. Nevertheless, the fact remains that, in a mixed solution of the sulphates of calcium and magnesium, the presence of the former may be clearly detected up to the proportion of about 1 part in 56,000 of fluid containing about 1,000 parts of magnesium salt. Rougher experiments made with the corresponding chlorides led to similar results.

(3.) The influence of ammonium salts in obstructing the precipitation of calcium in presence of magnesium is very marked. A calcium salt, in presence of a very large proportion of both magnesium and ammonium salts, cannot be certainly recognised except somewhere near $\frac{1}{4000}$th of the calcium salt be present in solution. The influence of free ammonia with sulphate of ammonium and sulphate of magnesium, in like large proportions, is so great as to only just admit of the recognition of the calcium when from $\frac{1}{800}$th to $\frac{1}{1000}$th is present. Nevertheless, enough, and rather more than enough, ammoniacal salt may be present to prevent any precipitation of magnesium by excess of ammonia, and a moderate excess of ammonia may also be present, without sensibly affecting the estimation of the lime in a quantitative experiment. Chloride of ammonium does not dissolve the precipitate when it is once formed.

The analytical experiments on weighed mixtures of calcium and magnesium salts, imperatively necessary in introducing a new re-agent, are not yet completed, most of the experiments of this kind made till now, having been vitiated through ignorance of the conditions necessary to ensure success. I give, however, the results of one experiment, the conditions of which approached more nearly to those I now know of as being necessary than the others, reserving the series, together with the methods adopted for obtaining pure materials to work with, for a second Paper.

| | Taken. | Found. |
|---|---|---|
| Magnesia ............... | 0·3097 grms. | 0·3120 |
| Carbonate of calcium... | 0·0043 | 0·0042 |

The weighed quantities of carbonate of calcium and of magnesium were dissolved in a slight excess of hydrochloric acid; neutralised carefully by ammonia, precipitated by tungstate of sodium, and then the filtrate, with the usual precautions, by common phosphate of sodium. The excess in the weight of pyro-phosphate of magnesium, led to the suspicion that some tungstic acid had been carried down; a suspicion amply confirmed by the colouration obtained from the solution of the ignited precipitate in dilute hydrochloric acid when treated with tin.

A little in anticipation of my intended future paper upon the subject, I now add such details respecting the manipulation required in separating lime from magnesia by tungstate of sodium as my experience has shown to be necessary. It is convenient to have the solution of the magnesium and calcium salts made somewhat alkaline by ammonia, but a very large quantity of this, as well as of ammoniacal salt, is, as we have seen, to be avoided. The beaker in which the precipitation is to be effected should, while perfectly dry and warm, be rubbed within by chamois leather on which a drop or two of fine oil (such as is used for oiling balances) has been put. If this precaution be not taken, it will be found impossible to detach the precipitate of tungstate of calcium from the sides and bottom of the vessel. A considerable excess of the re-agent is not necessary; but, if it occur, is not material. If, on addition of the re-agent, a white, floculent precipitate forms immediately, it is well to add a few drops of ammonia, when the floculent precipitate will re-dissolve, but if it does not re-dissolve, after warming, there is some other element present, which, if ordinary Epsom salts are used will probably be manganese. The tungstate of calcium precipitate is very dense; it forms slowly in very dilute solutions, and, in all cases, several hours should be allowed for it to form. The solution should be warmed meanwhile, but must not be allowed to boil. The precipitate must be washed till the filtrate shows no cloudiness on standing, with nitrate of silver when the salts are chlorides; or, if they are sulphates, till chloride of barium gives no cloudiness. The precipitate must then be further washed with dilute solution of ammonia, but these washings need not be saved. The filter should be burnt separately, after the precipitate is cleared from it as nearly as possible. After the ignited precipitate is weighed, a little strong solution of ammonia should be poured upon it, and allowed to stand for awhile, when the ammonia is decanted, and supersaturated with acid. If a precipitate falls after a time, the tungstate of calcium precipitate should (without being removed from the crucible) be allowed to stand for some hours with more ammonia—it is then washed by decantation, again ignited, and weighed. The ignited precipitate should be perfectly white.

The filtrate, containing the magnesium salt and tungstate of sodium, may be at once precipitated by phosphate of sodium in the usual way, but if this is done, much washing is required to get rid of the little tungstic acid that adheres obstinately to the precipitate. It is better, especially when a great excess of the re-agent has been used, to first precipitate the tungstic acid, by a considerable excess of hydrochloric acid, and boil until the precipitate becomes dense and intensely yellow. The solution is then filtered, supersaturated with ammonia, and the magnesia precipitated in the usual way; but, even in this case, it is better to wash lastly with stronger ammonia solution than ordinary.

## MICROSCOPICAL SECTION.

January 16th, 1865.

J. Sidebotham, Esq., President of the Section, in the Chair.

### *Exhibitions and Presentations.*

A box of two dozen slides of botanical specimens, beautifully mounted, presented to the Section by Mr. J. E. Whalley.

Specimens of carbonate of magnesia from Greece; the surfaces showing very delicate dendritic markings of oxide of manganese, well seen under the microscope.—Mr. A. Brothers.

A fine specimen of Polyporus versicolor.—Mr. Grindon.

A set of mounted specimens of leaves from the south of Europe and the Rocky Mountains, showing curious and novel markings.—Mr. W. H. Heys.

### *Communications.*

The following note from Mr. Dancer, addressed to the President of the Section, was read:—

"Sir,—I beg to state that, since our last meeting, I have carefully examined, with various powers of the microscope, the cotton hairs whilst undergoing dissolution in Schweizer's

Ammoniacal Solution of Copper. I am inclined to the belief that cotton hairs do not contain spiral vessels properly so-called. I think that the spiral apparatus, which has been described by Mr. C. O'Neill and Mr. Heys as spiral vessels, can be clearly traced to a mechanical action which the solvent exerts on the vegetable cell. At some future time I hope to illustrate this to the Section.—Yours truly,

"J. B. DANCER."

Mr. HEYS explained that he had not intended to describe the cotton hairs as containing spiral vessels, in the botanical sense of that term, but had spoken of the appearance within them as that of a spiral thread.

Mr. WATSON read a communication "On the Plumules or Battledore Scales of the Lycænidæ," in which he showed that they will serve the purposes of identification by exhibiting generic and specific alliances, and differences similar to those found in the plumules of the Pieridæ, described by him in a previous paper. Fifty-three figures of the plumules, drawn by Mr. Sidebotham, were shown as illustrations of the subject. He said the points he desired to insist upon as likely to be useful in this investigation were—That the plumules are always identical in individuals of the same species, and mere varieties can therefore be detected by this test; and that, in very closely-allied species which are difficult of distinction by the more ordinary characters, these scales will often be found to be different.

Mr. SIDEBOTHAM read "Notes on the Development of the Wings of Lepidopterous Insects." He said that their great and rapid increase of size soon after the insect emerges from the chrysalis is caused by air, taken in through the spiracles, being sent into the vessels of the wings; the membrane is expanded in consequence, and the scales, which were before packed under each other as closely as possible, are made to slide out until they remain in the fully developed wing like the tiles of a roof. He exhibited preserved specimens of the Currant moth and the Tiger moth, with the wings both in their small and in their expanded state, also a coloured sketch of one of them, and it was seen that in the unexpanded state the wings lie flat without any folding, and all their markings are a correct representation in miniature of what they ultimately become.

### January 30th, 1865.

J. SIDEBOTHAM, ESQ., President of the Section, in the Chair.

### *Exhibitions and Presentations.*

Two powers for the Microscope (⅛in. and ¼in.), made by Mr. Dancer, presented to the Section by the Right Rev. the Lord Bishop of Manchester.

Remains of an Ichthyosaurus.—T. Alcock. The points noticed were the bones situated in the triangular space enclosed by the lower jaw, which were well shown as the creature lay on its back; a broken rib reunited during life; the numerous small bands of bone, three or four corresponding with each rib, encircling the body in front; remains of skin in various parts; and, in the situation of the stomach, a layer of minute teeth and fragments of scales, the remains of food taken during life.

Mountain Limestone fossils from Clitheroe. A new Rhynchonella not yet specifically named; many beautiful specimens of Crania; and a set of extreme varieties of Orthis resupinata.—Mr. Parker.

Double feathers from pheasant.—Mr. Latham.

A set of the photographs of Dinornis in the York Museum. Dried specimens of the male flowers of Aucuba.—Mr. Grindon.

## *Communications.*

Dr. ALCOCK read "Notes on a Visit to Walton Hall," with remarks on Mr. Waterton's method of preserving animals.

Dr. CARRINGTON read communications "On an Annelidan Larva," and "On the Embryology of Annelids."

## Ordinary Meeting, February 21st, 1865.

R. Angus Smith, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Thomas Worthington was elected an Ordinary Member of the Society.

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M. Breguet, of Paris, exhibited and explained the construction of Dumas' lamp for use in coal mines, the principle of which consists in the employment of the light from a Geissler's vacuum tube, excited by a small Ruhmkorff's induction coil.

Professor Roscoe stated that he had very frequently been asked for information respecting the mode of preparing the sealed bulbs, containing exactly equal volumes of chlorine and hydrogen gases, which he employed for exhibiting the chemical combination of these gases effected by the action of light, and as the successful preparation of these bulbs depends upon exactly observing certain minute conditions, he ventured to submit the following particulars to the Society. The apparatus needed, consists of a stout tube or narrow bottle of about 120 cubic centimetres capacity, fitted with a caoutchouc stopper with three holes bored through it. Into one of these holes a vent gas delivery tube passes, on to which three small wash bulbs are blown; into the other two holes are inserted the rounded ends of two lengths of the gas carbon, commonly used as terminals for the electric lamp; these poles are of such a length that they pass to the bottom of the glass bottle. This is then filled with strong aqueous hydrochloric acid containing about 30 per cent of the anhydrous acid; the stopper in the poles and wash bulbs containing a few drops of water is then fixed into its position, and the evolution vessel placed in a beaker of cold water whilst contact is made with the terminals of four ordinary sized Bunsen's cells, the whole apparatus being placed in a dark room. The mixed gases at once begin to be given off, and ought to pass through the wash bulbs at the rate of about two bubbles per second. It is absolutely necessary that the gas be allowed to come off at this rate for three hours* before it is collected, as up to this time it does not attain a sufficient degree of purity and sensitiveness, whilst after the lapse of this time it is generally found to be fit for use. In order to absorb the excess of chlorine, the waste gas may be led into a condenser containing slacked lime and charcoal in alternate layers. When the evolution has gone on for the above mentioned time a bulb tube, connected by caoutchouc joinings, is placed between the evolution vessel and the condenser, and the gas allowed to pass through. The bulbs, which are made of fusible glass tubing, are blown about the size of a hen's egg, and so thin that they easily break when pressed with the finger. At each side of the bulb the tube is drawn out so as to be very thin in the glass, and to leave the internal diameter not less than 1mm., whilst at the extremities the tube is wider so as to fit ordinary joinings. When the gas has passed through the tube for about ten minutes, the joinings are loosened and each end stopped with a piece of glass rod. The bulb tube thus closed is then removed from the evoluting vessel, and the thinnest part of the tube brought some little distance above a very small Bunsen's flame; the glass then softens below a red-heat, and the ends may be drawn out and sealed with safety. It is advisable to number the bulbs, and to test the first and last by exposing them to a strong light. Frequently, in spite of every precaution, the gas explodes during the act of sealing, so that in this operation it is advisable to hold the bulb with a cloth rather than in the open hand. As soon as one bulb tube is removed another is placed in connection with the evolution flask, and after ten minutes sealed as described. The above quantity of acid will serve for the production of sixty bulbs. Thus prepared the sealed bulbs may be bept in the dark for any length of time without injury; some, which were known to have been made more than a year, were found to be perfectly good. To explode these bulbs it is only necessary to expose them to diffuse daylight or sunlight, when the combination occurs instantly. Of artificial lights the bright flash produced by the combustion of the vapour of bisulphide of carbon in nitric oxide is most effective; but the light of burning magnesium wire, of phosphorous in oxygen, or the electric light, answers perfectly well Professor ROSCOE stated that Mr. Dancer, of Cross Street, had undertaken to supply the bulbs to persons unable to prepare them.

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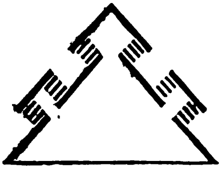
* See Bunsen and Roscoe, Photochemical Researches, 1857, p. 355.

A paper "On a new form of roof for Dyehouses," by Mr. JOHN THOM, was read, communicated by the President.

The object of the present communication is to describe the construction of a roof for buildings in which there is a good deal of vapour, so as to produce the minimum amount of condensation of such vapour inside the buildings, and thus avoid the production of drops, as well as the minor evil of an obscure atmosphere. Any one practically acquainted with dyeing processes knows well the loss which arises from these two evils. In all cases that I have seen, except where the dyehouse was the lower flat of a series, and where, of course, the covering of the dyehouse was the floor of the room above, the roof of a dyehouse is made about the usual angle or pitch of an ordinary building supplemented with openings, and these openings on different plans, for allowing the steam or vapour to escape. The usual one being made thus:—



This form seems to send out a large amount of steam or vapour from the quantity of cold air which is mixed with the vapour inside and escapes with it. If one watches the action of the cold air and vapour in this form of roof, it will be observed, that the cold air comes in by all the windward openings, strikes the hot moist air or vapour, drives part out and forces a great part down into the building. I found by taking off the side lifts, and leaving only the top ones, that an improvement was effected. Still, however, the same evils were produced by the top louvre boards, although in a less degree, owing to a less quantity of cold air being admitted.

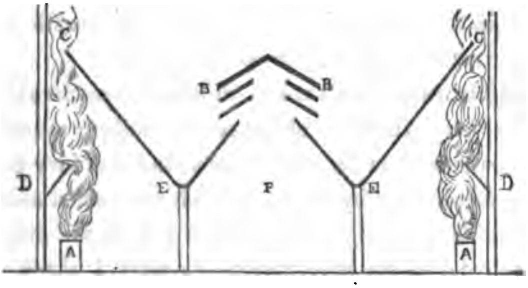
I also tried removing the top cover, hood, or louvre boards, and contracting the opening, leaving three feet all along the top of the roof without any cover whatever; and this, in spite of the admission of rain, I found to be the best of the old form. Even with this, however, we had always drops and an obscure atmosphere, except in bright dry days. But with this form the number of drops were less and the building clearer than with so many openings.

With the old form of roof the most successful plan of keeping the building free from vapour or drops, is to cover over every vessel from which vapour arises, and to have a tube passing from each cover quite through the roof. But this plan has several objections to it; the chief being, that owing to the boxing in of the winces, &c., one does not see how the goods are going on, and whether the processes are being conducted successfully or not. The workmen also aggravate this evil, by avoiding as much as possible the opening of the covers or boxes to see the goods, on account of the blast of steam which issues out when an opening is made. I have twice fitted up such covers and tubes to all our dyebecks, and both times have been obliged to remove them. The last time I fitted them up I made for the front a light frame with canvass, and this could be raised by a cord and pulley, so as to allow the man to avoid the steam when he opened the box.

Being dissatisfied with all the plans I had tried of getting rid of the vapour, I determined to try a new roof.

Now, if all idea of the old form of roof is laid aside, and it is asked what it is that is really wanted, the conclusion will be that condensation of the vapour must be avoided; that the less air admitted the better, and that the air be admitted under the vapour and pass with it in the same direction, out of the building, mixing as little as possible with it. The admission of air must be at the command of the foreman of the building, and the roof should be at such a pitch that if drops be condensed on it they should not fall; but trickle along the spar or glass into the gutter made to receive them.

Adjoined is a sketch of the plan which I have ultimately adopted to effect these objects.



A A becks along the sides. B louvre boards which can be opened or shut to admit air. C C openings for escape of hot vapour, having the same number of feet of opening as surface of hot water. D D boards which catch rain drops from unprotected part of roof. F part for assorting goods, &c. All the roof is timber and glass; there are no slates.

By this plan the opening for emission of steam is as small as can be allowed, and all the air necessary is admitted at the proper place, and in such quantity as may be desired. This roof has now been at work in summer and winter, and a drop has never been seen to fall from it except sometimes from gutter E E, and this being invariably in one place it is of no consequence. Except in unusually close weather, the whole place is quite free from condensed vapour. The men are all dry and comfortable, instead of being stifled with the hot vapour and drenched with drops as they formerly were before this new form of roof was tried. There are many details which I cannot describe here, but which I show on the model and drawings. Modifications would be required for different arrangement of becks; but the same principle could always be carried out.

A paper was also read "On the Action of Caustic Soda on Ethylic and Methylic Alcohol," by Mr. A. Mylius, communicated by Dr. E. Schunck, F.R.S.

Schunck's experiments concerning the action on indigo blue of acetate of soda, caustic soda, and alcohol, first led me to examine in his laboratory the influence of caustic soda on alcohol and methylic alcohol in sealed tubes.

I obtained in each case a resin, differing considerably from the resinous body which is obtained by boiling an alcoholic solution of caustic soda for some time at the ordinary pressure and then precipitating by an acid. This so-called aldehyde-resin has a different composition according to the proportion of acid employed in its preparation, so that I think it not improbable that it may be composed of two resins. Its colour is dark brown, and it is soluble in alkali.

On heating the same solution for some time in sealed tubes, so as to obtain increased pressure, there is formed by the influence of the alkali a resin which is insoluble in alkali. In a short time the liquid becomes red, and on the addition of water the resin is precipitated, the liquid becoming colourless. This takes place also when methylic alcohol is employed.

The resins thus formed are of a red colour.

After filtration and washing with water the resin was dissolved in alcohol, and the residue, after evaporation, dried at 100° C. When cold these resins are hard and brittle, but they have no crystalline structure. They are soluble in alcohol and ether. The resin formed from methylic alcohol melts at 59° C., the other at 65° C. The odour of the former is like that of cedar wood, while that of the latter more resembles the smell of oranges. In general there is a great resemblance between these resins and the natural resins, such as copal, &c. In employing methylic alcohol the formation of the resin takes place much more easily, and a greater quantity of product is obtained. I tried in various ways to decolorise the resins, but did not succeed.

Through the alkaline liquid filtered from the resin a current of carbonic acid was passed, then it was evaporated, and the saline residue having been treated with sulphuric acid in excess, the liquid was distilled. The distillate was acid; silver solution was reduced by it in an instant, so that there was little doubt of the presence of formic acid (but the peculiar smell of propionic acid could not be perceived).

From the analysis it must be inferred that the two resins have the same composition; but still, as their properties are not identical, I think they are only isomeric.

The analysis afforded the following results:

I. 0·1860 grm. (methylic alc.) gave 0·5080 grm. carbonic acid and 0·1550 grm. water.

II. 0·2465 grm. (methylic alc.) gave 0·6795 grm. carbonic acid and 0·1965 grm. water.

III. 0·2090 grm. (ethylic alc.) gave 0·5745 grm. carbonic acid and 0·1590 grm. water.

IV. 0·1805 grm. (ethylic alc.) gave 0·4960 grm. carbonic acid and 0·1330 grm. water.

These numbers lead to the following composition:

| | I. | II. | III. | IV. |
|---|---|---|---|---|
| C | 74·50 | 75·10 | 74·97 | 74·95 |
| H | 8·90 | 8·85 | 8·95 | 8·26 |
| O | 16·60 | 16·05 | 16·08 | 16·79 |
| | 100·00 | 100·00 | 100·00 | 100·00 |

The formula $C_{12} H_8 O_2$ with which they correspond requires in 100 parts

| | |
|---|---|
| C | 75·00 |
| H | 8·40 |
| O | 16·60 |
| | 100·00 |

The formation of the resins from the alcohols might perhaps be explained by assuming that aldehyde is formed by the oxidation of alcohol, and then by simple loss of water the aldehyde would be converted into resin, since

$$3(C_4\ H_4\ O_2) - 4HO = C_{12}\ H_8\ O_2.$$

But the formic acid in this case would not stand in any relation to the resin, and even the formation of the resin from methylic alcohol could not easily be explained.

If we adopt the formula $C_{24} H_{18} O_4$, which requires 74·28 per cent carbon and 9·10 per cent hydrogen, we must assume that carbonic and formic acid are taken up by the alcohol, though it is very doubtful whether these acids could separate from a strong base like soda in order to form a neutral resin. The following equations will show what may be imagined to take place in this case:

$$5(C_4\ H_6\ O_2) + C_2\ O_4 + C_2\ H_2\ O_4 = C_{24}\ H_{18}\ O_4 + 14\ HO.$$

$$10(C_2\ H_4\ O_2) + C_2\ O_4 + C_2\ H_2\ O_4 = C_{24}\ H_{18}\ O_4 + 24\ HO.$$

Taking two atoms of water from $C_{12} H_8 O_2$, the formula of benzole, $C_{12} H_6$, remains. I tried to form this carbo-hydrogen by distilling the resin; the distillate had a strong smell of carbolic acid; I acted on it with nitric acid; oxidation took place immediately after the addition of the acid. I now added water, which gave a yellow deposit. I dissolved the latter in alcohol, but on evaporating the alcohol I obtained a resin of the same appearance as the original one. Nitrobenzole had not been formed.

The two resins are of a constant composition, which is proved by the accordance between the third and the fourth analyses, which were made with specimens prepared at different times.

In order to ascertain whether formic or acetic acid takes part in the formation of the resins, I made two other experiments. In one case I added acetate of soda to the caustic soda and alcohol, and in the second case formiate of soda. I analysed the products, and arrived at the following results:

I. 0·2050 grm. (acetate of soda) gave 0·6190 grm. carbonic acid and 0·1625 grm. water.

II. 0·2255 grm. (acetate of soda) gave 0·6800 grm. carbonic acid and 0·1745 grm. water.

These numbers lead to the following composition:

| | I. | II. |
|---|---|---|
| C | 82·37 | 82·24 |
| H | 8·80 | 8·58 |
| O | 8·83 | 9·18 |
| | 100·00 | 100·00 |

The formula $C_{28} H_{18} O_2$ requires

| | |
|---|---|
| C | 82·90 |
| H | 8·91 |
| O | 8·19 |
| | 100·00 |

Assuming that acetic acid is necessary for the formation of this resin, which differs from the products obtained without the addition of the acetate, the following equation will show the way in which the resin has been formed:

$$4(C_4 H_6 O_2) + 3(C_4 H_4 O_4) = C_{28} H_{18} O_2 + 18 HO.$$

In employing formiate of soda I obtained a resin of the same composition as that produced when no formiate was present.

0·2100 grm. gave 0·5795 grm. carbonic acid and 0·1610 grm. water = 75·25 per cent carbon and 8·57 per cent hydrogen.

The investigation has therefore led to the discovery of the following facts:

(1) The resins which are obtained by the action of caustic soda on ethylic and methylic alcohol in sealed tubes differ from the resin that is formed by the same substances at the ordinary pressure in open vessels.

(2) Methylic and ethylic alcohol produce resins of the same composition.

(3) Formic acid is formed.

(4) When acetate of soda is added the resulting resin differs in its composition.

## PHOTOGRAPHIC SECTION.

January 5th, 1865.

Joseph Sidebotham, Esq., in the Chair.

Mr. E. C. Buxton was elected an Ordinary Member.

Some enlarged photographs by Dr. Van Monkhoven were exhibited. The original negatives were four inches square, and some of the enlargements upwards of forty-four inches.

Some specimens of panoramic pictures taken by Mr. J. R. Johnson with his pantascopic camera were exhibited. In this camera the lens turns on its optical centre, depicting the objects composing the view on the screen or collodionised film, which moves tangentially in a contrary direction to the motion of the lens, and thus successive portions of the view are received by successive portions of the moving plate.

Mr. Dancer, F.R.A.S., read a paper "On the exhibition, stereoscopically, of Photography on a large scale." Diagrams were exhibited of the single image prism stereoscope, the double reflecting stereoscope, and the opera glass stereoscope instruments devised by Sir David Brewster for uniting binocular pictures for the use of lectures, &c. Some beautiful stereoscopic transparencies were cut in two, and each half placed in its position in oxhydrogen lanterns mounted with achromatic object glasses. The half stereographs were then projected in juxtaposition on a long screen, and to realize the proper stereoscopic effect, the members were supplied with achromatised prismatic stereoscopes which had been prepared expressly for the purpose. Mr. DANCER made some remarks on the perfection which the lenticular stereoscope had attained by the employment of achromatic lenses, and urged the necessity of photographers and opticians agreeing to uniform foci for the lenses of stereoscopic cameras and stereoscopes, as it was only by viewing the pictures with lenses of equivalent foci to those with which they were taken that they got a truthful representation. If that rule were observed, the objects would have the same apparent magnitude as they would have to the eyes of a person standing on the spot from whence the view was photographed; that well known rule was too frequently forgotten.

February 2nd, 1865.

J. P. JOULE, LL.D., F.R.S., Vice-President of the Section, in the Chair.

Mr. A. BROTHERS, F.R.A.S., exhibited a stereoscopic picture of the Blue John Mine in Derbyshire, which he had taken by aid of the magnesium light, giving an exposure of five minutes. The negative was slightly fogged, owing to the lenses not having been wiped when descending from the cold atmosphere of the surface into the mine, which is some 3 to 400 feet below it. The dense fumes of magnesia caused by so long an exposure prevented another trial.

Dr. **Joule** exhibited a camera, which was an improvement on the one described in the Photographic Notes, September, 1856. The body of the camera consists of a mahogany box holding a wide glass bottle, which is made use of to hold the silver solution. The plate is sensitised by turning the camera on its back, which causes the nitrate of silver to flow over it.

Mr. **Montefiore** described some experiments which he had made for enlarging negatives by aid of the magic lantern, using magnesium wire for the illuminating power. The wire had been found to burn very steadily in a jet of ordinary gas.

Mr. A. **Brothers**, F.R.A.S., read a paper entitled "Was Daguerre a Discoverer?"

Mr. **Sidebotham** read a paper "On the proper focus of lens to be used in taking photographic landscapes, also on some modes of measuring the size of objects therein depicted."

The object of the paper was to show that neither lenses of very long or very short focus give correct perspective, the former making distant objects appear too near, and the latter just the contrary. Lenses of less focus than 7 inches, and greater than 14, make this defect painfully visible.

The latter portion of the paper advocated the placing a rod or mark of such a length, and in such a position in the picture, that it could be used as a scale of measurement for the principal objects. Also that a correct scale be made for each focus of lens, by placing poles of measurement at the distance of 100 yards apart, and marking each 10 yards distance from the camera, so that when once the scale was formed, all photographs taken by that focus of lens (the distance of objects being known) their size could be measured; or their size being known their distance.

The author, also explained the mode of obtaining the exact measurement of objects, by laying divided scales lengthways and across, so that any person could verify the measurement from the photograph without risk of error. This had been suggested to the Astronomer Royal for Scotland, Professor Smyth, who was at present in Egypt, taking measurements of certain portions of the interior of the great Pyramid, and extracts from some of his letters were read, approving the same; the illumination of the objects being accomplished by the use of Magnesium.

## Ordinary Meeting, March 7th, 1865.

R. **Angus Smith**, Ph.D., F.R.S., &c., President, in the Chair.

The following paper "On the Action of Sea water upon certain Metals and Alloys," was read by F. **Crace Calvert**, Ph.D., F.R.S., F.C.S., &c., and R. **Johnson**, F.C.S.

We were induced to examine the action exerted by sea water, in consequence of the rapid changes which have taken place of late years in naval architecture, and especially in the substitution of metals and alloys for wood.

To carry out the above views, we took 20 square centimetres of each metal, which we cleaned with great care and attention, in order that the action of the sea water might have its full effect; then two plates of each metal were placed in separate glass vessels, and immersed in equal volumes of sea water. After one month the plates were taken out, and any compounds that had adhered to the surface carefully removed; the plates were then dried and reweighed, and the loss estimated. To render our results of more practical value, we have calculated the action of 100 litres of sea water upon one square metre of each metal, and the following are the amounts of metals dissolved:—

| | Grammes. |
|---|---|
| Steel | 29·16 |
| Iron | 27·37 |
| Copper (best selected) | 12·96 |

| | Grammes. |
|---|---|
| Copper (rough cake) | 13·85 |
| Zinc | 5·66 |
| Galvanised Iron | 1·12 |
| Block Tin | 1·45 |
| Stream Tin | 1·45 |
| Lead (virgin) | Trace. |
| „ (common) | Trace. |

These results appear to us to lead to the following conclusions:—

1. That the metal now most in vogue for shipbuilding, namely, iron, is that which is most readily attacked.

2. That this metal is most materially preserved from the action of sea water when coated with zinc, and therefore, in our opinion, it would amply repay shipbuilders to use galvanised iron as a substitute for that metal itself.

The above facts perfectly confirm those which we have already published in our paper "On Galvanised Iron for Armour Plated Ships," in which it was shown, that when iron was in contact with oak they mutually acted upon each other, producing a rapid destruction of the two materials, whilst little or no action took place between galvanised iron and the wood.

3. The extraordinary resistance which lead offers to the action of sea water, naturally suggests its use as a preservative to iron vessels against the destructive action of that element; and although we are aware that pure lead is too soft to withstand the wear and tear which ship bottoms are subjected to, still we feel that an alloy of lead could be devised which would meet the requirements of shipbuilders.

Feeling that experiments made with a limited amount of sea water might not be a fair criterion of the action of the ocean upon metals, we repeated our experiments upon plates of 40 centimetres square, which were immersed for one month in the sea on the western coast (Fleetwood), taking the precaution that they should be constantly beneath the surface of the water, and suspended by flax rope attached to a wooden structure, to prevent any galvanic action taking place between the plates and the structure to which they were attached.

The following are the amounts of metals dissolved:—

| | Grammes. |
|---|---|
| Steel | 105·31 |
| Iron | 99·30 |
| Copper (best selected) | 29·72 |
| Zinc | 34·34 |
| Galvanised Iron | 14·42 |
| Lead (virgin) | 25·69 |
| „ (common) | 25·85 |

The above figures suggest the following remarks:—

That the action has been much more intense, in this instance, than when the metals were placed in a limited amount of water at the laboratory. These results are due probably to several causes acting at the same time, viz.:—that the metal was exposed to the constantly renewing surface of an active agent; and that there was also a considerable friction exerted on the surface of the plate by the constant motion of the water, there being at Fleetwood a powerful tide and rough seas. What substantiates this opinion is, that the lead plates undoubtedly lost the greater part of the weight, not by the solvent action of the sea water, but from particles of lead detached from them, in consequence of their coming in contact with sand and the wooden supports to which they were attached; but this cause of destruction having been observed with lead plates, it was afterwards carefully guarded against in the case of all the other metal plates.

We also deemed it desirable to examine the action of sea water on various brasses. We therefore immersed for one month, plates of various alloys in that fluid, and proceeded to record our results:—

### Action of 200 Litres of Sea Water upon 1 Square Metre Surface of the following Brasses:

| *Composition of the Brasses.* | *Quantity of Metals Dissolved.* | | | |
|---|---|---|---|---|
| | IRON. | COPPER. | ZINC. | TOTAL. |
| Pure Copper ... 50<br>Pure Zinc ...... 50<br>100 | — | 1·110 | 10·537 | 11·647 |
| Commercial Brass:<br>Copper......... 66·<br>Zinc............ 32·5<br>Iron and Lead 1·5<br>100 | 0·579 | 3·667 | 3·324 | 7·570 |
| Muntz Metal (Sheet):<br>Copper......... 70·<br>Zinc............ 29·2<br>Iron and Lead 0·8<br>100·0 | 0·438 | 4·226 | 2·721 | 7·385 |
| Muntz Metal (Bars):<br>Copper......... 62<br>Zinc............ 37<br>Lead and Iron 1<br>100·0 | 0·501 | 2·697 | 3·493 | 6·691 |
| Prepared Brass:<br>Copper......... 50<br>Zinc............ 48<br>Tin ........... 2<br>100 | TIN.<br>0·365 | 7·04 | 3·477 | 10·882 |

The above table shows how very differently sea water acts upon divers brasses and the influence exercised upon the copper and the zinc composing them, by the existence in them of a very small proportion of another metal; thus, in pure brass the zinc is most rapidly dissolved (which, *en passant*, is the contrary to what takes place in galvanised iron), whilst it acts as a preservative to the copper.

Tin, on the other hand, appears to preserve the zinc, but to assist the action of sea water upon the copper.

The great difference between the action of sea water upon pure copper and upon Muntz metal seems to us to be due not only to the fact that copper is alloyed to zinc, but to the small proportion of lead and iron which that alloy contains; and there can be no doubt that shipbuilders derive great benefit by using it for the keel of their vessels.

We were so surprised at the inaction of sea water upon lead that we were induced to compare its action with that of several distinct varieties of water, viz., Manchester Corporation water—well water—distilled water in contact with air—the same deprived of air—and the following are the amounts of metals dissolved by 200 litres of these waters upon 1 square metre of surface during eight weeks.

| | Grammes. |
|---|---|
| Manchester Corporation Water ........... | 2·094 |
| Well Water .................................... | 1·477 |
| Distilled Water (with air) ................. | 110·003 |
| ,, ,, (without air) .............. | 1·829 |
| Sea Water ........................................ | 0·038 |

These figures require no comment, as they confirm our previous result that sea water has no action on lead.

Mr. JOHN ROBINSON exhibited specimens of iron and brass which had been acted upon by the water of the river Medlock, and stated he had found that an alloy of lead, tin, and antimony, resisted the action of sea water better than any other metal or alloy he had tried.

## PHOTOGRAPHIC SECTION.

March 2nd, 1865.

Mr. JOHN PARRY in the Chair.

Mr. DANCER read a paper entitled, "The Opaque Microscope not New," in which he proved that the so-called new instrument was of very old date, and was described and used previous to the year 1780; and that an improved form of it was exhibited nightly at the Manchester Mechanics' Institution in the winter of 1840-1841.

Mr. SIDEBOTHAM exhibited some prints taken by the Wothlytype process, and described the mode of their production. Others printed by the ordinary silver process from the same negatives were also exhibited, and pronounced to be superior in every way.

Mr. BROTHERS exhibited two photographic prints taken by Mr. Pouncy, of Dorchester, one of them obtained direct from the negative in printers' ink, the other printed from stone. Mr. Brothers also exhibited, with a copying camera, the mode of obtaining photographs of microscopic objects by burning magnesium; also a negative and a print from it of an insect dissection so enlarged.

Mr. E. C. BUXTON, jun., read a paper entitled "Photographic Experience in India," and illustrated it by a number of large and beautiful views he had taken in 1863 and 1864.

The difficulties of photography in hot climates such as India are usually believed to be very great, and until quite recently Indian photographs—or, at least, such as *I* had the chance of seeing—were little more than patches of black and white. No doubt there were many exceptions, but I think that was the general character of Indian photography. The hardness and want of half-tone was attributed to the intense light of the sun, the heat, and the difficulty of preserving chemicals in good working order.

I remember reading a letter in one of the journals from a photographer in India, in which he said that the plates could not be kept moist more than a few seconds after leaving the bath, and that to prevent the surface of the film from drying so rapidly he was obliged to fasten a pad of wet blotting-paper inside the shutter of the dark slide. How he contrived to draw out the shutter I cannot understand. My own opinion is, that a photographer who can take good negatives in this country will succeed equally well, if not better, in India, or other hot climates.

A few days before leaving Calcutta, at the beginning of last April, I gave a friend some lessons in the art of coating and developing a plate. In July I received prints from his negatives superior to the ordinary productions of amateurs in this country of two year's practice.

The Exhibition of the Bengal Photographic Society closed just after my arrival in Calcutta. I could only pay one short visit; but my impression was that the number of good pictures was larger than in the annual Exhibition of the London Photographic Society. Probably this improvement in the art is owing to the general use of bromo-iodised collodion, and the greater purity of the nitrate of silver and other chemicals.

About four years since I spent the winter in Algeria and Egypt, taking photographs both with the wet and dry process. Neither country is very warm at that time of year; but the excessive dryness of Egypt gave me some little insight into what was wanted in the photographic line when I started for Singapore in September, 1863. I had previously asked advice from photographers who had worked in India, mentioning the little difficulty I experienced in Egypt. They shook their heads, evidently pitying my ignorance. "Only wait," said one, "till you find your nitrate bath as black as ink some fine morning!" I humbly ventured to hope that such a sad fate was not in store for my bath. Another positively declared that no man except Ottewill could make a camera warranted not to warp or crack. I was also advised to make my own collodion on arriving out, or at least to mix the iodisers myself. My knowledge of chemistry being very limited, the prospect of turning collodion manufacturer was anything but agreeable. Nevertheless, I laid in a stock of ether and alcohol, and all sorts of iodides and bromides. Gun cotton I did not trouble my head about, though where I expected to find any in Singapore is more than I can now remember. Fortunately my skill was never put to the test.

In *this* country Thomas's collodion with magnesium iodiser had always been my favourite, but after a few weeks of hot weather the loss of sensitiveness is very great; therefore, in addition to several pounds of Thomas's collodion packed in tin cases with sawdust, I took a quantity of Mawson's. This was ordered specially for India. Nothing could possibly work better than it did: it was perfection. All the other chemicals required—such as nitrate of silver, pyrogallic and acetic acids. &c.—should be taken from this country. They are to be had in Singapore, Hong Kong, and the principal towns in India; but the prices usually charged are ruinous, and no purchaser can tell how long the article he requires may have been in stock.

The most important matter, after all, is the choice of cameras, lenses, and a good dark tent. The tent I used was Smartt's, made by Murray and Heath. The time required for setting it up and arranging the bath and developing bottles in their proper places is about five minutes. Murray and Heath also supplied me with a square bellows camera for 11 × 9 plates, fitted with Dallmeyer's No. 3 triplet and Grubb's C lens, together with a field box for chemicals and glass plates. Mr. Rogerson made all the rest of my apparatus, viz., a rigid stereoscopic camera, with rack-and-pinion, fitted with Dallmeyer's single and combination lenses, plate boxes, and draining boxes for holding negatives in the field. These draining boxes are also most useful for carrying negatives on a long journey, such as the overland route, and will stand almost any amount of knocking about without injury to the contents. I need only say that a box similar to the one you now see, when full of negatives, fell to the ground from an elephant's back: the box was a little worse, and that was all. A nest of three ebonite funnels will be found very useful: glass funnels are certain to be broken. The developing cups may also be of ebonite, but I must say I prefer glass.

On arriving at Singapore the keys of my boxes could nowhere be found. A couple of Chinamen kindly undertook to pick the locks and make new keys. Their "little bill" amounted to three dollars—about thirteen shillings! During my stay in the island there was scarcely a morning when the foliage was absolutely still. At sunrise a breeze nearly always sprung up, and continued more or less till sunset; then it became perfectly calm, and during the night there was frequently heavy rain, with thunder and lightning.

My first attempt at photography was rather disagreeable. I had placed the camera under the shade of a large bamboo, and was focussing carefully. I ought to mention that the morning dress in Singapore is made as light and cool as possible. Loose trousers of silk or calico, grass slippers, and a flannel shirt. In this costume I was suddenly attacked by a swarm of great red ants. They had dropped down from the branches overhead, and were all over me before I noticed them, biting ferociously. At first I thought a snake had bitten me. As soon as I got clear of the focussing cloth and saw the ants, I had nothing for it but to run indoors and plunge into a bath of cold water. After this I carefully avoided bamboo trees. These red ants make nests by gumming the leaves together, in the same way as caterpillars in this country.

The scenery in Singapore is more suitable for the stereoscope than for large pictures. Knowing nothing of the language, and having to do everything myself, small plates were quite as much as I could manage. At first the tremendous heat inside the tent made me feel very sick and giddy, but it soon ceased to be an inconvenience.

Though I used a glass bath with glass top, and silver wire dipper, a kind of scum always formed after a day's work. This had to be cleaned off, and the solution filtered before use. Once, after taking three successful instantaneous negatives, the bath solution became suddenly turbid, and the surface covered with a thick black scum. After filtration it worked as well as ever. With this exception, which I can in no way account for, the bath was never out of order.

The tent was always pitched in a shady spot, if possible, the ground well watered, and the inside of the tent rubbed with a wet sponge to prevent dust. For a few minutes the atmosphere was bearable enough, but after a couple of negatives had been developed it was quite another matter. The stopper of the collodion bottle used to blow out, and the smell of ether, mixed with cyanide, in such a temperature, was anything but agreeable.

After a little experience, however, I found less difficulty in taking good negatives than in this country. Mawson's collodion worked splendidly. The developer I first tried was eight grains of iron and twenty drops of acetic acid to the ounce of water. This did not answer, so I tried fifteen grains of iron and fifteen drops of acetic acid; and, finally, thirty grains of iron, thirty drops of acid, thirty drops of alcohol, and two drops of ammonia to one ounce of water. Nothing could be better than the last formula. I used it afterwards in India both for large and small plates.

Going up from Hong Kong to Shanghae, most unfortunately my bath was left behind. There were plenty of interesting objects near Shanghae, and some very fine bridges over the canal at a town called Wong-dow, which had been burnt by the Taepings. Here I saw a man catching fish with cormorants, and would have given anything for my camera. There was no help for it, so I shot pheasants instead.

The "Alabama" was coaling in the New Harbour, Singapore, when I arrived there on my way to Calcutta. Of course my only thought was how to take a picture of her. All my apparatus had been laid up for a month, and it was raining hard. At night when I was going to bed a report got about that she was to sail early next morning. I had to come on board, and, spite of heat and swarms of the most bloodthirsty mosquitoes, to clean plates and filter bath solution. At four o'clock in the morning everything was ready; but, meanwhile, one of Captain Sherrard Osborne's fleet had come in, and totally spoilt the point of view I had chosen. At daylight I crossed the straits in a boat, and set up the tent under a bank, hoping to avoid the direct rays of the sun. I was slightly mistaken. Before long the sun shone directly on the tent, and the woodwork inside soon became unpleasantly hot to the touch. I got two pretty good views of the "Alabama" at anchor, and another in which she appeared steaming out of the harbour.

The steamer left Singapore on December 24th, and the same evening a ship was reported on fire. At nine o'clock we were close to the burning vessel, which proved to be the "Martaban," set on fire by the "Alabama." The water being perfectly smooth, the effect was magnificent.

On the 26th I arrived at Penang early in the morning. Nothing can exceed the beauty of this island. The heat on the low grounds near the sea is very great; but on the hills, which are often hidden by great fleecy clouds, the atmosphere is deliciously cool and healthy. The steamer only stopped a few hours, but I secured some beautiful negatives. A crowd of Chinese and Malays favoured me with their company. The water running out of the waste pipe caused great astonishment. I was told they called my tent "ruma Setan" (the "Devil's house"), which name is also applied to the Freemason's lodge at Singapore.

On New Year's Eve we anchored at Diamond harbour, on the Hooghly, and landed at Calcutta next morning. As soon as possible I got my baggage on shore and prepared a silver bath of thirty grains of silver to the ounce of water. Finding the light far quicker than at Singapore, I thought it well to make the solution tolerably acid, and added one drachm of acetic acid to ninety ounces of bath solution. With this acid bath I took a great many instantaneous stereoscopic views, and several negatives 11 × 9 with the triplet, in less than a second.

Within a mile or two of Calcutta there was at that time a most beautiful jungle of palms, cocoa-nuts, bamboos, &c.—a perfect paradise for photography. All this, or the greater part of it, together with the splendid trees in the Botanical Gardens, have since been destroyed by the great cyclone.

Here I used Thomas's collodion, newly iodised with magnesium, with great success. After being iodised a few days, however, the film invariably split at the lower edge of the plate when immersed in the nitrate bath. The crack was about two inches long and one-eighth broad, extending upwards. I should be glad if any one could explain how this was caused. After this I used Mawson's collodion almost entirely. Once I mixed the two together, and found them work well.

The chief drawback to wet photography in India is the enormous weight of apparatus required. It was bad enough by railway, but when I had to transport three heavy boxes by horse *dâk*, as the miserable public carriages are called, it became simply ruinous.

My visit to the ruins of Gaur, in the Maldah district, some 200 miles above Calcutta, gave me more pleasure than anything I saw in India. From Calcutta to Rajmahal there is a railway. There I put my baggage into bullock carts to cross the Ganges, and proceed to Gaur, a distance of thirty or forty miles. I myself rode to the ferry, and was delighted to see a couple of elephants waiting on the opposite bank to convey me to Mootrapore, the residence of an indigo planter—the most hospitable man in the world, and the slayer of at least 200 tigers. Next day I went on with the elephants, and the day following arrived at Gaur, found a tent pitched, and the carts unloaded.

Gaur was the ancient capital of Bengal, and the ruins of the city, surrounded by dense jungle, are magnificent. My first act was to drink a bottle of beer, the second to light a cheroot, and the third to filter the everlasting bath, and clean half-a-dozen plates. Having performed all these interesting operations satisfactorily I went to sleep, and started early next morning for the ruins—my servant on one elephant, with tent, camera, and field box, while the bath accompanied me on the other elephant. I may as well tell you that I never trusted the bath out of sight longer than I could help, knowing that if any accident occurred there was no chance of getting another between Calcutta and Agra.

The first subject I tried was a fine old minaret, standing by itself, near a large tank swarming with alligators. The attempt was a total failure—nothing but fog, I could not account for it then, nor can I now. Out of fifty-six large plates, however, exposed in India, this was the only failure.

I made matters worse directly afterwards, by stamping on the focussing-screen: luckily half the glass escaped being broken. I would advise any photographer going to out-of-the-way places always to carry a spare focussing-screen. A collodionised plate will answer the purpose at a pinch, but with a badly-lighted subject it is difficult to focus accurately without ground glass.

The next plate was all right, so I moved on to a grove of date palms, every tree of which was full of monkeys, and it was plain that unless they were driven away I might as well attempt to photograph the palm trees in a gale of wind. At last they all took their departure—or at least I thought so. A dark gateway at the end of the grove became beautifully lighted, and not a leaf stirred. I lit a cheroot, placed the dark slide in position, and uncovered the lens, intending to give an exposure of one minute and a-half. Twenty seconds were wanting to complete the time, and already I saw a perfect negative developed, when suddenly an old grey monkey dropped from the top of a tree, and swung on a bough which, with infinite pains, I had brought into focus by an unlimited use of the swing-back. Of course I covered the lens immediately; but the mischief was done. The father of monkeys grinned and made horrible faces, then dropped to the ground and disappeared. Presently afterwards, while looking about for another view, a whole tribe of monkeys came and sat close to the camera, huddled together like a swarm of bees. The light coming through the branches of trees was very feeble, and having only a long-focus lens, it was useless to attempt taking their portraits.

The last day I spent in Gaur was one of downright hard work. The ruins were a long distance apart, and the tent had to be pitched and taken down five times. As it was getting dusk we passed through a magnificent gateway in the old wall. I was quite tired out and half asleep; but the mahout, or elephant driver, called my attention to it, whereupon I woke up and told my servant to pitch the tent for the fifth time. While he was doing so I looked out for the best point of view. There was no difficulty in finding it. The difficulty consisted in the clouds of dust raised by a long line of pilgrims incessantly passing. A troop of monkeys chattered in the branches overhead. For the monkeys there was no remedy; for the pilgrims there was. Coming out of the tent I was surprised to find a perfectly clear atmosphere. At a little distance the elephants were standing right across the narrow road, and behind them at least 200 pilgrims, partly held in awe by the elephants and partly by a *berkendars*, which, I think, means a "fighting man," who had accompanied me.

After leaving Gaur I went by train to Benares. Stayed three days, and took a few negatives. There are some very very fine mosques in Benares, but so built round as to prevent photographing with ordinary lenses. A globe lens would be of immense service in such places, and it is surprising that they are not in more general use.

The railway is now open from Calcutta to Delhi; but at the beginning of last year there were two breaks in the journey from Benares to Allahabad, and from Agra to Delhi.

I was obliged to travel by *dâk* from Benares to Allahabad, thence by rail to Cawnpore, and again by *dâk* to Lucknow. At the latter place the weather was miserably cold, and the wind too high for photography. The Kaiser Bagh reminded me of the Palais Royal at Paris. It would make an excellent picture in a pantoscopic camera.

I was much disappointed at not being able to do more at Lucknow: once I did set up my camera near the Presidency, but a sudden squall of wind upset it, diminishing the size of my unfortunate small focussing screen.

Leaving Lucknow at midnight, I had time to photograph the memorial over the well at Cawnpore: it stands at the end of a large garden. No natives are allowed to enter unless by special permission; and Europeans driving through the garden do so at foot's pace.

No words can describe the magnificence of the Taj Mehal, at Agra—a mausoleum of white marble, built by the Sultan Akbar over the tomb of his wife. The marble is inlaid in the most exquisite manner with precious stones, in the form of leaves and flowers. In front of the building is an avenue of dark cypress trees, and a long line of fountains, at the end of which stands the Taj flashing like the sun itself. It would be hard to conceive a more difficult subject for a photograph; yet by using a weak developer, the black cypress trees, the shining white marble, and the fleecy clouds were all perfectly rendered.

From Agra to Delhi is one hundred and thirty miles by *dâk*—a most wearisome journey through sandy plains, dotted here and there with low jungle. The Delhi water is full of salt; at least I found it so, though a professional photographer living on the spot told me "it was good enough in his hands, and answered perfectly." I acquiesced humbly, but took a pint of distilled water he was kind enough to give me.

The splendid monument called the Kūtub, on the site of ancient Delhi, is about eleven miles from the present city. The height of the pillar is even now, I believe, 340 feet, and is supposed formerly to have reached 400 feet. The water was quite fit to use, so I brought back several bottles full, intending to take views of the Cashmere gate, &c.; but a letter from the great tiger-slayer, before mentioned, warned me to return at once if I intended to join his tiger party on the 9th of March.

Photography is great, but tiger hunting is greater; so I left Delhi one Monday morning, and, travelling incessantly, reached Calcutta on the Saturday. Here I left all my apparatus, except a plate or two for the benefit of a dead tiger. This I accomplished, but after seven or eight hours' shooting on the top of an elephant even photography becomes a burden.

Having been in at the death of seven tigers, I left Calcutta the first week in April, and I, with all my negatives—namely, fifty-five 11 × 9 inches, and six dozen stereoscopic—arrived in England towards the end of May.

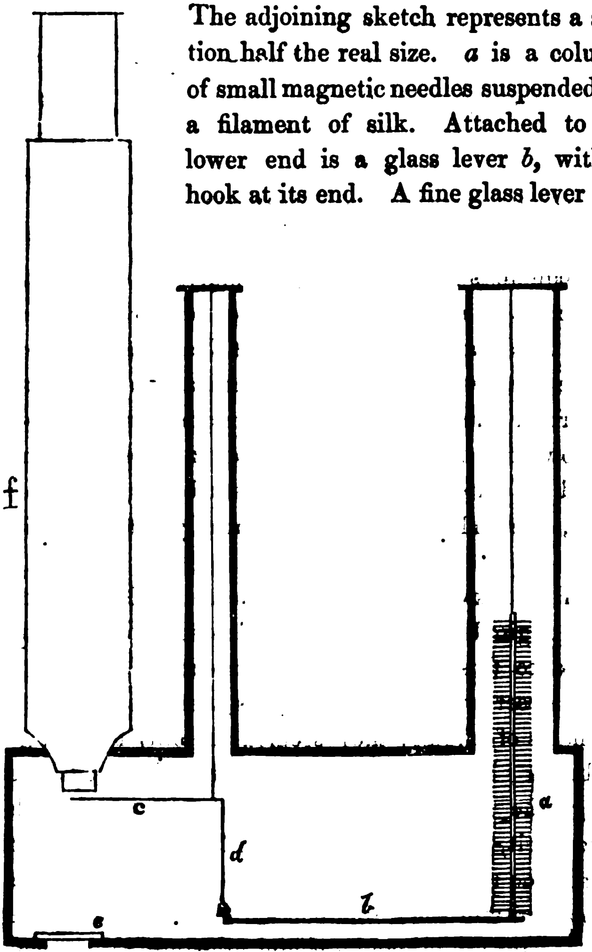
I cannot conceive anything more delightful than a photographic tour in India—arriving out at the end of October, and returning in April. If a man's health be good there is no difficulty in obtaining good pictures. In fact, one great source of failure in this country—dirty plates—scarcely exist in India. If a plate be washed with clean water, and rubbed dry, it is as clean as man can make it. According to my experience collodion cannot be too much shaken before use. I think it a great mistake to use a bottle which has been standing quiet for any length of time; but I know plenty of learned photographers who hold the very opposite opinion.

In conclusion, I venture to hope that photographers intending to pursue the art in India or other hot countries may find the foregoing remarks of service in the selection of apparatus and chemicals.

## Ordinary Meeting, March 21st, 1865.

R. Angus Smith, Ph.D., F.R.S., President,
in the Chair.

Dr. Joule described an instrument he had constructed for showing rapidly minute changes of magnetic declination. The adjoining sketch represents a section half the real size. *a* is a column of small magnetic needles suspended by a filament of silk. Attached to its lower end is a glass lever *b*, with a hook at its end. A fine glass lever *c* is



suspended by a single filament of silk; its shorter arm being connected with the first lever by means of a small hook attached to the fibre *d*. The whole is enclosed in a stout copper box, into which light is admitted through a lens *e*, cemented into an orifice immediately under the object glass of the microscope *f*.

The microscope magnifies about 300 linear, and has a micrometer in its eyepiece, with divisions corresponding to $\frac{1}{1000}$ of an inch. One division corresponds to a deflection of the needle of $4\frac{1}{2}''$, and as a tenth of a division can be very readily observed, the instrument measures deflections to within half a second. So rapid is the action, that on applying a small magnetic force the index takes up its new position steadily in two seconds of time. Besides being a damper to the motion of the needle, the copper box, by its conducting power, equalizes the temperature rapidly, so that the indications are not to any considerable extent disturbed by currents of air. The success of the present instrument encourages the hope that very much greater delicacy may be obtained by a further multiplication of the motion and the use of a more powerful microscope. Dr. JOULE stated that he had observed an extensive magnetic disturbance the preceding evening, the index being driven entirely out of the field of view.

The PRESIDENT said that three meteorological instruments of true originality and of unprecedented delicacy had been described to this Society by the inventor Dr. Joule. For common observation the instruments were too refined, but in some fields of enquiry they seemed the only hopeful guides. Manchester has not yet a Meteorological Observatory, although the proposal has often been made to establish one. Private spirit, as in the instance of Mr. Vernon and others, has made the necessity less felt than before. But there is now an opportunity of beginning one with entirely new apparatus of Manchester origin, which would probably very much alter the quality of the enquiries made in meteorological establishments.

Mr. **Baxendell** stated in reference to Dr. Joule's observation, that on the same evening he had observed a very fine auroral arch, which at 8h. 20m., G. M. T., passed over ε Bootis, *α* Ursæ majoris, Capella, and a point midway between the Pleiades and ε Tauri. There was at the same time a considerable segment of bright auroral light in the northern part of the heavens. No streamers were visible. About twenty minutes later it was noticed that the position of the arch had changed, the western portion having moved very sensibly in a southerly and the eastern portion in a northerly direction. This change of position was probably connected with the remarkable disturbance of the magnetic needle noticed by. Dr. Joule. The western portion of the arch was brighter than the eastern, and was estimated to be from ten to twelve times brighter than the milky way in Cepheus and Cassiopeia.

Mr. **Baxendell** also stated that the Society had in its possession a thermometer constructed by the late Dr. Dalton, and which, it is believed, was used by him in many of his meteorological observations. The scale has the initials J.D. and the year 1823 engraved upon it; and the freezing and boiling points of water are indicated on the stem by fine file marks. As it is known that the zero points of thermometers sometimes change to the extent of *one* or even *two* degrees in the course of several years, it occurred to Mr. **Baxendell** that it would be interesting to ascertain whether any change had taken place in this thermometer, and he had therefore lately tested very carefully the position of the freezing point, but found that no sensible alteration had taken place; and he believed therefore that great confidence might be placed in the observations which Dalton had made with this instrument.

Mr. Dyer communicated a letter from Mr. Joseph Barratt, of Southport, in which the writer endeavoured to account for storms on principles which appear to be almost identical with those advanced many years ago by the late Professor Espy and Mr. Thomas Hopkins.

A Paper was read entitled "Further Observations on the Permian and Triassic Strata of Lancashire," by E. W. Binney, F.R.S., &c.

In previous memoirs published in the Transactions of the Society, the author had given what information he possessed in a fragmentary state, just as he obtained it, of the permian strata of Lancashire and the north-western counties of Westmoreland, Cumberland, and Dumfries, as well as the north-western corner of Yorkshire, and he took sections where he was fortunate enough to obtain them, but he made no attempt to lay the strata down continuously on a map, his materials being far from sufficient for such a purpose.

By looking at a map of the county of Lancaster, the observer will find a great gap between the permian beds of Grimshaw Delph, Bradley Brook, and Skillaw Clough, to the north-west of Wigan, and the sections described by the author at Rougham Point near Cartmel, and Stank near Ulverston. The lower coal measures from Harrock Hill can be traced pretty well towards Chorley, and thence to near Withnell, and then the millstone grit runs to Hoghton, and across the country not very well seen to Griesdale, Scorton, Cleveley, Ellel, Ashton, near Abbey Lighthouse on the Lune, over the mouth of that river to Robshaw Point, and on to Heysham. The country forming the western boundary of the above line is a low district, a good deal covered up with drift, and affording few natural sections to show clearly the relation of the carboniferous to the permian strata. The district probably may afford some sections if carefully investigated, but up to this time it has been quietly dismissed by colouring it red for trias.

In this communication the author gave a little more information which he had lately obtained in a line from Hoghton Tower to Fleetwood; also at Cockersand Abbey, south of the mouth of the Lune, and Robshaw Point and Heysham to the north of the same river; and made a few remarks on some singular red sandstones hitherto classed as trias in the neighbourhood of Whiston, Huyton, Knowsley, Croxteth Park, and Rainford, and laid down as such on the maps of the Geological Survey.

No doubt it is a very difficult matter to determine with absolute certainty where the trias strata end and the permian begin, when there are no organic remains to guide us and we have to trust to a bed of red marl or a deposit of red sandstone. In the author's several memoirs published on this subject, so far as South Lancashire was concerned, the red marls and limestones of Newtown and Bedford are assumed to be the uppermost permian deposits found. It is quite true, as stated in his third memoir—"Some of the sections near Manchester, especially that seen in the valley of the Irk in Cheetham and Newtown, would apparently show that the red marls containing limestones and fossils of the genera *Bakevellia*, *Schizodus*, &c., passed into the overlying trias;" but as a whole it was assumed from other facts that the red sandstone of the trias was unconformable to the underlying permian beds. In a paper published by Sir R. I. Murchison and Professor Harkness, printed in the *Quarterly Journal of the Geological Society* for May, 1864, as well as in the author's last memoir, the thick red sandstones of St. Bees are described as permian and not as trias, and were traced down, as Professor Sedgwick had previously followed them, into Furness near Howcote and Barrow. Anyone who sees the red sandstones much used for building purposes at Shawk, Maryport, and St. Bees, and compares them with that at Howcote, will not be able to distinguish the one from the others. It is only from their physical characters that we can compare these sandstones; for up to this time, so far as his knowledge extends, no organic remains have been met with in them. Now this is bringing a permian red sandstone above the Newtown and Bedford red marls and limestones, and introduces for the first time an upper permian sandstone into Lancashire; and this rock runs into the trias so regularly that it will be very difficult to separate it by any well marked boundary from the lower soft sandstone or pebble beds of the trias as laid down and described in the maps and memoirs of the Geological Survey.

It is pretty clear if some of these permian and triassic sandstones are to be classed by their physical characters alone, that certain of the latter rocks, as laid down by the Geological Survey in the Huyton, Croxteth, and Knowsley districts, will probably have to be put into the permian, for no one can tell the red flaggy sandstone of Knowsley quarry from the Howcote and St. Bees sandstones, and it must be taken as permian just as the Howcote rock is identified with that at St. Bees.

As it is desirable to attempt to connect the permian deposits of the South and West of Lancashire with those in the north of the county, as seen at Rougham Point near Flookborough, and Stank near Furness Abbey, he gave what information he was possessed of. The only section hitherto seen near Preston, is one in the Ribble below that town, and appears to be a portion of the pebble beds of the trias. How far it extends up the valley of the Ribble and to Roach Bridge in Samlesbury, where the soft red and variegated sandstones and conglomerate (permian beds) rest unconformably on what appears to be limestone shale, has not been yet determined. Near Cockersand Abbey on the south side of the mouth of the Lune, west of the town of Lancaster, below high water mark, is a small patch of what appears to be permian sandstone.

To the north of the last named place across the Lune at

Robshaw Point, the same soft red sandstone is seen on the beach covered by the tide, and appears to be a continuation of that rock seen to the south, but much better exposed. With these exceptions no further evidence has yet been obtained of any permian beds until we reach Rougham Point.

The triassic beds in South Lancashire, as seen near Liverpool, according to Mr. Hull, are as follows* :—

| FORMATION. | DIVISION. | SUBDIVISION. |
|---|---|---|
| New Red Sandstone. | Keuper. | 1. Red marl, with beds of upper keuper sandstone. |
| | | 2. Lower keuper sandstone or waterstone, with a base of breccia or conglomerate. |
| | Bunter. | 1. Upper red and mottled sandstone. |
| | | 2. Pebble beds. |
| | | 3. Lower red and mottled sandstone. |

Next, as seen near Manchester, where the same author classes the bunter as composed of

1. Upper red and mottled sandstone.
2. Pebble beds.

It will be seen from the above divisions that the lower soft red and mottled sandstone of Liverpool is left out at Manchester altogether, the lowest member of the trias there being the pebble bed. There certainly is the Vauxhall sandstone, which would pass very well for the lower soft red; but the Newtown fossils found above it, clearly cut off that rock from the trias, and establish it with the permian beyond all question.

The author had not made many divisions of the bunter portions of the trias. No doubt they are useful in different places, and have sometimes to be varied with the districts to which they are applied. In the north, about Carlisle, up to this time, only one bed of soft red sandstone without pebbles has come under his notice. But at Sutton, as previously alluded to, there is a soft red sandstone without pebbles

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[*] Manchester Geological Society's Transactions, Vol. II. p. 23.

resting on permian red marls. Similar sandstones, in the same position, are seen near the canal at Bedford; below Messrs. Hampson and Co.'s Printworks, at Clayton Bridge, Manchester; and near Messrs. Brocklehurst's Lime Works, at Ardwick, near Manchester. There is also a soft red sandstone, apparently dipping, under the pebble beds of Heaton Mersey, near Stockport, well seen on the banks of the Mersey from Stockport to Fogg Brook, which would pass for the lower soft sandstone of the trias; but for some reason with which the author is unacquainted, the gentlemen connected with the survey prefer (he is informed) to class this sandstone underlying the pebble beds with the permian rather than the trias.

It appears that through the western part of Cheshire and the adjoining county of Flint, as well as in West Lancashire, where there are few, if any, permian strata exposed, the Geological Survey has always had a lower soft red and mottled sandstone; but when the east part of Lancashire is reached and undoubted permian beds are found, this supposed lowest member of the trias disappears.

The soft yellow and variegated sandstones of Whiston, Croxteth Park, and Rainford, all resting unconformably on coal measures, described by Mr. Hull, are evidently of the same age as the Rainford and Grimshaw delph beds. Unfortunately in no instance have any red marls been yet found lying either above or below them. He had described the latter as permian, whilst Mr. Hull thinks they are the lower red and mottled sandstone of the trias. But with respect to the Knowsley quarry, it so much resembles the St. Bees and Howcote upper permian sandstones, that if they were found in Furness, Sir R. J. Murchison and Professor Harkness would, without doubt, claim them as permian.

It is many years since the author first saw the Knowsley quarry, and he then in his note book remarked that those sandstones, especially that belonging to Mr. Littler, could not be distinguished from the upper permian sandstones of the neighbourhood of Dumfries, which he had just returned from examining. Now, if they can be proved to overlie immediately the coarse grained, false bedded, soft red and mottled sandstones of Whiston and Croxteth Park, both the latter as well as the former, will have to be classed as permian rocks according to the present Geological nomenclature of the North West of England; and he thinks that the new sections he now describes at Roach Bridge, Cockersand Abbey, and Robshaw Point, tend to confirm this view.

In all the quarries of Lancashire where the trias sandstones have been wrought, the author has never seen so hard and thin bedded a stone as that found at Knowsley. It was formerly used for paving sets in Liverpool, and large quantities of it were broken for road metal purposes, for which he never knew a trias rock used. Some of its beds also afforded fined grained flags, with faces as smooth as any permian sandstone he had even seen in the neighbourhood of Dumfries. A good example of pebble beds is seen at Kirkby Rough; but this rock bears no resemblance to the stone at Knowsley quarry, and the two stones cannot well be classed as the same from their characters.

## MICROSCOPICAL SECTION.

February 20th, 1865.

Dr. W. C. Williamson, F.R.S., in the Chair.

Dr. **Alcock** showed mounted specimens of the Carapaces of Entomostraca, picked from shore sand from the coast of Galway. They included Cythere albo-maculata, angustata, variabilis, flavida, convexa, impressa, pellucida, and quadridentata; besides about thirty other species of Cythere and Cythereis which are not described in Dr. Baird's Monograph.

Professor **Williamson** said that the further we extend our observations of the lower forms of animal life, the greater become our difficulties in determining specific distinctions; and in the present case it must be remembered that we have only the shell or carapace for examination, and this outer skin is of less value for distinctive characters than the internal parts. Then again it is known that some of the entomostraca undergo several metamorphoses similar to those passed through by the higher crustacea before they become adult, so that he should not be at all surprised if some ten or twenty of these different forms turned out to belong to one and the same species. There still remained a third difficulty, the greatest of all, namely, the doubtfulness of what a species is; and in the lower forms of life the variability is so great that he doubted if specific distinctions could be made with any certainty. The same remark is applicable to the lowest forms of the higher divisions of animals, and the entomostraca standing low in the articulate class are probably subject to similar variability.

Specimens of Hydra with Paramœcium aurelia and Trichodina pediculus parasitic upon them were shown by Mr. A. Brothers.

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February 27th, 1865.

J. Sidebotham, Esq., President of the Section, in the Chair.

### *Exhibitions.*

The shell of an oyster with a thick layer of soft chalk-like deposit lining its inner surface.—Mr. Nevill.

A ball composed apparently of fragments of some vegetable fibre felted, from the shore of the Mediterranean at Cannes.—Mr. Sidebotham.

Flowers of a species of Euphorbia, and a leaf of Cycas.—Mr. Grindon.

Mounted specimens of the larval shells of Saxicava rugosa, Arca tetragona, and Ostrea edulis; also of Lima, Pecten, Cardium, and others not yet specifically distinguished, from Connemara shore sand.—Thomas Alcock, M.D.

A series of shells of Helix nemoralis, Helix hortensis, and Helix hybrida were shown by Mr. Sidebotham, who stated his belief that the two former are good species, and the third a true hybrid.

### *Communication.*

Mr. G. E. Hunt read the following "Notes on Mosses."

Campylopus setifolius, Wils.—This species was described by Wilson in his Bryologia Britannica, from specimens collected by the late Dr. Taylor on Carrig Mountain, Ireland. Since then it has been observed by Dr. Moore, of Dublin, in County Wicklow and on Cromaglaun; by myself, in great abundance at Cromaglaun and Gap of Dunloe, Killarney. In these stations it is the female plant that we find. In

August, 1863, however, I met the male plant on the moors of the Isle of Skye, this being the only recorded occurrence of the male plant, and the only occasion of the species being found out of Ireland. It is at once distinguished from every other Campylopus by the large auricles of the base of the leaf, which are composed of perfectly colourless, diaphanous cells, and by the large red quadrate cells above this base.

In Dicranodontium longirostre, which presents some characters similar to this species, the large quadrate cells above the base are green. Specimens were exhibited of both the above species; also Scotch ones of Dicranodontium aristatum.

At Southport, in November last, I observed a new species of Brachythecium, intermediate between campestre and rutabulum, differing from the former in its less plicate leaves and very rough setæ, and from the latter in its slightly plicate leaves, lanceolate, gradually tapering from a wide base to a very acute point, not at all accuminate, shining; inflorescence, as in these species, monoicous. If a variety, it must be united with Brachythecium campestre, which has not yet been certainly identified in Britain. Specimens were exhibited.

## PHYSICAL AND MATHEMATICAL SECTION.

### Annual Meeting, March 16th, 1865.

ROBERT WORTHINGTON, F.R.A.S., Vice-President of the Section, in the Chair.

The following gentlemen were elected Officers of the Section for the ensuing year:—

**President.**
E. W. BINNEY, F.R.S., F.G.S.

**Vice-Presidents.**
ROBERT WORTHINGTON, F.R.A.S.
JOSEPH BAXENDELL, F.R.A.S.

**Treasurer.**
THOMAS CARRICK.

**Secretary.**
PROFESSOR R. B. CLIFTON, M.A., F.R.A.S.

Mr. BAXENDELL communicated a series of Raingauge and Anemometer Observations made at St. Martin's Parsonage, Castleton Moor, during the year 1864, by the Rev. J. Chadwick Bates, M.A., F.R.A.S., F.G.S.

The geographical position of St. Martin's Parsonage is lat. 53° 35′ 20″ N.; long. 2° 10′ 31″ W.; and the site of the raingauges is 475 feet above mean sea level. The gauges employed comprise three of five inches diameter, constructed on Howard's principle, and made by Casella, of 23, Hatton Garden; and three of eight inches diameter, on Mr. Glaisher's plan, made by Negretti and Zambra. One gauge of each set is placed at an elevation of one foot, another at five feet, and the third at twenty feet above the ground; and they have all been leveled and tested by Mr. Symons.

The anemometer is by Casella, and is a Robinson's, as improved by Col. Sir Henry James.

To ensure correctness as far as possible, every instrument has been carefully tested both before being placed and also afterwards; and they are all read by Mr. Bates every morning at nine o'clock, and in his absence by his schoolmaster; and that their readings may be as much as possible alike, they often read them together for the purpose of comparison.

The monthly amounts of rain received by the different gauges and the movement of the wind are shown in the following table:—

| | 5-INCH GAUGES. | | | 8-INCH GAUGES. | | | Total Movement of Wind. |
|---|---|---|---|---|---|---|---|
| | 20 feet elevation. | 5 feet elevation. | 1 foot elevation. | 20 feet elevation. | 5 feet elevation. | 1 foot elevation. | |
| January | 2·087 | 2·219 | 2·350 | 2·060 | 2·244 | 2·320 | 6422 |
| February | 4·135 | 4·248 | 4·312 | 4·385 | 4·607 | 4·564 | 6663 |
| March | 1·999 | 2·108 | 2·268 | 1·915 | 2·210 | 2·273 | 9016 |
| April | 1·205 | 1·285 | 1·396 | 1·119 | 1·309 | 1·406 | 5927 |
| May | 3·185 | 3·250 | 3·349 | 3·245 | 3·293 | 3·348 | 5997 |
| June | 3·414 | 3·566 | 3·719 | 3·529 | 3·643 | 3·725 | 6799 |
| July | 2·042 | 2·141 | 2·269 | 2·124 | 2·196 | 2·304 | 6170 |
| August | 2·205 | 2·326 | 2·504 | 2·277 | 2·378 | 2·452 | 5142 |
| September | 3·624 | 3·803 | 4·104 | 3·776 | 3·923 | 4·091 | 6230 |
| October | 2·615 | 2·738 | 2·849 | 2·690 | 2·704 | 2·785 | 7556 |
| November | 3·548 | 3·776 | 4·077 | 3·699 | 3·947 | 4·068 | 6302 |
| December | 2·287 | 2·456 | 2·672 | 2·344 | 2·543 | 2·718 | 6089 |
| | 32·346 | 33·916 | 35·869 | 33·163 | 34·997 | 36·054 | 78313 |

From these results it appears that in every month of the year, and with both sets of gauges, the highest gauge received the least amount of rain; and that the 8 inch Glaisher gauges received slightly greater amounts than the corresponding 5 inch Howard's. The movement of the wind was greatest in March and least in August. No relation appears to exist between the monthly amounts of rainfall and the monthly movement of the wind.

### NOTE BY MR. BAXENDELL.

The differences between the amounts of rain received by gauges placed at different elevations at the same station, are attributed by some meteorologists to the influence of the wind. In order to test the soundness of this view, Mr. Bates's observations were arranged in three groups; the first group, comprising all the rainy days on which the movement of the wind did not exceed 200 miles; the second, those on which it was greater than 200 miles, but did not exceed 300; and the third, those on which it exceeded 300 miles per day. The number of days in each group, the mean daily movement of the wind, and the amounts of rain received by the different gauges were as follows:—

| Group. | Number of Days in Group. | Mean Daily Movement of Wind. | 5 Inch Gauges. | | | 8 Inch Gauges. | | |
|---|---|---|---|---|---|---|---|---|
| | | | Elevation. | | | Elevation. | | |
| | | | 20ft. | 5ft. | 1ft. | 20ft. | 5ft. | 1ft. |
| | | | In. | In. | In. | In. | In. | In. |
| 1 | 61 | 133 | 8·329 | 8·635 | 9·071 | 8·517 | 8·786 | 9·007 |
| 2 | 59 | 249 | 12·713 | 13·388 | 14·171 | 13·117 | 13·875 | 14·390 |
| 3 | 55 | 385 | 11·304 | 11·893 | 12·629 | 11·526 | 12·342 | 12·660 |

Representing by unity the quantity of rain received by the lowest gauge in each set, we have the ratios given in the following table:—

| Group. | Mean Daily Movement of Wind. | 5 Inch Gauges. | | | 8 Inch Gauges. | | |
|---|---|---|---|---|---|---|---|
| | | Ratios of quantities of Rain at Elevations of | | | Ratios of quantities of Rain at Elevations of | | |
| | | 20ft. | 5ft. | 1ft. | 20ft. | 5ft. | 1ft. |
| | | In. | In. | In. | In. | In. | In. |
| 1 | 133 | 0·918 | 0·951 | 1·000 | 0·945 | 0·975 | 1·000 |
| 2 | 249 | 0·897 | 9·944 | 1·000 | 0·911 | 0·964 | 1·000 |
| 3 | 385 | 0·895 | 0·941 | 1·000 | 0·910 | 0·974 | 1·000 |

Comparing the results for group 1 with those for group 2, it would appear that an increase in the velocity of the wind from 133 to 249 miles per day, has a very sensible effect in diminishing the ratios for the higher gauges; but on comparing groups 2 and 3, it appears that the effect of a still further increase in the velocity of the wind from 249 to 385 miles per day, is hardly appreciable. The differences between the ratios in groups 1 and 2 must therefore be due, to a great extent, to some other cause than the influence of the wind. Determining for each group the mean daily rainfall, we have

| Group. | | | 5 in. Gauge at 1 foot elevation. In. | 8 in. Gauge at 1 foot elevation. In. |
|---|---|---|---|---|
| 1 | Mean daily Rainfall | = | 0·148 | 0·147 |
| 2 | | = | 0·240 | 0·243 |
| 3 | | = | 0·229 | 0·230 |

The mean daily rainfall is therefore much less in group 1 than in either of the other groups, thus indicating a less abundance of rain-forming moisture in the atmosphere, owing to which the rain drops in falling could not increase in size so rapidly as under the more favourable conditions which existed on the days included in groups 2 and 3.

Separating the rainy days from those on which no rain fell, we have for each month the following results:—

| | No. of Days of Rain. | Total Movement of the Wind. | Mean Daily Movement of Wind on Days of Rain. | No. of Fair Days. | Total Movement of the Wind. | Mean Daily Movement of Wind on Fair Days. |
|---|---|---|---|---|---|---|
| January | 13 | 3234 | 249 | 18 | 3188 | 177 |
| February | 12 | 3583 | 298 | 17 | 3080 | 181 |
| March | 17 | 6039 | 355 | 14 | 2977 | 213 |
| April | 11 | 1900 | 172 | 19 | 4027 | 212 |
| May | 12 | 2158 | 179 | 19 | 3844 | 202 |
| June | 19 | 4669 | 245 | 11 | 2130 | 193 |
| July | 10 | 2451 | 245 | 21 | 3719 | 177 |
| August | 16 | 3153 | 197 | 15 | 1989 | 132 |
| September | 22 | 5184 | 235 | 8 | 1046 | 131 |
| October | 12 | 3911 | 326 | 19 | 3615 | 192 |
| November | 17 | 4532 | 266 | 13 | 1770 | 136 |
| December | 14 | 3224 | 230 | 17 | 2865 | 168 |
| | 175 | 44033 | 251·6 | 191 | 34280 | 179·4 |

From this table we see that in every month, except April and May, the mean daily movement of the wind was greater on rainy than on fair days; and that the mean daily movement on rainy days for the entire year was 251·6 miles, and on days on which no rain fell only 179·4 miles.

The mean results for the meteorological quarters, and their ratios, are—

| | Mean Daily Movement of the Wind on Rainy Days. | Mean Daily Movement of the Wind on Fair Days. | Ratios. |
|---|---|---|---|
| Winter | 257 | 175 | 0·68 |
| Spring | 252 | 208 | 0·82 |
| Summer | 228 | 166 | 0·72 |
| Autumn | 267 | 161 | 0·60 |

From the numbers in the last column it appears that the mean velocity of the wind on days when no rain fell, as compared with that on rainy days, was greatest in spring and least in autumn; and that the relative velocities in winter and summer were very nearly equal. It will be interesting to ascertain whether this remarkable relation holds good through a series of years.

Arranging the rainy days in four groups, the first containing the days on which the rainfall did not exceed 0·2 inch; the second those on which it was above 0·2 inch, but did not exceed 0·4 inch; the third those on which it was above 0·4 inch, but did not exceed 0·6 inch; and the fourth those on which it exceeded 0·6 inch; and determining for each group the mean daily movement of the wind, we have the following results:—

| Group. | No. of Days in the Group. | Daily Rainfall. in. in. | Mean Daily Movement of the Wind. |
|---|---|---|---|
| 1 | 108 | 0·0 to 0·2 | 236 |
| 2 | 40 | 0·2 — 0·4 | 270 |
| 3 | 17 | 0·4 — 0·6 | 300 |
| 4 | 10 | above 0·6 | 263 |

It appears, therefore, that the maximum mean daily movement of the wind occurs when the daily rainfall is about half an inch, and that during excessive falls of rain the velocity of the wind is very sensibly diminished.

Mr. G. V. VERNON, F.R.A.S., communicated the following "Note on the Rainfall of the last Twenty-nine Years at Royton, Oldham," by John Heap, Esq.

Seeing in the proceedings of the Manchester Literary and

Philosophical Society a few days since, a statement of rainfall in the past year and comparisons with the average fall for 71 years, I was led to examine the mean monthly values from my observations during the last 29 years, in order to ascertain how far they would bear out the theory of a maximum value in October and minimum in April, and find they agree with the theory in every particular, even with the exception in September. And moreover I find that the sum of the monthly mean values of any two months, six months apart, as January and July, February and August, &c., is very nearly the same value, being about one-sixth of the yearly average.

Below are the results, and also Mr. VERNON's mean values:—

| 1864. | Days. | Fall in Inches. | Average of 29 Years. | Difference from 29 Years Average. |
|---|---|---|---|---|
| January......... | 11 | 1·479 | 2·369 | −0·890 |
| February ...... | 12 | 3·585 | 2·201 | +1·384 |
| March ......... | 17 | 2·275 | 2·133 | +0·142 |
| April ........... | 10 | 1·260 | 2·093 | −0·833 |
| May ........... | 11 | 2·833 | 2·388 | +0·445 |
| June ........... | 18 | 3·450 | 3·117 | +0·333 |
| July ........... | 9 | 2·035 | 3·294 | −1·259 |
| August ......... | 13 | 2·832 | 3·595 | −0·763 |
| September...... | 21 | 4·130 | 3·071 | +1·059 |
| October ......... | 13 | 2·800 | 3·680 | −0·880 |
| November ...... | 18 | 3·885 | 3·026 | +0·859 |
| December ...... | 17 | 2·860 | 2·656 | +0·204 |
| | 170 | 33·424 | 33·623 | −0·199 |

| Monthly means of 1864. | Ins. | Monthly means of, for 71 years. | Ins. |
|---|---|---|---|
| January and July... | 5·663* | January and July ... | 6·029 |
| February „ August | 5·796 | February „ August | 5·971 |
| March „ Sep. ... | 5·204 | March „ Sep. .. | 5·545 |
| April „ Oct. ... | 5·773 | April „ Oct. ... | 5·856 |
| May „ Nov.... | 5·414 | May „ Nov.... | 5·824 |
| June „ Dec.... | 5·773 | June „ Dec. ... | 6·173 |
| | 33·623 | | 35·398 |

## Ordinary Meeting, April 4th, 1865.

R. ANGUS SMITH, Ph.D., F.R.S., President, in the Chair.

Messrs. H. M. Ormerod and W. Brockbank were appointed Auditors of the Society's Accounts for the present Session.

A communication was read entitled "An Instance of the Injurious Action of Alkalies on Cotton Fibre," by Messrs. HEINRICH CARO and WILLIAM DANCER.

A remarkable instance of the deleterious action of alkali on cotton fibre has lately come under our notice, when examining some indigo prints, which had been stiffened or finished with silicate of soda, and kept in bales during about two years. The strength of the fibre of the greater part of these goods had decreased to about one-third of the strength of some pieces which had been packed in the same bales, and which differed in no other respect from the others except in their having been finished with starch. We therefore surmised that silicate of soda had been the primary cause of the deterioration of the goods. Further observations convinced us, however, that the injury was due to the long continued action of free or carbonated alkali upon the cotton fibre.

Some of the sound pieces (which, as before mentioned, had been finished with starch) had been packed between the silicated goods, and had abstracted soda from them which had penetrated from the places of contact into the interior of the pieces to a considerable depth. In the same ratio in which the pieces had taken up soda it was found that they had diminished in strength. On the other hand it was found that in such places of contact the silicate of soda of the silicated goods had suffered a partial decomposition, extending to the depth of four or five layers of the pieces. The silicate of soda in the middle of the pieces contained from 70 to 74 per cent of silicic acid, combined with from 30 to 26 per cent of soda; whilst the analysis of the silicate of soda contained in the contact layers showed that from one-third to two-thirds of its soda had been abstracted This loss of soda was accompanied by a change of strength of the cloth which appeared to bear some proportion to it; the layers or folds of the cloth *decreasing* in strength as they were removed from contact with the starched goods until the silicate of soda attained the same composition as that found in the most rotten parts of the piece, this generally taking place about the fourth or fifth layer or fold of the piece, as before stated.

The following table shows the changes in strength produced by this decomposition of the silicate of soda.

| | FINISHED WITH STARCH. | | FINISHED WITH SILICATE OF SODA. | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Middle, | Contact Layer. | Contact Layer. | 2nd. | 3rd. | 4th. | 5th. | Middle. |
| Strength ... | 100 | 81 | 89 | 68 | 62 | 54 | 48 | 35 |

The silicate of soda had evidently been decomposed with the formation of free alkali and an acid silicate which appears to have very little action on the cotton fibre.

In some places the decomposition had gone further, and free silicic acid had separated out in the form of a white powder upon the surface of the cloth. The same decomposition, accompanied by the same changes in the strength of the cloth, was observed upon all pieces which had been in contact with the paper used for wrapping the bales. In this instance the paper had absorbed the liberated soda, and the cloth in contact with it had almost entirely retained its original strength.

The white portions of the patterns were in a further advanced state of decay than the blue ones, in most instances retaining only 10 per cent of their original strength. In the goods finished with starch only, the whites were equally as strong as the blues. In the goods finished with silicate of soda the whites were almost as strong as the blues in all places where the before-mentioned decomposition of the silicate of soda had been accompanied by an abstraction of soda; but in the interior of the goods, where the silicate of soda had retained its original composition, the strength of the whites had decreased to about one-third of that of the blues. It was therefore evident that this excessive decay of the whites was due to some cause which had assisted the action of the alkali upon them, and we believe to have found an explanation of this in the action of the silicate of soda upon the sulphate of lead contained in them to the amount of about 10 per cent of the mineral ash.

Sulphate of lead has been an ingredient of the resist paste printed upon the places intended to remain white, and by the subsequent action of lime and sulphuric acid it has become fixed in the fibre. We have noticed that sulphate of lead decomposes solutions of silicate of soda very rapidly, with formation of sulphate of soda, free silicic acid, and silicate of lead.

These changes give rise to the production of a crystallisable and strongly efflorescent salt, and to an increase in bulk; and we think that the mechanical effect produced by the crystallisation of the sulphate of soda formed may have caused a further and final disintegration of the fibre already weakened by the action of the alkali. Under the microscope the fibre of the white portions of the pattern presented the appearance of *cylindrical* tubes, partially covered with minute crystals (soluble in water); in some places these tubes appeared to be split longitudinally.

A paper on the same subject was lately read before the Chemical Society of London by Dr. F. CRACE CALVERT, F.R.S.

A paper, entitled "Remarks on the Microscopical Appearances of Cotton Hair during Dissolution in the Ammoniacal Solution of Copper," was read by J. B. DANCER, F.R.A.S.

The structure of cotton hairs has occasionally furnished an interesting topic for conversation at the meetings of our Microscopical Section.

Two of our members, Mr. Chas. O'Neill and Mr. Heys, have given considerable time and attention to this subject. Mr. Walter Crum, F.R.S., communicated to the Chemical Society a memoir "On the Cotton Fibre," and the manner in which it unites with colouring matter. His paper is illustrated with some beautifully executed drawings of the microscopical appearances of cotton in the natural state, and when mordanted, mercerised, and treated with various dyes; this paper is well worthy the attention of those interested in this branch of inquiry. Mr. Crum has presented a copy of his memoir to this society. His description of the ordinary appearance of the cotton fibre agrees so nearly with what I believe it to be, that I will take the liberty of referring to his printed paper at page 5. To Mr. Crum's description I may add, that many specimens of cotton, especially on the cylindrical portion of the hairs, shew transverse markings. At times these appear at tolerably regular intervals, they have been claimed as evidences of spiral structure; when, however, they are examined with magnifying powers of 1,000 to 1,200 diameters they proved to be cracks in the external membrane. Other portions of cotton exhibit longitudinal furrows, irregular in length and direction—having a shrivelled appearance something like the bark of an aged tree. In gun cotton, the transverse cracks are very numerous. From an examination of transverse sections of cotton, I incline to the opinion that there is an external membrane distinct from the true cell wall or cellulose matter;* inside the cellulose there is an irregular cavity, this, in some specimens (when viewed longitudinally), appears to contain granules, probably the remains of the organising fluid contents of the cell, the mucous matter which is seen in growing cotton as mentioned by Captain Mitchell, in his letter to Mr. Hurst, read at this society, March 22nd, 1864.

On the 21st of April, 1863, Mr. Chas. O'Neill made a communication to this section, "On the Appearance of Cotton Fibre during Solution and Disintegration;" these experiments referred to the application of Schweizer's solution of copper and ammonia.

Under the action of this solvent, Mr. O'Neill considers that cotton exhibits spiral vessels situated either inside or outside the external membrane. In a paper, read by the same gentleman, on the 18th of May, 1863, it is stated that spiral vessels are seen during the solution of gun cotton in ether and alcohol. On the 21st of December, 1863, Mr. Heys read a paper before this section, in which he refers to spiral vessels in cotton hairs which seem to prevent the collapse of the tubes. The announcement of the discovery of spiral vessels excited my curiosity. Having often examined varieties of cotton under the microscope, without suspecting any such structure, I was naturally desirous of witnessing its appearance during dissolution.

A careful examination of cotton in the copper solvent, with powers varying from 50 to 1,200 diameters, showed me the appearances described by Mr. O'Neill. I could not, however, endorse his interpretations of them. On the 16th

* See Mr. O'Neill's Paper, April 25th, 1863.

of January, 1865, I sent a letter to the chairman of the microscopical section, stating my belief that the spiral appearances could be clearly traced to a mechanical action which the solvent exerted on the vegetable cell, and that at some future time I hoped to illustrate this to the members of the section.

Since December last, I have subjected cotton during microscopical examination to a variety of influences in acids, alkalies, metallic solutions, iodine, and also gun cotton in varied proportions of ether and alcohol. Repeated experiments tend to confirm my disbelief in the existence of spiral vessels, properly so-called, either inside or outside cotton hairs.

It would be difficult to explain, by means of drawings, how these pseudo spirals are created, and have, therefore, supplied a number of microscopes for the purpose of showing at the close of the meeting the actual appearances.

Some of the gentlemen present have witnessed these experiments, but, for the benefit of those who have not, I shall attempt a brief explanation to enable them to comprehend more readily what they will see under the microscopes.

In order to observe the action of the copper solvent on cotton, place a few hairs about a quarter of an inch in length on a glass plate, and cover them with thin glass; it is useful to rub a little beeswax on the glass plate, in such a manner, as will just support the covering glass to prevent too great a pressure on the cotton; then arrange the cotton under the microscope with a power of not less than 200 diameters. The solvent should be applied by a glass pipette to the edge of the covering glass whilst the observer is looking through the microscope (this is important). If the solvent is very strong, the action is too rapid for the eye to follow, if of moderate strength it will be seen that as soon as the solvent comes into contact with the cotton in the field of view, a rapid rotation or twisting of the hairs takes place.

In my opinion, it is this rotating action which brings about the appearances which have been mistaken for spiral vessels.

The explanation which I have to offer for the phenomenon is this: first, we have the external membrane of the cotton, then the cellulose and primordial utricle, and finally, the dried contents in the cell, which I take to be the remains of the organising fluid.

Observation shows that the external membrane is not elastic and only partially soluble.

The cellulose is exceedingly elastic and soluble, and expands to a remarkable degree in the act of dissolution. The contents of the cell behave in a similar manner to that of the external membrane; it is neither elastic nor very soluble. The most successful experiment is made by allowing the copper solvent to come at once into contact with some length of the cotton hair. The solvent permeates some parts of the external membrane more easily than others, and causes a rapid expansion of the cellulose, which bursts the external membrane, and as this action is taking place at various portions of the same hair, a tangential force is exerted which twists and contorts the cotton in the direction of its length, and thus a spiral appearance is given to the whole structure of the cell.

The non-elastic external covering is twisted round the expanded cellulose, sometimes as a single band, at others like a bundle of fibres.

In those parts where the external covering has given way all round the hair, the cellulose expands into a bulb, pushing back the external membrane into a series of folds which form a ligature, and resists the expansive force of the cellulose. A number of these ligatures cause the expanded cellulose to assume the appearance of a string of beads. The lateral expansion of the cellulose contracts the length of the hair, and this causes the contents in the cavity of the cell to assume a corrugated appearance; this corrugation has also been subjected to the twisting power along with the other parts of the cell, and thus its spiral appearance is produced.

What becomes of the primordial utricle, I cannot state with certainty. After the disappearance of the cellulose there is an envelope left, which surrounds the contents of the cavity, this may be the primordial utricle, or the film left by the drying up of the protoplasmic or organising fluid.

If the solvent is made to come into contact with the ends of recently-cut cotton a beautiful trumpet mouth is produced—the exposed surface of cellulose has expanded and pushed back the external covering into folds—the contents of the cell may, in this case, be seen projecting from the mouth of the trumpet form.

Long after the complete dissolution of the cellulose has taken place, the external membrane remains just as the rotation or twistings had left it, some portions in the form of rings, which had been the ligatures between the bulbous expansions, other portions as irregular spirals.

The cell contents also remain as twisted corrugations. From the observed difference in solubility between the cellulose and the external and internal matter, I should imagine a difference in constitution.

A few experiments have led me to suspect that some of the spiral appearances observed in hemp and flax fibres during dissolution may possibly be caused by the mechanical action of the solvent employed.

P.S.—In making the cupric oxide with ammonia, the oxide of copper requires a thorough washing before dissolving in the ammonia. The presence of any salt of ammonia, even in very small quantities, interferes with its power in dissolving cotton.

Mr. DANCER also read a paper "On Pseudoscopic Vision through Prisms."

If we look with both eyes at an object, such as the flat top of a table for example, and then interpose a prism between one eye and the object, we discover, after a short time, that the portion of the surface to which the sight is particularly directed has apparently changed its distance. If, in trying the experiment, the thin edge of the prism is turned inwards to the nose, the flat surface will appear concave, if, on the contrary, the base or thick edge is turned towards the nose, the surface will appear convex. The full effect of this alteration in the appearance of the object is not realized immediately, some persons see it perfectly in a few seconds, others require some moments of steady gazing before it becomes evident to them.

The character of the surface to which the vision is directed exercises some influence in producing the effect. A circular table covered with a cloth of a bright pattern, having a few articles disposed towards the edges, exhibits this fallacious vision in a marked degree.

The angle of the prisms for shewing these experiments should be about 15 degrees, if less than this, the elevation or depression of surface is not sufficient to produce a good effect; if the angle is much greater than 15 degrees, many persons are unable to unite the refracted image of the prism with the real image seen by the other eye.

Achromatic prisms are much to be preferred in these experiments to those which are uncorrected for colour. Experiments with these prisms have shewn that the power of converging the optic axes, differ very considerably in individuals.

Oculists occasionally recommend prismatic lenses mounted in spectacles to assist persons who suffer from insufficiency of the recti interni muscles; it would be interesting to know if those so assisted, have noticed the fallacious appearances which the healthy eye can appreciate. The pseudoscopic effects are exaggerated by using a prism to each eye, but in most persons this produces a painful sensation.

The explanation of these phenomena, which I offer with some hesitation, is based upon the supposition that in binocular vision we estimate the distance of an object by the degree of convergence of the optic axes. In these experiments, when a flat surface appears concave by the interposition of the prism: the optic axes are made to converge on a point situated behind the real surface, and the imagination gradually removes the object to this apparent distance.

When the base of the prism is towards the nose, then the flat surface becomes convex, in this case the optic axes cross in front of the real surface, and the imagination raises the object to that point. A diagram of the convergence of the optic axes on an object, before and after the interposition of the prism, will show that when the thin edge of the prism is turned towards the nose, the effort made to unite the real and the refracted image is the same as if the vision was directed to a point more distant than the real object. The opposite to this takes place when the base of the prism is turned towards the nose. It is very possible that the pseudoscopic vision through prisms may have been noticed by others, but I have not been able to discover any description of such in the works to which I have access.

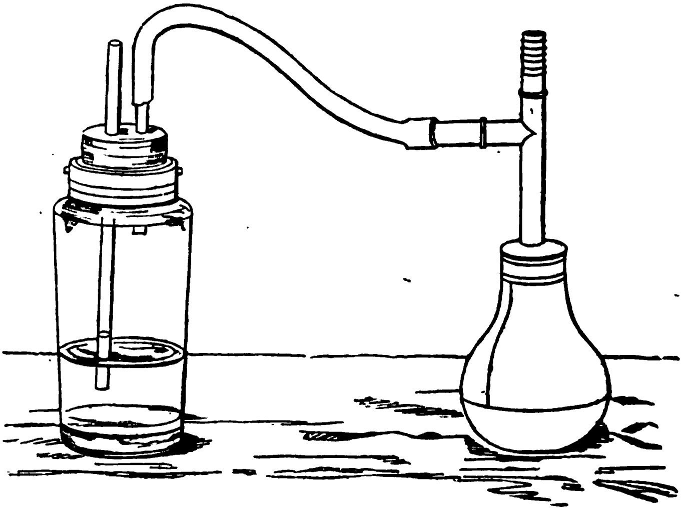
**Dr. Angus Smith** explained a mode of analysis which he has called minimetric. The idea, he said, may perhaps not be quite new, but it is well to give the method a name. It is based mainly on the fact that we can retain in the memory with great exactness the character of a precipitate of a given degree of translucency. For carbonic acid the author finds a precipitate of carbonate of baryta caused in baryta by ·2515 cub. c. of carbonic acid, or nearly three times that amount in lime water. If the carbonic acid in air is sought, the air is made to act on the baryta until the precipitate is obtained. In other words we use the *smallest measure* of air which will produce the precipitate. For this reason the name *minimetric* is adopted. The plan may be used for hydrochloric acid, sulphuric and sulphurous acid, sulphuretted hydrogen, &c., and probably has been used frequently without bringing it forward as a method for accurate use.

Two modes of using this mode of analysis were described. The first was by the use of a finger pump, an elastic ball with two valves. When pressed the air was driven out, and when expanding the air was drawn through the liquid. The air and liquid were then shaken together. This was repeated until the precipitate was attained.

One easy method of finding the precipitate for comparison was by shaking half an ounce avoirdupois (14·17 cub. c.) with 23 ounces of air in Manchester, or nearly 30 in London, or elsewhere, according to residence.

Experiments made with this apparatus shew it to be extremely delicate. The carbonic acid in the air of a room can be estimated in a few minutes.

A table is made of the following kind, but it must be adapted to the size of the bulb. A cut showing the bottle and finger pump is given.



For very bad air smaller bulbs were shewn, such as were recommended for workshops, mines, &c.; a convenient size for common life is here given:—

| No. of Strokes of the Finger Pump, or No. of Ballfulls of Air. | Per cent. of Carbonic Acid indicated in the Air. | Actual Amount of Carbonic Acid in the Air of the Ball. |
|---|---|---|
| | | Grammes. |
| 1 | 6·0 | 0·2515 |
| 2 | 3·0 | 0·1257 |
| 3 | 2·0 | 0·0838 |
| 4 | 1·5 | 0·0629 |
| 5 | 1·2 | 0.0503 |
| 6 | 1·0 | 0·0419 |
| 7 | 0.8 | 0·0359 |
| 8 | 0·75 | 0·0316 |
| 9 | 0·66 | 0·0279 |
| 10 | 0·056 | 0·0251 |
| 11 | 0·054 | 0·0229 |
| 12 | 0·0499 | 0·0209 |
| 13 | 0·0460 | 0·0193 |
| 14 | 0·0428 | 0·0180 |
| 15 | 0·0399 | 0·0167 |

In all cases only half an ounce of baryta solution was used.

### *Minimetric House and Workshop Method.*

### A.

This method is partly described in the report on the air of mines, and long tables given. There were shown two modes of using it, first, with baryta water; second, with lime water.

Suppose we desire to know if the air contains more than 0·04 per cent of carbonic acid, we fill a bottle containing 5·482 ounces with air by pumping as elsewhere described, with a little finger pump, and shake in it half an ounce of baryta water. If there is any precipitate at all, the amount of carbonic acid in the air is above 0·04 per cent. This would indicate that the air is less pure than outside.

If we allow 0·06 p.c. of carbonic acid in a room, we take a bottle of the size of 3·6+½ ounce=4·1 ounces, or 116·23 c.c., and if, after a trial as before, we find a precipitate, however small, or a decided, although slight milkiness, the air is deteriorated beyond 0·06. This relates to dwelling-houses. If for workshops ¼ p.c. (0·25) is allowed, a bottle of 0·867+0·5 ounce=1·367 ounces or 38·744 c.c. is sufficient. This could go into the waistcoat pocket.

If 0·5 or ½ per cent. is permitted, a bottle of 0·433+0·5 ounce is enough=0·933 ounces or 26·475 c.c. This amounts to nothing more than shaking an ounce bottle. The addition of half an ounce is for the baryta water.

## B.

The lime water method will probably be adopted more usually, as lime is so common. The experiment is exactly the same as with baryta water, but larger bottles are required.

0·06 carbonic acid in the air gives no precipitate or milkiness when ½ ounce of lime water is added to a bottle of the air, containing..................................10·9 ounce.
(=309 cub. cent.)

| | | |
|---|---|---|
| 0·25 ditto | ditto | 2·997 ounces.<br>(=84·958 cub. c.) |
| 0·5 ditto | ditto | 1·748 ounces.<br>(=49·564 cub. c.) |

The author said that by this simple method the greatest refinement could be attained.

## Annual Meeting, April 25th, 1865.

R. Angus Smith, Ph.D., F.R.S., President, in the Chair.

On the motion of Mr. Brockbank, seconded by Mr. Carrick, it was resolved unanimously—"That the election of Sectional Associates be continued during the next session."

The following report of the Council was read by one of the Secretaries: —

In presenting their annual report the Council have again to congratulate the Society on the satisfactory position of its finances. Although several payments of a somewhat exceptional character have been made during the past year, the balance in the Treasurer's hands has only been reduced from £371. 8s. 1d. on the 31st of March, 1864, to £360. 4s. 3d. on the 31st of March, 1865.

The number of ordinary members on the 1st of April, 1864, was 193, and *ten* new members have since been elected. The loss by resignations has been *three*, by defaulters *four*, and by deaths the unusually large number of *nine*. The number of ordinary members on the roll of the Society on the 1st of April instant was 187.

The deceased members are,—Messrs. John Atkinson, C. F. Ekman, John Gould, John Hetherington, Thomas Hopkins John Jesse, F.R.S., William Nield, John Shuttleworth, and Robert Walker, M.D.

In the death of their late Honorary Librarian, Mr. C. F. Ekman, the Society have lost an officer whose intelligence, zeal, and devotion to his duties and to the interests of the

Society, have rarely been equalled. In his hands the Society's library has become one of the most valuable libraries of reference in the kingdom; and before his death he had succeeded in establishing a system of exchange of publications with nearly all the leading home and foreign literary and scientific societies. He took a very prominent part in the framing of the new code of rules passed by the Society on the 22nd of January, 1861; and his unwearied exertions and influence, and the valuable assistance he was ever ready to render to members or others engaged in original researches, contributed materially to raise the Society to its present high position.

Mr. John Atkinson, F.G.S., had been a member of the Society for many years, and was a constant attendant at its meetings, to which he made frequent communications. During late years he supplied the Society with his meteorological observations made at Thelwall, and which were printed in our proceedings. He was for several years the active Secretary of the Manchester Geological Society, to which he contributed several papers published in its memoirs.

Mr. Thomas Hopkins was one of the oldest members of the Society, having been elected on the 18th of April, 1823. He was for many years a very active and efficient member of the Council, and was for some time one of the Society's Vice-Presidents. He contributed several valuable papers on meteorological subjects to the Society's memoirs, and was the author of a work on "Atmospherical Changes."

In accordance with the resolution passed at a meeting of the Society held on 12th January, 1864, the Council procured dies for a medal, and six copies of the medal in bronze; and appointed a committee to consider and report upon a scheme for regulating the selection of subjects and services for which a medal should be awarded. Owing, however, to the importance of the questions which have been brought under their notice, and the necessity of giving them careful and mature consideration, the Committee have not yet been able to present their report, but hope to be able to do so at an early meeting in the ensuing session.

Since the last annual meeting the proceedings of the Sections have been marked by the same activity and interest which have characterised them from their establishment. In connexion with the Sections the Council refer with satisfaction to the success which has attended the scheme for admitting Sectional Associates. Twenty gentlemen have availed themselves of the advantages which it offers; and by the communications some of them have made, and the part they have taken in the discussions of the various subjects that have been brought under notice, they have added much to the interest and usefulness of the meetings.

The following is a list of the communications made to the Society at its ordinary and sectional meetings during the past session:

*October 4th*, 1864.—"Remarks on Mr. Dyer's Paper, entitled 'Notes on Spinning Machines,'" by H. Brierly, Esq., communicated by E. W. Binney, Esq., F.R.S., &c.

*October 18th*, 1864.—"On the Relation of Force to Matter and Mind. Part I.," by the Rev. Thomas P. Kirkman, M.A., F.R.S., Hon. Mem.

*October 13th*, 1864.—"Note on a New Star near the Greenwich Variable, No. 1773, of the Twelve-Year Catalogue," by Joseph Baxendell, F.R.A.S.

*October 17th*, 1864.—"On a Contrivance for regulating the amount of light transmitted from the Source of Illumination to the Mirror of the Microscope," by J. B. Dancer, F.R.A.S.

*November 15th*, 1864.—"On the Composition of the Atmosphere," by R. Angus Smith, Ph.D., F.R.S., President of the Society.

*November 29th*, 1864. — "On Differential Equations," by His Honour Chief Justice Cockle, M.A., F.R.A.S., &c., communicated by the Rev. Robert Harley, F.R.S., &c.

*November 29th*, 1864.—"Notes on Marine Shells found in Stratified Drift at Macclesfield," by R. D. Darbishire, B.A., F.G.S.

*November 28th*, 1864.—"On Specimens of Foraminifera from Roundstone, Connemara," by Thomas Alcock, M.D.

*December 8th*, 1864.—"Note on the Period and Changes of the Greenwich Variable in Vulpecula, No. 1773 of the Twelve-year Catalogue," by Josh. Baxendell, F.R.A.S.

*December 8th*, 1864.—"Observations of the Greenwich Variable in Vulpecula and its Companion Stars," by George Knott, F.R.A.S., communicated by Josh. Baxendell, F.R.A.S.

*December 13th*, 1864.—"On the Discovery of the Bones of the Mammoth (Elephas Primigenius) in a Fissure of the Carboniferous Limestone at Waterhouses, near Leek," by William Brockbank, Esq.

*November 3rd*, 1864.—"On Printing Transparencies for the Stereoscope and Magic Lantern," by Joseph Sidebotham, Esq.

*December 19th*, 1864.—"On the Structure of Cotton," by W. H. Heys, Esq.

*January 10th*, 1865.—"On some Products derived from Indigo Blue," by Dr. E. Schunck, F.R.S., &c.

*January 10th and 24th*, 1865.—"On some Physiological Effects of Carbonic Acid," by Dr. R. Angus Smith, F.R.S., President.

*January 24th*, 1865.—"Observations of a large group of Solar Spots," by Jas. Nasmyth, Esq., C.E., Corresponding Member of the Society.

*January 12th*, 1865.—"Account of a Fire-ball seen December 13th. 1864," by Thomas Heelis, F.R.A.S.

*January 12th*, 1865.—"Note on the Rainfall of 1864," by G. V. Vernon, F.R.A.S.

*February 7th*, 1865.—"On a New Re-agent for the Separation of Calcium from Magnesium," by Edward Sonstadt, Esq.

*January 16th*, 1865.—"On the Plumules or Battledore Scales of the Lycænidæ," by John Watson, Esq.

*January 16th*, 1865.—"Notes on the Development of the Wings of Lepidopterous Insects," by Josh. Sidebotham, Esq.

*February 21st*, 1865.—"On a new form of Roof for Dyehouses," by John Thom, Esq.

*February 21st*, 1865.—"On the Action of Caustic Soda on Ethylic and Methylic Alcohol," by Mr. A. Mylius, communicated by Dr. E. Schunck, F.R.S.

*January 5th*, 1865.—"On the Exhibition Stereoscopically of Photographs on a large scale." by J. B. Dancer, F.R.A.S.

*February 2nd*, 1865.—"On the proper focus of Lens to be used in taking Photographic Landscapes; also on some modes of measuring the size of objects therein depicted," by Joseph Sidebotham, Esq.

*March 7th*, 1865.—"On the Action of Sea Water upon certain Metals and Alloys," by F. Crace Calvert, Ph.D., F.R.S., &c., and R. Johnson, F.C.S.

*March 2nd*, 1865.—"The Opaque Microscope not New," by J. B. Dancer, F.R.A.S.

*March 2nd*, 1865.—"Photographic Experience in India," by E. C. Buxton, Jun., Esq.

*March 21st*, 1865.—"On an Instrument for showing rapidly Minute Changes of Magnetic Declination," by J. P. Joule, LL.D., F.R.S., &c.

*March 21st*, 1865.—"Further Observations on the Permian and Triassic Strata of Lancashire," by E. W. Binney, F.R.S., &c.

*February 27th*, 1865.—"Notes on Mosses," by G. E. Hunt, Esq.

*March 16th*, 1865.—"Results of Raingauge and Anemometer Observations made at St. Martin's Parsonage, Castleton Moor, during the year 1864," by the Rev. J. C. Bates, M.A., F.R.A.S., and Note on the same, by Josh. Baxendell, F.R.A.S.

*April 4th*, 1865.—"An Instance of the Injurious Action of Alkalies on Cotton Fibre," by Messrs. Heinrich Caro and William Dancer.

*April 4th*, 1865.—"Remarks on the Microscopical Appearances of Cotton Hair during Dissolution in the Ammoniacal Solution of Copper," by J. B. Dancer, F.R.A.S.

*April 4th*, 1865.—"On Pseudoscopic Vision through Prisms," by J. B. Dancer, F.R.A.S.

*April 4th*, 1865.—"On the Minimetric Method of Analysis," by Dr. R. Angus Smith, F.R.S., &c., President of the Society.

The printing of volume second of series third of the Society's Memoirs, comprising the papers read before the Society during the Sessions 1861-2, 1862-3, 1863-4, has been completed, and bound copies are now ready for distribution at 5s. to members of the Society and 10s. to the public.

A new volume has already been commenced, containing papers read during the past session; and arrangements have been made by which papers intended for the Memoirs will be printed immediately after they have been passed by the Council.

The Librarian reports that when he took office he did it with the idea of preparing a Catalogue which had for some years been much required. On further examination he found it necessary both to rearrange the books, and to prepare a Stock-book; some delay was thus entailed. He is now, however, glad to inform the members that an alphabetical catalogue is in process of printing.

On the motion of Professor CLIFTON, seconded by Mr. DARBISHIRE, the Annual Report was unanimously adopted.

The following gentlemen were elected officers of the Society for the ensuing year:—

**President.**

R. ANGUS SMITH, PH.D., F.R.S., &c.

**Vice-Presidents.**

JAMES PRESCOTT JOULE, LL.D., F.R.S., &c.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
JOSEPH CHESBOROUGH DYER.
EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.

**Secretaries.**

HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S., F.C.S.
JOSEPH BAXENDELL, F.R.A.S.

**Treasurer.**

ROBERT WORTHINGTON, F.R.A.S.

**Librarian.**

THOMAS WINDSOR, M.R.C.S.

**Other Members of the Committee.**

REV. WILLIAM GASKELL, M.A.
PETER SPENCE, F.S.A., F.C.S.
GEORGE VENABLES VERNON, F.R.A.S., M.B.M.S.
ROBERT BELLAMY CLIFTON, M.A., F.R.A.S.
JOSEPH SIDEBOTHAM.
HENRY MERE ORMEROD.

## ROBERT WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER,

## FROM 31st MARCH, 1864, TO 31st MARCH, 1865.

**Dr.**

| Date | | | £ s. d. | £ s. d. |
|---|---|---|---|---|
| 1864. April 1. | —To Balance in the Bank of Heywood Brothers and Co. .. | | | 371 8 1 |
| 1865. Mar. 31. | —To *Members' Contributions:* | | | |
| | Arrears, 1863-4 .......... 5.. | | 10 10 0 | |
| | ,, half Subscription, 1864 .......... 6.. | | 6 6 0 | |
| | ,, Admission Fees, 1863-4 .......... 6.. | | 12 12 0 | |
| | Members on the Roll, April 1, 1864, 193, at 42s. .......... | £405 6 0 | | |
| | Deduct Compounders .......... 3 | | | |
| | ,, Dead .......... 9 | | | |
| | ,, In Arrear, March 31, 1865 .......... 11 | 48 6 0 | | |
| | 23 | | 357 0 0 | |
| | 8 Members elected in 1864, April—December | | 16 16 0 | |
| | 2 ,, ,, 1865, half subscription. | £2 2 0 | | |
| | Deduct in arrear .......... | 1 1 0 | | |
| | | | 1 1 0 | |
| | Nine Admission Fees received 1864-65 .......... | | 18 18 0 | |
| | | | | 423 3 0 |
| ,, ,, | To *Sale of Publications:* | | | |
| | Memoirs—Ser. 3, Vol. I. .......... | | 1 1 0 | |
| | Proceedings .......... | | 0 4 6 | |
| | Memoir of Dalton .......... | | 0 2 6 | |
| | For Books not returned to Library .......... | | 1 2 6 | |
| | | | | 2 10 6 |
| ,, ,, | To *Sundry Income:* | | | |
| | Section Contribution: Mathematical .......... | £2 2 0 | | |
| | ,, ,, Microscopical .......... | 2 2 0 | | |
| | ,, ,, Photographic .......... | 2 2 0 | | |
| | Eight Associates of Microscopical Section .. | 4 0 0 | | |
| | Ten Associates of Photographic Section .... | 5 5 0 | | |
| | | | 15 11 0 | |
| | Late Photographic Society, Attendance, Coal, &c., at Meetings .......... | | 5 0 0 | |
| | Interest allowed by Bankers .......... | | 18 7 0 | |
| | | | | 38 18 0 |
| | | | | £835 19 7 |
| | Library Fund .......... | | £86 7 0 | |
| | Compositions .......... | | 72 10 0 | |
| | General Balance .......... | | 201 7 3 | |
| | Total Balance .......... | | £360 4 3 | |

**Cr.**

| | | £ s. d. | £ s. d. |
|---|---|---|---|
| March 31, 1865. | | | |
| By *Charges on Property:* | | | |
| Chief Rent .......... | | 12 11 7 | |
| Fire Insurance .......... | | 7 17 6 | |
| Property Tax .......... | | 4 5 0 | |
| | | | 24 14 1 |
| By *House Expenditure:* | | | |
| Water, Gas, Candles, and Coal .......... | | 21 19 11 | |
| Cleaning and Petty Expenses .......... | | 3 13 3 | |
| Furniture, Bookcases, &c. .......... | | 61 7 5 | |
| Repairs and Alterations .......... | | 67 17 11 | |
| Tea and Coffee at Meetings .......... | | 19 14 9 | |
| | | | 174 13 3 |
| By *Administrative Charges:* | | | |
| Wages—Temporary Keeper of Rooms .......... | 3 10 0 | | |
| Ditto —Assistant ditto .......... | 1 0 0 | | |
| Ditto —Keeper of Rooms .......... | 38 5 0 | | |
| Ditto —Assistant Librarian .......... | 21 0 0 | | |
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| Attendance on Sections .......... | | 6 6 0 | |
| Postage and Parcels .......... | | 18 0 0 | |
| Stationery, Printing Circulars, &c., and Receipt Stamps .... | | 18 12 4 | |
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| By *Publishing:* | | | |
| Memoirs—Printing, Engraving, &c. .......... | | 22 6 0 | |
| Printing Proceedings .......... | | 48 15 0 | |
| Editor of Memoirs and Proceedings .......... | | 50 0 0 | |
| | | | 121 1 0 |
| By *Library:* | | | |
| Periodicals, Binding, &c. .......... | | 18 13 2 | |
| Subscription to Ray Society .......... | | 1 1 0 | |
| ,, Cavendish Society .......... | | 1 1 0 | |
| ,, Palæontographical Society .......... | | 1 1 0 | |
| | | | 21 16 2 |
| By *Sundry Expenditure:* | | | |
| Advertising for Keeper of Rooms .......... | | 0 4 6 | |
| Society's Medal .......... | | 26 13 0 | |
| | | | 26 17 6 |
| Total Disbursements .......... | | | 475 15 4 |
| Balance in Bank of Heywood Brothers & Co. .......... | | 335 0 3 | |
| ,, Treasurer's hands .......... | | 25 4 0 | |
| | | | 360 4 3 |
| | | | £835 19 7 |

ROBERT WORTHINGTON, Treasurer,
*1st April*, 1865.

Examined and found correct, 21st April, 1865:

HENRY M. ORMEROD, } Auditors.
WM. BROCKBANK,

Dr. Joule communicated the following extracts of letters referring to the auroral arch of the 20th of March last:—

*The Astronomer Royal to Alexander S. Herschel, Esq.,*
Royal Observatory, Greenwich,
1865, March 31.

"I have referred to our Magnetic Photograms on the 20th inst. The magnets were considerably disturbed for many hours before and after the appearance of the arch; but the great disturbances of declination and hor. force were from 7h. to 8h. 45m., and those of vert. force nearly 45m. later. Of these you shall have copies when the time-scales are properly attached. There is scarcely a more important inquiry at the present time than that of the connexion between the *individual* phases of auroras, and the corresponding *individual* phases of magnetic disturbance. The first requisite is accurate clock time. By all means establish a reference to my Time Signals.

I have seen many auroras, probably more than 20, fewer than 50. I am quite familiar with the ordinary appearances: low arch, grey or red streamers rising to or beyond the magnetic zenith, great red clouds, light flashing over limited spaces (the successive portions of space taking the light in successive portions of time) and others; but I think I have seen the lofty well defined pale arch only twice. In some accounts of arches which I have read, two or three arches have followed in succession, all based in the magnetic E and W, and rising like a skipping-rope. As they approach very near the magnetic zenith, they exhibit a radiated structure And, finally, it appears that an arch really is a very long slender parallelopiped, its length in magnetic E and W consisting *of beams in the direction of magnetic dip.* If you have not been possessed of this idea before, the possession of it may sharpen you in future observations or future inquiries."

*Alexander S. Herschel, Esq., to R. P. Greg, Esq.*

Collingwood, Hawkhurst, Kent,
1865, April 11.

"I was much obliged by the receipt of an observation of the auroral arch of March 20 (from the *Liverpool Mercury*), as seen at Windermere. Compared with another letter from the same writer (Mr. Hall) in the *Standard*, it gave a good place by the stars. In the 'Proceedings of the Manchester Literary and Philosophical Society' is another description by Mr. Baxendell. The arch was well observed here, almost stationary, varying its altitude from 18° to 20°, and at last to 22° or 23° in the course of half an hour. The first observations correspond with those of Windermere, at the time when the arch was nearly vertical over Windermere; and the last with those at Manchester when the arch, as seen by Mr. Baxendell, was nearly overhead at Manchester. Both give the height of the arch 102 miles above the earth, and I doubt if a better double observation of an arch of the aurora of the 'skipping-rope' form has ever been obtained. The result can hardly be more than five miles, certainly not so much as ten miles in error either way. Mr. Airy says that among twenty to fifty auroras that he has witnessed he has only twice seen the auroral arch, 'rising like a skipping-rope.' Perhaps you would have the goodness to communicate Mr. Airy's letter to Dr. Joule, as it will interest him in his observations of the magnetic variations; and at the same time to Mr. Baxendell."

The Rev. T. P. Kirkman, F.R.S., made the following communication:—

As the printing of my completion of the Theory of Groups is delayed longer than I expected, I desire to insert the following table of *corrections* to be made in the list of *titles* at page 142 of the Proceedings of this Society, July, 1863.

## CORRIGENDA, p. 142, &c.

In the group of order 6·4·2, for $13_{2^3}$ read $7_{2^3} + 6_{41}$;
in that of the order 5·2, erase $10_{41}$;
in the second title, page 144, for $4_{2^31^2}$ read $4_{42^2}$;
in the eighth title of that page , for $12_{42^2} + 8_8$ read $4_{42^2} + 16_8$;
in the ninth title, p. 142, for $4_{2^4}$ read $4_{2^31^2}$;
in the title 8·6·4·2, for $52_{2^31^2} + 24_{42^2}$ read $28_{2^31^2} + 36_{42^2} + 12_{41^4}$.

## ADDENDA.

$4 = 1 + 3_{2^2}$, Q = 1.

$6 \cdot 2 = 1 + 3_{2^21^2} + 8_{3^2}$, Q = 15.

$6 \cdot 4 = 1 + 6_{41^2} + 3_{2^21^2} + 8_{3^2} + 6_{2^3}$, Q = 15.

$8 \cdot 4 = 1 + 6_{2^21^4} + 13_{2^4} + 12_{4^2}$, Q = 210.

$8 \cdot 4 = 1 + 6_{2^21^4} + 4_{4^2} + 5_{2^4} + 16_{42^2}$, Q = 315.

$8 \cdot 4 = 1 + 6_{2^21^4} + 4_{4^2} + 5_{2^4} + 16_8$, Q = 315.

$8 \cdot 4 = 1 + 8_{4^2} + 5_{2^4} + 2_{2^21^4} + 8_{2^31^2} + 8_8$, Q = 630.

$8 \cdot 8 = 1 + 10_{2^21^4} + 8_{421^2} + 4_{4^2} + 9_{2^4} + 16_8 + 16_{42^2}$, Q = 315.

$8 \cdot 8 = 1 + 4_{41^4} + 8_{2^31^2} + 6_{2^21^4} + 5_{2^4} + 20_{4^2} + 20_{42^2}$, Q = 315.

$8 \cdot 6 \cdot 4 = 1 + 6_{2^21^4} + 12_{4^2} + 13_{2^4} + 32_{3^21^2} + 32_{62} + 24_{2^31^2} +$
$12_{41^4} + 48_8 + 12_{42^2}$ Q = 105.

The omission of the above-given groups of 6·2 and 6·4 in my first processes was the main cause of the above errors and defects.

## MICROSCOPICAL SECTION.

March 20th, 1865.

J. Sidebotham, Esq., President of the Section, in the Chair.

Mr. Dancer read a Paper, "On the Microscopical Appearances of Cotton Hair during dissolution in the Ammoniacal Solution of Copper."

Mr. Heys, in proposing a vote of thanks, said that he was gratified to find that the important subject of the structure of cotton, in which he had himself for some time felt an interest, was now being taken up in a manner likely to clear away all doubts.

Mr. Heys said he had been asked by Mr. Dale to introduce to the notice of members a preparation of Canada Balsam for mounting, which has the property of hardening in a very short time. It consisted of Balsam first made perfectly solid by evaporation, and then dissolved in Bisulphide of Carbon. He found the smell a strong objection to its use, but the results were very satisfactory, the balsam on a slide becoming perfectly hard in a few hours. He thought however the dry balsam might prove to be the more important element in the preparation, and that its solution in Chloroform would probably be found to answer all practical purposes.

Mr. Dancer exhibited many beautiful photographs of microscopic objects by Dr. Maddox.

## March 27th, 1865.

JOHN PARRY, Esq., in the Chair.

### *Exhibitions.*

Sections of various Shells.—MR. PARRY.

An apparatus for applying pressure to the cover-glasses of objects freshly mounted in Canada Balsam. It consisted of a dozen small upright pistons placed in a frame, and each furnished with a spiral spring coiled around the rod and pressing against the upper horizontal bar of the frame.

### *Communications.*

Dr. ALCOCK exhibited a second time his specimens of shells of Marine Entomostraca, from the coast of Galway. He said that renewed examination of them and the collection of many more specimens had strengthened his belief that, however numerous their forms, they are entitled to be considered as so many distinct species, and he did not think that the general arguments used by Prof. Williamson in support of an opposite opinion at the meeting of February 20th, had any application to their particular case. In the first place it was stated that the outer skin or shells of these creatures is of less value for distinction than the internal parts; but Dr. Baird had described nine out of his fifteen species from living specimens, and yet in these his specific distinctions are mainly derived from the shells. Again, we were reminded that some of the Entomostraca are known to undergo metamorphoses, and that this might probably be the case with the genera Cythere and Cythereis; but Cypris, which approaches very nearly in character to Cythere, does not undergo these changes, and might furnish some ground for the supposition that these Marine Entomostraca also do not; but it would be sufficient for the present purpose to state that there is no support for the supposition that they do, on the general ground that some kinds of Entomostraca are known to undergo these changes, because it is also well known that other kinds do not.

But a further objection was that low forms of animal life are liable to extreme variability, and that the same remark is applicable to the lower groups in the higher divisions of animals, the argument being that in these cases the variability may be so great as to render specific distinctions of little or no value. He felt compelled to state that he did not admit this excessive variability as a certain fact, even in the lowest forms of animal life, until it could be actually proved to be true by most careful observation, and even then he would apply it no further than to the particular cases in which it had been proved; but as to making it a general rule with the lowest groups of the higher divisions of animals, he saw not the slightest reason for doing so, and here, more than in the former case, the diversity of character in the different groups would render it extremely unlikely that what might apply to one would also be found to apply to another. Even among low forms of animal life such as the Foraminifera, he had searched in vain for examples of that extreme variability which must necessarily confound specific distinctions, and he was sure that every young working naturalist, at all events, would agree with him that a conclusion so discouraging to exact observations as that suggested by Prof. Williamson, ought to be accepted by no one unless he became convinced of its truth by his own painful experience. With regard to the Foraminifera he had no hesitation in saying that, with only a few exceptions, the extraordinary uniformity of character in individuals of the same type was a fact that must strike every observer. In the cases of Miliolina and Polymorphina he would admit there is difficulty, but an examination of many of their numerous forms did not suggest to his mind that a great tendency to variation, in the ordinary sense of the term, would explain their diversity. Knowing, however, from his own observations what is meant by the statement that the Foraminifera are liable to great variation, he was prepared to say that the shells of the Marine Entomostraca do not at all show this liability; the forms are clearly defined, and distinct from one another, and intermediate forms blending the characters of two others rarely if ever occur.

Then as to the last objection which was brought forward, namely the difficulty of defining what a species is, he believed that practically it might be put aside altogether; for the fact was that any creature whatever which could be shown to be clearly distinct from all others that had been described must be admitted as a species, and must remain a species until it could be proved to be unworthy of this distinction. In conclusion he would say that he had no strong opinion as to whether this question of what a species is could or could not be answered, but he felt sure that to take Prof. Williamson's suggestion, and merge some ten or twenty of these forms of Marine Entomostraca into one species would be to make it impossible to form even a conjecture of what is meant by that term.

A Paper was read "On the Chætopod Annelides of the Southport Sands," by BENJ. CARRINGTON, M.D.

### AMPHINOMADÆ.

*Aphrodite aculeata*, L. Found sparingly near low-water mark; more frequent after storms.

*Pholoë inornata*, Johns. Very rare.

*Polynoa squamata*, Sav. Found occasionally within old shells; frequent among oysters, and the refuse from the fishing boats.

There are two well-marked varieties:

*a.* with a dark-brown crescentic mark on each of the elytra.

β. ochracea, uniform pale orange or stone colour.

*Polynoa cirrata*, Johns. Very rare.

*P. asterinæ*, sp. nov. Linear-oblong, scales twenty pairs or more, smooth, with a black entire border, seated on each third ring; intermediate feet cirriferous, bearing at the base a ciliated crest. Upper antennæ three, the central one longest.*

Not uncommon, occupying the groove between the suckers, of *Asterias aurantiaca*. I was first led to suspect the presence of some foreign species, by observing a blue phosphorescent light, given off from defined points of the rays, when the star-fish was placed in fresh water. It seems a very sluggish worm, and how it contrives to escape the surrounding suckers, and whether it shares the food captured by the star-fish, are points yet to be determined.

Body one to two inches long, by a line in breadth, posterior segments narrowed, ending in two filiform styles. Peach-blossom, or flesh coloured; very fragile, so that it is almost impossible to obtain an entire specimen. Scales white, chartaceous, with a narrow black border; first six pairs placed on alternate feet, the remainder on each third foot, not broader than the body, so that the feet are exposed, easily detached. In a line with the pedicels of the scales, on the intermediate rings, we find on each side a crest-shaped process, ending towards the mesian line in a short papilla. These are ciliated, as are the upper margins of the feet, so that horizontal currents are produced, as well as a central one from before backward. The dorsal papillæ seem to perform the functions of branchiæ. They also contain ova, which the ciliary currents serve to distribute.

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* In describing a species, I have thought it best to follow the nomenclature now in use, although I agree with Professor Huxley that a change is desirable. He proposes, after Milne Edwards, that the rings shall be called *somites*, the head *prestomium*, central antenna *prestomial tentacle*, upper and lower lateral antennæ *superior* and *inferior prestomial cirri*, foot tubercle *parapodium*, its upper and lower rami *notopodium* and *neuropodium*, &c.

I think it right also to state, that as my acquaintance with Marine Zoology is very recent, and I have been unable to consult several foreign works on the subject, I introduce the following species with great diffidence. To the best of my belief they are new to the British fauna, nor have I been able to identify them with species described by *Aud.* and *Edwards*, *Oersted*, *Grube*, *Ehlers*, &c.

Feet simple, with seven to ten strong spear-shaped golden setæ, apex toothed on one side. Near the dorsum of the foot is a small fascicle, containing four to six short curved toothed bristles.

Head concealed, roundish, emarginate. Upper antennæ three, the lateral ones very short, two jointed, central one much longer, equal to the two lower antennæ. Eyes four, distant. Tentacular cirri two pairs.

Readily distinguished by the long, flesh-coloured body, and marginate, smooth scales. In some young specimens, the black border is absent or ill-defined.

*P. maculosa*, sp. nov. Scales kidney-shaped, smooth, entire, membranous, having a dark curved spot round the centre, seated on alternate feet, intermediate feet bearing cirri; superior antennæ three equal; ventral surface of posterior rings, marked with four black dots.

Very rare; only one specimen found in company with *P. asterinæ*.

The specimen before me, which unfortunately has lost the anal segment, is oblong-obtuse, slightly narrowed from the middle, breadth two lines, by $\frac{3}{4}$ inch long. Scales twelve pairs, covering the head and feet, firmly attached, hyaline, especially near the border, which is slightly undulated, crossed from the inner margin by a retort-shaped black mark. Feet obtuse, obscurely biramous; upper branch much shorter, the setæ short, falcate, and serrate; lower branch bearing a tuft of twenty to thirty slender, half spear-shaped, oblique, pale setæ, toothed on one side, and ending in one or two larger teeth; shaft long, smooth, terete.

Head concealed, round, notched in front; eyes four, placed on the occipital portion; upper antennæ three equal, two jointed, the central one stout apiculate; two lower antennæ much longer, and exceeding the tentacular cirri. When viewed from the ventral surface, the basal portion of the antennæ is nearly black, the three central ones converging like the rays of a tripod, of which the dark-coloured oblong oral opening forms the handle. Posterior segments after the fifteenth marked with four rows of stellate spots.

Distinguished from *P. asterinæ* by the larger thin translucent scales, which are firmly attached and not bordered, by the more numerous and slender setæ, the equal upper antennæ, and the absence of the ciliated processes on the dorsal surface.

There is no other British species with smooth scales with which it is likely to be confounded. From *P. spinifera*, Ehlers, (which seems to me identical with Johnston's *P. scabra*,) and from *P. pellucida*, Ehlers (Annelid. Chætop., t. iii. f. 1—13), it may be known by the scabrous cirri and antennæ of those species. *P. maculata*, Grube, seems to be a form of *P. cirrata*, having the scales garnished with a few large papillæ.

*Sigalion Carringtonii*, nov. sp. Body vermiform, obtuse at both ends. Scales very numerous, attached to each ring, pellucid, outer border fringed with pectinate glands; feet exposed, bifid, densely setiguous. Attached to the pedicel at the base of each foot is a curved ciliated cirrus.

Met with occasionally, near low water mark, on the Birkdale shore, buried in the moist sand, where it lies coiled in a spiral manner. First discovered in July, 1864, when exploring the sands with my friend *Mr. C. H. Brown*, who named it as above.

Body linear, obtuse in front, tapering very gradually towards the anal segment, which terminates abruptly in two long styles. Length two to three inches by two lines in breadth. Colour greyish-white, opalescent, reflecting prismatic tints. Feet very numerous, slender at the base, biramous, upper branch gibbous at the apex, from which depends a short cirrus, furnished with a dense tuft of long silky setæ, fine as spun glass, which arch backward towards the scales, and a lower tuft of jointed ones; inferior ramus, bearing two kinds of setæ, the lower very long and flexible, and the intermediate ones jointed, the blade filiform, rough, articulate like the hair of a mole.

Scales smooth, hyaline, persistent, convex, closely imbricated, not covering the feet, fringed at the outer margin with a few pectinate-pinnate processes. Some of the anterior scales are seated on alternate feet, but the majority arise from a large ovate tubercle, which is found at the base of each foot. These tubercles are filled with ova, and appear like a white opaque spot through the scales, and from their outer border a stout curved cirrus originates, clothed with vibratile cilia on the lower side, and extending a little beyond the scales.

Head small, concealed, hemispherical; eyes four, minute, the pairs approximate. Upper antennæ minute, two on each side, placed at the angles of a broad basal portion, which, like the feet, bears a tuft of silky setæ; lower antennæ much longer. The anterior feet exceed the head in length, and project beneath it almost to the median line, so that it is difficult to make out the exact details.

Proboscis as broad as long, compressed; the lips clothed with a row of simple fimbriæ; jaws four, alternate, pointed.

The worm is sluggish in confinement, generally remaining coiled spirally like a serpent. Under the lens it is a beautiful object, the long silky setæ spreading like the feathers of a bird of paradise—the daintily fringed, translucent scales, through which the ciliated tentacles are seen in constant motion—and the play of prismatic colours on the surface—are sure to excite wonder and admiration.

### NEREIDÆ.

*Phyllodoce lamelligera*, Johns. Rare; at low water mark buried beneath the sand. It is a beautiful species, swimming freely in sea-water.

*P. Vittata,* Ehlers, *Annelid. Chætopod.,* 1864, t. vi. f. 7—14. New to Britain. Two specimens appeared in water containing a mass of *Sabellaria alveolata* from New Brighton, May, 1864 *(C. H. Brown),* and I have since met with one or two others at Southport.

Distinguished from *P. lamelligera* by its smaller size, never more than a line in breadth by two to three inches long, filiform, very active, generally assuming serpentine or spiral curves; pale olive, convex above, each ring crossed by a narrow, *stippled, steel-grey band, about a third of its breadth.* Head short, broadly ovate, obtuse; antennæ four apical, spindle-shaped; tentacular cirri four pairs, the two lowest very long (equal to five or six rings). Eyes large, black, crossed behind by the narrow first segment. Branchial leaflets ovate, reticulate, parallel with the body; lower branchiæ short obtuse, attached to the base of the feet. Anal segment bearing two leaf-like processes, resembling the branchiæ.

This active little worm resembles one figured by Sir G. Dalyell, but wants the central antenna.

*P. attenuata,* sp. nov.? Body very slender, from half a line to a line broad, four to six inches long; anterior rings as broad as long; branchial leaflet broadly ovate, seated on a pedicle as long as the feet, olive-brown veined; lower leaflet ovate acute; middle and posterior rings attenuated, *twice as long as broad;* branchiæ as wide as the segments over which they arch; feet small simple; bristles cultrate, curved, jointed, finely toothed at the base of the blade.

In the shape and relative size of the feet and branchial leaflets it agrees with *P. lamelligera*, but the disproportionally slender body, and oblong segments, distinguish it at a glance from any form of that species with which I am acquainted.

A solitary specimen and a portion of another are all that remain of this curious worm. Unfortunately, both head and anal segment have been rendered indistinguishable by the action of the spirit.

*P. Clava*, sp. nov. Worm minute, one to two inches long by $\frac{1}{12}$in. to $\frac{1}{16}$in. broad; rings narrow, depressed, pale drab or greenish; feet simple, bearing on the upper side ovate *tumid* branchiæ, lying parallel to the body; near the base on the ventral side of the feet short obtuse lower branchial papillæ are attached. Rings gradually tapering, and ending in an obtuse anal segment, which bears two clavate solid styles, larger than the branchiæ. Head broad at the base, terminating in a thin ornithorhynchus-like snout, apex obtuse; antennæ four, short, divergent. First and second rings half as broad as the rest, giving attachment to four pairs of short tentacular cirri. I am doubtful about the the identity of this Annelid, as I have seen no figure of *P. clavigera, Aud. and Ed.*

Rare, occupying vertical burrows in the sand, about half tide mark.

*Goniada Alcockiana*, sp. nov. Body tapering at both ends, anterior third terete terminating in a conical horn-like snout, posterior segments depressed, broader, channelled above, ending in two long jointed styles. Eyes, and tentacular cirri 0. Proboscis very long, clavate curved, on each side of the base are eight Λ-shaped dentacles, mouth armed with seven jaws.

Body filiform, 1½ inch long by a line wide, colour reddish brown; heteromorphous, anterior rings to the 45th, very convex, narrow; feet minute papillæform. Lower two-thirds of the worm depressed, rather broader; feet longer, oblique, from a dilated base, composed of four acute segments, the two outer shorter and divergent (branchial); each foot bears two fasciculi of bristles, the upper short curved, arising from the basal portion; lower composed of long, white, falcate jointed bristles. Apex of the conical snout bearing four minute antennæ, when the proboscis is exerted it stands up like a small horn. Proboscis nearly as long as the terete portion of the body, curved, fluted above, armed with seven minute black jaws, five in the upper and two in the lower half; middle jaws larger tridentate. At each side of the basal portion there is also a row of minute inversely V-shaped black dentacles, eight in number.

Very rare, only one specimen collected. I have great pleasure in associating the name of Dr. Alcock, who has done so much for the spread of natural science in Manchester, with this curious species. ?

*G. maculata*, Johns; the only other British species is distinguished by its greater length, 4 to 6 inches, or, according to Oersted, 18 to 20 inches; whilst its extreme breadth is only a line and a half! This species, according to Johnson, is destitute of jaws, and Dr. Baird informs me from Oersted's figure there appear to be no anal cirri. It is distinguished also by having three brown maculæ on each segment. In *G. Norvegica*, Oers., there are eighteen dentacles on each side of the proboscis.

*Glycera alba*, Lam.

A single specimen only obtained among tufts of *Antennaria antennina.*

*Pollicita peripatus*, Johns.

Several specimens found at the base of *Alcyonium digitatum* brought from deep water after storms.

*Scyllis prolifera*, Müll.

Probably abundant in wet places, covered with a stratum of mud, but from its minute size easily overlooked.

*Nereis pelagica*, L.

*N. viridis*, L. (*N. cerulia*, Penn.) Both these species are abundant in wet hollows, about half-tide mark, occupying a deep burrow in the sand. They vary much in colour, from a deep velvetty green to orange. There is another form, with longer feet, bright orange or flesh-coloured, shaded with olive, which is frequent near high-water mark, where the tide is absent for months together, which may be distinct.

*N. brevimana*, Johns.

*N. margaritacea*, Leach.

*N. Dumarillii*, Aud. and Edw.

These species are found occasionally among oysters dredged from deep water, or the refuse from fishing boats.

*Nereis bilineata*, Johns.

Not uncommon. Always found occupying the terminal coils of old whelk shells, and generally those which have been taken possession of by *Pagurus Barnhardus*.

It is one of the handsomest of the Nereids.

*Nepthys margaritacea*, Sars.

*N. Hombergii*, Sav. Common in wet places, buried among the sand. Some specimens are six to eight inches long, and as thick as the little finger. Besides the above species there are several small ones, which I have not yet examined minutely, but which are probably new to Britain.

### ARICIÆ.

*Nerine vulgaris*, Johns.

I am doubtful whether my specimens belong to this species.

*N. coniocephala*, Johns.

Common in damp hollows about mid-tide, along with *Arenicola*. It occupies a friable tube, descending a foot or more below the surface.

*Spio seticornis*, Bast. }
*S. crenaticornis*, Mont. } (*Leucodore ciliatus*, Johns.) These seem to me to be forms of one species, sometimes excavating a burrow between the laminæ of old shells, at others constructing a sandy tube.

*S. quadricornis*, Lam. Very common below high-water mark, forming a slender cylindrical sand tube; it has four tentacles, the two lower shorter, and the anal segment terminates in four ovate styles. *Branchiarius quadrangularis*, Mont., seems identical with this worm, but, as frequently happens, the specimen had lost the anterior segments.

*Ophelia coarctata*, M. Edwards.

One specimen only met with. New to Britain.

*Mæa mirabilis*, Johns.

Frequent near low-water mark, in wet places where the sand is intermingled with mud. It bears a close superficial resemblance to the smaller Nemertoid worms, Astemma, &c., and has the same white colour and elastic texture. Dr. Baird informs me there is one specimen in the British Museum, from the coast of Fife. Like myself, he failed to identify it with any known form, and I had named it provisionally *Rhynophylla bitentaculata*; but, since this paper was in type, he advises me that it is probably identical with the worm described in Dr. Johnston's Catalogue, at p. 278, as *Mæa mirabilis*.

As I have not been able to compare it with the description, and it may prove distinct, I append the notes I had drawn up from the examination of living specimens.

Prestomial segment leaf-like, ovate, broader than the body, strengthened in the centre by five ribs, ciliated below, margin mobile undulated reticulate. Proboscis cordate, retractile, tumid, shorter than the upper lip, from the lower margin of which spring two long flexible trigonous tentaculæ, clothed throughout the inner surface by four to six rows of conical papillæ, resembling the suckers of Asterinæ. Eyes and antennæ 0.

Its hold on the sand is so firm that specimens are seldom obtained entire. When creeping through the sand the thin mobile upper lip acts as a wedge, and the turbinate soft probosis is rapidly protruded like a bladder, enlarging the opening. When the surface is reached the head is partially withdrawn, and the two papillose tentacular cirri are directed upwards.

Body three to six inches in length, white, opalescent, as thick as a crow quill; segments very numerous, quadrangular, slightly winged, as broad as long, the upper eight a little narrower, especially at the base, where it joins the lower portion. In addition to the small transparent branchial laminæ on the ventral surface of each of the upper segments, there is an oval appressed scale. Intestine simple, containing sand and mud. Numerous ova are found at the lateral margins of each ring after the eighth. The setæ of the eighth segment are very numerous and delicate, resembling in form the pendulum of a clock, while those of the lower rings are stronger, shaped like golf sticks.

This worm shows no disposition to swim in water, but remains in one place, with the leaf-like snout curved upwards. The blood is colourless or nearly so, and I could make out no circulation as in many worms, but the margin of the snout is covered with a delicate net-work of vessels, and the tentacular papillæ are each supplied with a vascular loop. These papillæ are depressed at the apex, and supplied with muscular fibres like true suckers, but I have never seen them used to seize any object, or aid in progression.

*Arenicola piscatorum*, Lam. Very abundant.

## LUMBRICINÆ.

*Lumbricus lineatus*, Müll.

Very rare, among surface mud.

*L. capitatus*, Johns.

Only one specimen, constricted below the snout.

*L. pellucidus*, Temp. (Clitelis minutus, Temp.)

Found within putrid specimens of the heart urchin, and among other rejectamenta of the tide.

## CAPITIBRANCHIATA.

*Pectinaria belgica*. Lam.

Empty tubes common, living worms occasionally found near low-water mark.

*Sabellaria Anglica,* Grube, (*S. alveolata,* Sav.)
Very common within shells, especially the whelk.

*S. Crassissima,* Lam. Rare.

*Terebella conchilega,* Pall.
Frequent near low water.

*T. chrysodon* Mont.
*T. nebulosa,* Mont.
*T. constrictor,* Mont.
Attached to shells, &c.—not rare.

I have found two specimens of a minute species with only eight tentacles, among tubes of *sabellaria,* perhaps the young of some larger form, they resemble *T. ostreata,* Dalyell.

*Ops,* gen. nov. (*One of the names of Cybele.*)

Tube slender, strong but flexible when moist, coated with minute, closely imbricated fragments of shell, attached edgewise.

Worm terete, of equal breadth throughout ($\frac{1}{16}$ inch). Branchial fans two, terminal, very short, composed of soft, thick, pectinated processes, the apices bifid, obtuse, incurved, surrounding the mouth like a star, not ciliated. Between the fans is a small scoop-like lip.

Rings distant, the upper one contracted at the apex, with lateral tufts of setæ, much shorter than the succeeding segments, which are six to eight times longer than broad.

Ventral surface channelled, on each side of it are pencils of slender setæ: and surrounding all but the ventral aspect of each ring, a *narrow rough band like a rasp,* the surface studded with conical papillæ. No lateral hooks.

Anal rings narrower, ending in an obtuse point.

*O. digitata,* sp. nov. Frequent opposite the Whitworth guns, near low tide mark, accompanying species of *Terebella.*

Tube three to four inches long, tapering a little at each end, which is open, as thick as a crow-quill. It has a very neat appearance from the uniform size of the shell fragments. Intestine simple, undulating, filled with mud and sand. Peristome not longer than broad, when closed conical, the flat, obtuse, fleshy segments curling inwards over the mouth. Colour pinkish. There is a blood vessel on each side of the intestine, but I could trace no circulation in the pectinated fans, which seemed to be used by the worm to collect the grains of sand, &c. The rasp-like collars, surrounding each ring, are very curious.

There is an animal figured by Dalyell, *Powers of the Creator*, &c., Vol. II., Pl. xxxv. f. 4, 5, under the name of *clymene borealis*, which may be identical with our species, but the peristome is said to consist of 16 to 24 teeth, which are figured as simple and recurved, "forming a shallow funnel."

This Annelid is simple in structure and very sluggish, and it bears more resemblance to the *Sipunculidæ* than to the higher worms. In the peristome it reminds us of *Chirodota digitata*, Esch. From *Orthonia* it differs in the rasp-like band which surrounds each segment.

*Sabella ventilabrum*. Rare, cast up from deep water after storms.

| | |
|---|---|
| *Serpula triguetra*, L. | Not uncommon on shells. |
| *S. contortuplicata*, L. | |

| | |
|---|---|
| *Spinorbis communis*, Flem. | Attached to sea-weed, sometimes abundant. |
| *S. lucidus*, Mont. | |
| *S. minuta*, Mont. | |

ECCLES, APRIL 2, 1865.

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## April 24th, 1865.

THOMAS ALCOCK, M.D., in the Chair.

### *Donations.*

Captain Mitchell of Madras, three slides of Indian Diatomaceæ, and contents of seers fishes' stomachs almost entirely consisting of Diatomaceæ.

Dr. ALCOCK read a communication on "Southport Natural

History." He showed specimens of Tellina donacina, Modiola tulipa and Fissurella reticulata found on the shore, also of the large variety of the Solen ensis, some distorted shells of Mactra stultorum and Syndosmya alba. Fissurella reticulata had not hitherto been recorded as a Southport shell. He made some observations on the Natterjack Toads, which were spawning in marshy places among the sand hills, (April 18th to 22nd,) exhibiting males and females living, and spawn preserved in glycerine.

Mr. R. D. DARBISHIRE mentioned the occurrence of the Old English or black rat in Messrs. Whitbread and Co's brewery in Chigwell street, London, and exhibited stuffed specimens of male, female, and young.

Mr. G. E. HUNT exhibited specimens of a Campylopus allied to C. setifolius, or possibly a new species.

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## Annual Meeting. May 15th, 1865.

Mr. J. SIDEBOTHAM, President, in the Chair.

The following officers for the ensuing session were duly elected:—

### President

ARTHUR G. LATHAM.

### Vice-Presidents.

JOSEPH SIDEBOTHAM.
R. D. DARBISHIRE, B.A., F.G.S.,
JOHN D. DANCER, F.R.A.S.

### Treasurer.

THOMAS H. NEVILL.

### Secretaries.

H. A. HURST.
THOMAS ALCOCK, M.D.

### Of the Council.

JOSEPH BAXENDELL, F.R.A.S.
JOHN PARRY.
W. H. HEYS.
W. C. WILLIAMSON, F.R.S., &c.
J. G. LYNDE.
J. WATSON.
G. H. GRINDON.
THOMAS COWARD.

### *Donation.*

Dr. Alcock presented to the Section 24 slides of mounted Foraminifera from shore sand, coast of Galway.

A fine stuffed specimen of Osprey, shot at Rostherne Mere, was exhibited by Mr. Harrison.

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## PHOTOGRAPHIC SECTION.

April 6th, 1865.

H. E. Roscoe, B.A., Ph.D., F.R.S., F.C.S., Vice-President of the Section, in the Chair.

Mr. J. **Sidebotham** said that two of the chief points in his paper, read on the 2nd February, were, that lenses either very short or very long in focus did not give true transcripts of nature; in the long focus the objects in the distance looking too near, and in the short focus too distant; and that lenses of from seven to ten inches focus gave correct perspective If large pictures were wanted they should be enlarged from negatives taken with a short focus lens. The second point was, that in all photographs there should be introduced some standard of measure, so that the focus of the lens being known, objects could be approximately measured.

Mr. J. B. **Dancer** remarked that in comparing pictures taken by short and long focal lenses for the purpose of showing the errors of perspective, it would be desirable to take into account the angle included in the field of view, the diameter of the stop, and the errors arising from the aberration in the lenses employed. In order to obtain a view of photographic pictures which should approach as nearly as possible to the view seen by the eye, they should be looked at through lenses of corresponding foci to the lenses with which they were taken.

Mr. JAMES MUDD read a paper entitled "A Photographer's Dream," in which he pointed out that a good picture is not due solely to the process or the apparatus used, but that more was dependent on the careful study of the laws of art, without which it was impossible, except by accident, to produce an artistic picture.

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## Annual Meeting, May 18th, 1865.

JOSEPH BAXENDELL, F.R.A.S., in the Chair.

The SECRETARY read the annual report, congratulating the members on the success of their first session. The various Papers read at the different meetings were referred to; the funds were also stated to be in a satisfactory condition.

The report was unanimously adopted.

The following gentlemen were elected to hold office during the ensuing year:—

### President.

THE RIGHT REV. THE LORD BISHOP OF MANCHESTER.

### Vice-Presidents.

J. P. JOULE, LL.D., F.R.S., &c.
H. E. ROSCOE, B.A., Ph.D., F.R.S., F.C.S.
JOSEPH BAXENDELL, F.R.A.S.

### Treasurer.

THOMAS H. NEVILL.

Secretary.
LESLIE J. MONTEFIORE.

Council.
J. B. DANCER, F.R.A.S.
E. C. BUXTON.
JOHN PARRY.
JOHN ROGERSON.
JOSEPH SIDEBOTHAM.
W. C. WILLIAMSON, F.R.S., &c.

Mr. GEORGE WARDLEY read a paper on "Glass Transparencies," in the course of which he detailed the various processes generally in use for printing glass transparencies, dwelling especially on the modification of the taupenot, in which, after coating the plate with collodion, instead of immersing it in the sensitive bath, it is thoroughly washed with water to get rid of the ether and alcohol, and is then coated with iodised albumen; the remainder of the process is then exactly as the taupenot. The tannin process answered well, but many prints were spoiled in consequence of halation. The best preservatives for printing transparencies were a mixture of tannin and honey, or tannin and raspberry syrup, the former giving brown and the latter black tones. The addition of the raspberry syrup to the tannin yielded pictures entirely free from halation.

Dr. J. P. JOULE, F.R.S., described some further improvements in his camera, by means of which glass only came in contact with the silver solution.

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The following paper was read at a meeting of the Microscopical Section, held November 28th, 1864—see page 52:—

"Notes on Natural History Specimens lately received from Connemara," by THOMAS ALCOCK, M.D.

The series of specimens which I have now to lay before you is so extensive, and I believe so interesting, that parts of it might properly form the material for several distinct communications; but at present I propose to show them as a whole, and, with the specimens, to hand in as complete lists as I can of the species in each class.

The richness of the coast of Galway is well known to every student of British Marine Zoology; for to whatever branch of the subject he devotes himself he finds alike that here some of his rarest treasures are to be obtained. It is not with the hope of making known to you much that is new that I am led to introduce this subject to your notice, but chiefly because I am convinced that natural history work amongst ourselves is best promoted by the formation of exact lists of the species which we actually know to have been found at some particular localities; and such lists of Connemara specimens, imperfect as they must necessarily be at first, I have now to lay before you. It is however no more than might be expected that in the course of careful examinations of so many objects some points have occurred to me which I think worthy of notice, and these I shall mention as I come to them in their natural order.

In the first place I have to show specimens of three species of Nullipore—namely, N. polymorpha, N. calcarea, and N. fasciculata; also N. calcarea var. depressa.

The list of *Foraminifera* is an extensive one, especially considering that all my specimens are from shore sand and from one locality. This sand is from Dogs Bay, Roundstone, and consists of many kinds of small shells of Mollusca, among which Rissoæ and Lacunæ are most noticeable at first sight, fragments of Lepraliæ and other Zoophytes, spines of Amphidotus, and sponge spicula, while the finer parts are made up entirely of Foraminifera. Of these I have found 58 species and named varieties, and also six very distinct forms which are not mentioned in Professor Williamson's Monograph on Recent British Foraminifera. Specimens of these, and of all the other forms contained in the list are mounted for inspection.

In the course of my frequent examinations of these objects

I have made a few observations on several of them, which may perhaps be interesting.

I find Orbulina universa common in the Dogs Bay sand; that is, I have picked out some hundreds of specimens. They vary greatly in size, the largest being four or five times the diameter of the smallest. They have the surface frosted with larger and smaller tubercles, arranged with a certain kind of regularity, but, though thus rough externally, the texture of the shell does not appear to be arenaceous as stated by Professor Williamson,—at least, if by that term is meant that it is formed of agglutinated grains of sand as is the case with some other species. When examined with a high power and transmitted light, the larger and smaller tubercles show black from their density and the spaces between them are partly occupied by objects like very transparent thin plates, of a uniform size and an imperfectly squared figure, the impression these convey being that they have been produced by a kind of crystallization of the material of the shell at the time of its original formation. I conclude that the colourless condition of my specimens depends on the perfect manner in which all animal matter has disappeared, and I think for an examination of the mere structure of the outer case this must be an advantage. It may be interesting to note that among the specimens are a few with one or more protuberances of parts of their surface, destroying the regular spherical figure, and indicating an incipient budding before the shell hardened; there is also one large and very handsome double specimen.

Besides the Orbulinas I have an example of another kind of spherical object, which for convenience I will mention here, though I do not suppose it to belong to the Foraminifera at all. It looks like a sphere of the most transparent glass, and is without colour or markings of any kind.

I have found all the forms of Lagena, excepting L. vulgaris typica and L. gracilis. Lagena striata and interrupta are abundant; and these, with very few exceptions, have the costæ passing forward to the extremity of the neck, in which case it is only one half of the whole number which do so, each alternate one stopping short at the base. Specimens where the costæ wind spirally around the neck are equally common with those in which they take a straight course. These Lagenæ have the appearance of old coarse shells, but they do not seem to have suffered from attrition; they are scarcely ever found with the neck broken short, though it may perhaps be almost equally rare to meet with one absolutely perfect. The varieties—clavata, perlucida, semistriata, and substriata, are comparatively rare, and all of them have forms and characters very distinct from striata and interrupta, while the two latter agree perfectly excepting in the matter of the costæ, which are found in different specimens to be interrupted in a great variety of ways, those with the costæ perfectly continuous being the least common; so that the conclusion I am inclined to come to is, that they need not be separated even as varieties, and that, whatever doubts may remain as to some of the other named varieties, the great abundance of these two and the constancy of their general characters make it certain that together they will form a good species under the name of Lagena striata. A few specimens of this species have a mucro at the base, and deformed ones are not uncommon; these, besides having the body variously mis-shapen, often have the neck bent, sometimes even so much as to give the specimen the form of a retort.

The Dogs Bay sand contains many forms of Entosolenia, some of them agreeing with those described by Professor Williamson, but others distinct; and of these latter I have ventured to name two, which may be described as follows:— 1. *Entosolenia Williamsoni*, a very abundant form, might pass at first sight for Lagena striata with the neck broken away, but a close examination shows it is a perfect shell, the body like L. striata but rather less full in proportion to its length than is usual in Connemara specimens, and the texture a little more glassy; its chief peculiarity however is in the neck, which is short and formed of two distinct portions, the first directly continuous with the body and having an outline similar to that of the lower part of the neck of Lagena abruptly cut short, and the second a cylindrical tube of comparatively small diameter continued from the middle of it. The first portion is ornamented with three circles of hexagonal reticulations, which are continuous below by their inferior angles with the longitudinal costæ of the body, and present an interesting combination of the superficial characters of E. costata and E. squamosa.—2. *Entosolenia Montagui* is a squamous form, but differs from the named varieties of E. squamosa in having its surface really covered with a pattern like scales instead of with raised reticulations. Well developed specimens are not at all flattened, though many are found as if crushed, and they then present an appearance resembling a dried fig.; the true shape however is a perfect oval, full and well rounded at the smaller end, and from the middle of this projects a short smooth cylindrical tube. With a low power of the microscope, the whole surface of the body appears to be made up of small almost square facets arranged in distinct longitudinal rows, but when these are more highly magnified each flattened surface is seen to rise a little anteriorly, and to have the front border rounded so as to give exactly the appearance of a covering of scales.

So far as I have yet seen the forms of Dentalina and Cristellaria are very rare in this sand, Nonionina Jeffreysii and elegans are also scarce, but Patellina corrugata, which is described as a rare species, is not very uncommon, and some remarkably fine specimens have been met with. All the forms of Rotalina occur excepting two, and there are several undescribed ones in addition; at present I have seen only one specimen of the rare species, R. inflata. There are two distinct varieties of Globigerina, one with the chambers globular, the other having them considerably flattened, which gives quite a different character to the shell. Truncatulina lobata is by far the most abundant species, and with Miliolina seminulum, constitutes the chief bulk of the the sand. The two forms of Cassidulina are equally common, and specimens have not been met with presenting intermediate links. Polymorphina lactea occurs in profusion, and, though the forms which are distinguished as typica, oblonga, and communis are well marked, a considerable proportion of the whole number of specimens collected seem to indicate an absence of any definite plan in the arrangement of the segments, the chambers being evidently thrown together without order, and in some cases producing an irregular nodulated mass, with two, three, or more distinct and perfectly formed open mouths on different parts of the surface. I find also specimens consisting of nothing more than the primordial segment, and these might be mistaken for a form of Entosolenia globosa but for the peculiar texture of the shell and the radiating grooves around the mouth; they are worthy, I think, of particular notice, as possibly capable of furnishing some more reliable marks of distinction than are found in adult shells, though at present all I have seen are of one character.

The forms of Textularia are numerous, and among them are four which can readily be separated, but may still pass for varieties of T. cuneiformis; one of them however differs considerably in having the texture of the shell much finer, and the chambers full and rounded. Textularia conica is abundant, and its character, in these Connemara specimens, is so distinct from T. cuneiformis that it seems impossible to admit it as only a variety of that species. In many of the specimens the apex of the cone is broken, exposing always three chambers, which are arranged like a trefoil and are placed almost on the same plane.

An examination of the specimens before you of the two forms of Biloculina, named respectively in Professor Williamson's Work,—B. ringens, typica and B. ringens, var. carinata, will suggest, I think, a doubt as to whether it is correct to throw them together as one species, the texture of the shells as well as the form of their mouths being very different.

All the named varieties of Miliolina occur in abundance, and among them are great numbers of evidently distorted and misshapen specimens which appear to me to give no help whatever in the way of supplying inosculating forms, but may prove useful by indicating facts bearing on the general development of the animals. Specimens with the last chamber, not broken but clearly left incomplete, are by no means uncommon.

*Lepraliæ.*—The specimens of many species of Lepraliæ which I have to show are from dead shells picked up from the shore of Dogs Bay, and from others dredged at the mouth of Birterbuy Bay. These promise a very rich harvest, and indications of a great number of species are further given by the specimens of detached cells and fragments picked out from the shell-sand, though the latter are generally too broken to serve for exact examination or description. At present I have done very little with the specimens, but cannot altogether omit mentioning them. Lepralia figularis is common, and the specimens are very beautiful ones. There are also many examples of a species nearly agreeing, but not quite identical, with Johnston's Lepralia ovalis; and Lepralia ciliata var. β of Johnston, Mr. Hassall's Lepralia insignis, is plentiful. There are many other striking forms which I have not yet had the means to identify, besides common ones about which there is no doubt; but at present I must pass over the subject without attempting to give a list.

*Echinodermata.*—The present list includes ten species, and on the table are very handsome specimens of four of these, namely, Uraster glacialis and U. violacea, Luidia fragillissima and Echinus lividus. The acquisition of the two species of Uraster just mentioned, which were not before in the Museum collection, led me to compare them with Uraster rubens, and see if some more reliable distinctions might not be found than the slight differences of proportion and form on which Forbes lays the most stress. The result of this examination is that the strong spines with which the bodies of all three are beset offer at once a clear and simple means of distinction, their character in each species being decidedly different.

The spine of Uraster rubens is club-shaped, the base spreading, and irregularly nodulated, the short shaft cylindrical and smooth, and the head barrel-shaped and beset with longitudinal rows of thin triangular projecting points. That of U. violacea has the basal part rising in the form of a short wide cylinder, crowned with an irregular circle of short spinules, from the midst of which the spine is continued in a conical shape, and near its summit is armed with rows of triangular projecting points like those of U. rubens. While that of U. glacialis has the form of a large strong cone with a very wide base rising from a dense circular bed of small blunt spinules. The apex of the cone appears truncated, and its sides near the top are marked with a few slight serrations.

*Entomostraca.*—In the shell-sand from which the Foraminifera were obtained great numbers of the cases of entomostraca occur. Specimens have been picked out of Cythere albo-maculata, angustata, variabilis, flavida, convexa, impressa, pellucida, and quadridentata, the last mentioned species being rather common; and besides these I have to show mounted specimens of about thirty forms perfectly distinct from one another and from those above mentioned. At present, however, from want of knowledge of what has been recently done in this subject, I am obliged to pass them over.

*Crustacea.* The collection of crustacea before you forms in itself a very handsome result of Mr. R. D. Darbishire's one day's dredging in Birterbuy Bay, and among the specimens are some choice species, as Eurynome aspera, Ebalia Bryeri and Cranchii and Atelecyclus heterodon. These and indeed all the other specimens, form valuable additions to the Museum collection, which promises soon to become really useful for study and reference. The plan adopted of showing the specimens dried, mounted on glass and enclosed in glass-lidded boxes, has been found quite satisfactory, preserving them both from dust and damage by handling, while by properly displaying several specimens of a species in different positions every part may be clearly seen.

*Mollusca.*—The list of Mollusca which I have next to present contains 138 species, including those dredged in Birterbuy Bay; the others are all beach specimens, collected by Mr. Glover and Mr. Darbishire, or picked out since from the shell sand.

The fact that 16 species of small land shells have been found in considerable numbers in the sand, along with the sea shells and foraminifera, may be worth notice; more or less of such an admixture is, I believe, not uncommon, but in the present case the proportions appear to be unusual, and the very great abundance of Helix pulchella is certainly remarkable. It should be mentioned, too, that Mr. Glover has collected from the sandy strip of ground separating Dogs Bay from Gorteen Bay many dead bleached specimens of Helix nemoralis and a few of H. aspersa remarkable for the great thickness and weight of the shells, in consequence of a very excessive deposition of calcareous matter in their substance; no similar shells in the living state have been found in the neighbourhood, though carefully looked for.

As to the sea shells, I shall mention only a few that have particularly attracted my notice. Otina otis was found on a mass of small living mussels brought by Mr. Glover. Cæcum glabrum is very abundant in the sand, and I have picked out some scores of specimens of this shell in the young state with the spiral nucleus attached, besides great numbers of separate spirals; the intermediate length of tube between the young and the adult state increases rapidly in size, and has a decidedly conical shape, so that the whole length of the shell, if it were to remain entire, would not be so great as might be supposed from seeing only the young and the mature portions. Skenea rota occurs sparingly; I have seen about half-a-dozen specimens. The minute shells which have proved perhaps the most interesting are those of the fry of different species of limpet, four forms of which have been met with; they are exceedingly abundant in the shell sand, and three of the species have a distinct spiral cap surmounting the apex of the conical part. The series of mounted specimens shows very clearly the several stages of growth to the adult form, and it will be seen that an internal partition is formed by degrees until at last it completely divides the interior of the cone from that the larval cap, which then drops off, leaving a depressed oval scar near the apex. *Patella pellucida* is at once distinguished by its brilliant greenish-blue marks; it is smooth and has a semi-transparent horn colour, much like that of the adult, though it gives little promise of the elegant shape it afterwards grows to; the larval cap is large and its spiral character distinct. A second form has so much the appearance of *Patella vulgata* that I shall venture to call it so. The cap is smaller than in P. pellucida, and the conical shell is opaque and strong, and has radiating ribs and markings of colour resembling the adult P. vulgata; in this shell the scar left when the cap has fallen is much deeper and coarser than in P. pellucida. A third form, which is much the most abundant in the sand, has a smooth, delicate, colourless shell, sometimes quite transparent, but often partly or wholly opaque; the cap is always white and has the appearance of a drop of wax on the apex of the cone. In this species the conical shell from its first commencement appears to grow uniformly and regularly around the margin of the larval shell, and thus at once becomes a complete cone, while in P. pellucida the growth is at first entirely on one side, forming as it were a very large and expanded outer lip. I refer the present shell, though without positive proof, to *Acmæa virginea.* The fourth limpet-like shell is quite different from those already mentioned, it forms a more depressed cone, and is strong, with a translucent flinty texture, and the surface is marked with numerous fine, equal, radiating ribs. There is no deciduous cap, but the apex of the cone is a little flattened and presents a circular area, which is smooth and glassy, and appears as though a small transparent button were sunk into the substance of the shell.

The occurrence of the shells of fry of great numbers of species both of univalve and bivalve mollusca in the sample of sand from Dogs Bay which I am examining, has led me to attempt their identification with the view of obtaining information as to the characters of some of the larval, or more properly speaking fœtal, forms, which have not hitherto been observed. In this inquiry it is necessary to give a more definite meaning to the term "Fry" than has generally been done, for it appears to have been used indifferently for the fœtal shell as it comes from the egg, and for the young which has grown considerably beyond that condition. Shells of the latter kind still show the fœtal shell perfectly, and at the same time display characters in the aftergrowth by which at all events the genus, and in some cases the species may be determined, and it is specimens with these double characters that I shall distinguish as fry. At present I will leave the microscopic univalve shells without further notice, excepting to remark that, while as a rule all fœtal shells are smooth and glassy, there are among the univalves some striking exceptions, where the shell is beautifully sculptured, as in some exquisite little specimens apparently the fry of *Murex erinaceus*. The whole shell (fig. 1) is of a transparent horn brown colour, and is covered with raised reticulations most gracefully disposed; those on the nucleus are longitudinal and transverse, enclosing minute square depressions; while on the succeeding whorls they are diagonal in opposite directions, and form diamond-shaped areolæ.

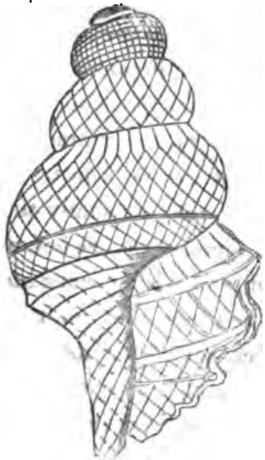

Fig. 1.

The bivalves have at present engaged most of my attention, and several of them will deserve a short notice. One of the most remarkable is a shell (fig. 2) which I refer to *Anomia*. It is transparent, and talclike in texture, and is very thin and delicate, so as to be almost always found more or less broken at the edges. Its substance is excavated by numerous tubes similar to those described by Dr. W. Carpenter in Anomia, and these tubes radiate from the edge of the nucleus in all directions to the margin of the shell. The nucleus itself is very glassy and transparent, and is full and rounded in shape, though still indistinctly triangular. But the singular feature in these specimens is that the growing shell, which completely surrounds the nucleus in the same way that it does in Ostrea, here leaves a small round hole on each side of the beak.—The fry of *Ostrea edulis* is common; the fœtal shell also occurs separate, and is readily distin-

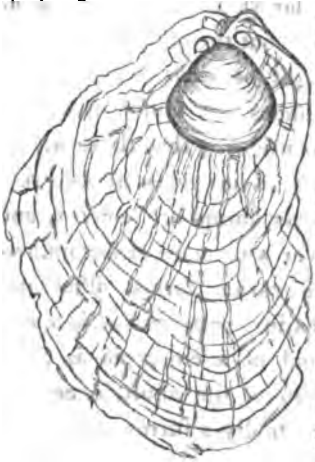

Fig. 2.

guished by having a notch about the middle of the ventral margin and by one of the valves being flatter than the other.—Shells having the form of *Pecten* are found in abundance, and about half a dozen kinds have been made out, though at present these cannot be referred to their species, the characters which distinguish them being of a different nature from those found in the adult shells; for, contrary to what is usually the case, the Pectens grow to a considerable microscopic size before the proper sculpture begins to be formed, and this intermediate growth resembles neither the fœtal shell nor the adult in structure. All the young Pectens which have been observed have a rounded shape like P. opercularis, and in all of them the nucleus is smooth, glassy, and transparent, with the valves ventricose and obscurely triangular, like those of Anomia. One form differs from all the others in having the intermediate growth entirely composed of cellular structure, which might lead to a suspicion that it is Avicula, but the general character is strictly that of Pecten. As might be expected from the conditions under which the specimens are found, none are double, and it may be worth mentioning that those with a cellular structure are flat, and, as far as has yet been observed, are all the same valve. The other forms are convex, and have the external surface pitted over with more or less numerous deeply excavated depressions of various forms, differently arranged so as to make so many distinct patterns.—*Lima subauriculata* is not uncommonly found of microscopic size, and it agrees with the characters given of the adult, excepting that the strong middle line is externally a furrow and internally a ridge, instead of the reverse. In this shell no distinct line of demarcation is seen between the fœtal shell and the aftergrowth. — *Crenella* presents an unusual case, as shown by the series of mounted specimens, in which the growth is seen to be continued to a considerable size on the plan of the original fœtal shell, and then suddenly changes to a totally different character, as has been already observed by Mr. Jeffreys (*Brit. Conch*, vol. ii. p. 132.)—*Arca tetragona* shows a remarkable nucleus, which differs from almost all others I have seen in being amber-coloured or light brown, and it has, scattered over its surface, a few marks which appear like short deeply-implanted hairs. Two species of *Cardium* have been distinguished, namely C. edule and C. echinatum; both have the fœtal shell smooth, transparent, and globular, but that of C. echinatum is considerably the larger. Several species of *Venus* have been observed, and in all of them the fœtal shell is small, globular, and glassy. The fry of *Tapes* is characteristic, and the form of it which I have found appears to agree with T. virginea. I must pass over many other shells about which there is more or less doubt, and will at present mention only one more, namely *Saxicava rugosa*, which is the most abundant of all the species in the Dogs Bay sand. The nucleus is very large, glassy, and transparent, and resembles a Cyclas in shape. The fry of this species shows very well how completely the characters of the adult shell are dormant in the fœtal state, and how immediately they are sometimes assumed with the first growth beyond that condition.

In conclusion I have only to say that I trust the results of further examinations of this very rich shore deposit will justify me in having introduced it to your notice, although at present my observations are so slight and imperfect; and I will further venture to express the hope that the large collection of specimens before you may tempt other members of the Section to visit the coast of Galway and make additions to the subjoined lists.

## LIST OF SPECIES FROM ROUNDSTONE.

## 1864.

### *Foraminifera.*

Orbulina universa.
Lagena vulgaris var. clavata.
„ var. perlucida.
Globigerina bulloides.
Planorbulina vulgaris.
Truncatulina lobata.

Lagena vulgaris var. semistriata.
,, var. striata.
,, var. interrupta.
,, var. substriata.
Entosolenia globosa var. lineata.
,, costata.
,, marginata.
,, ,, var. lucida.
,, Williamsoni (N.S.)
,, squamosa, typica.
,, ,, var. scalariformis.
,, ,, var. catenulata.
,, ,, hexagona.
,, Montagui (N.S.)
Nodosaria radicula.
,, Pyrula.
Nonionina Barleeana.
,, Jeffreysii.
,, elegans.
Polystomella crispa.
,, umbilicatula.
Patellina corrugata.
Rotalina Beccarii.
,, inflata.
,, oblonga.
,, concamerata.
,, mamilla.
,, nitida.

Bulimina pupoides, typica.
,, ,, var. marginata.
Uvigerina angulosa.
Cassidulina lævigata.
,, obtusa.
Polymorphina lactea, typica.
,, ,, oblonga.
,, ,, communis.
,, myristiformis.
Textularia cuneiformis, typica.
,, ,, var. conica.
,, variabilis.
,, ,, var. spathulata.
,, ,, var. difformis.
Biloculina ringens, typica.
,, ,, var. carinata.
Spiroloculina depressa, typica.
,, ,, var. rotundata.
,, ,, var. cymbium.
Miliolina trigonula.
,, seminulum.
,, ,, var. oblonga.
,, ,, var. disciformis.
,, bicornis, typica.
,, ,, var. angulata.
Spirilina foliacea.
,, perforata.

## *Echinodermata.*

Ophiura texturata.
Ophiocoma rosula.
Uraster glacialis.
,, violacea.
,, rubens
Asterina gibbosa.

Luidia fragillissima.
Echinus lividus.
Echinocyamus pusillus (in shell sand)
Amphidotus (spines in shell sand)

## *Crustacea.*

Stenorhynchus phalangium.
Inachus Dorsettensis.
Hyas coarctatus.
Eurynome aspera.
Zantho florida.

Ebalia Pennantii.
,, Bryeri.
,, Cranchii.
Atelecyclus heterodon.
Pagurus Bernhardus.

Zantho rivulosa.
Cancer pagurus.
Carcinus mænas.
Portunus puber.
„ corrugatus.
„ depurator.
„ pusillus.
Gonoplax angulata.

Pagurus Prideauxii.
„ Cuanensis.
„ lævis.
Porcellana platycheles.
„ longicornis.
Galathea squamifera.
Crangon vulgaris.
Palæmon serratus.
Ligia oceanica.

*Mollusca.*

Vitrina pellucida.
Zonites cellarius.
„ crystallinus.
Helix ericetorum.
„ lamellata.
„ fulva.
„ pulchella.
„ rotundata.
Pupa umbilicata.
„ muscorum.
„ Venetzii.
Clausilia nigricans.
Zua lubrica.
Limnæus truncatulus.
Conovulus bidentatus.
Carychium minimum.
Murex erinaceus.
Nassa reticulata.
„ incrassata.
„ pygmæa.
Purpura lapillus.
Mangelia nebula.
„ costata.
„ rufa.
„ linearis.
„ attenuata.
„ turricula.
Cypræa Europæa.
Lamellaria perspicua.
Otina otis.
Cerithiopsis tubercularis.

Trochus cinerarius.
„ magus.
„ Montagui.
„ umbilicatus.
„ zizyphinus.
Phasianella pullus.
Ianthina exigua.
„ communis.
Puncturella Noachina.
Fissurella reticulata.
Acmæa virginea.
Patella vulgata.
„ athletica.
„ pellucida.
Dentalium entale.
Cylichna cylindracea.
„ truncata.
Pleurobranchus plumula.
Spirialis Flemingii.
Anomia aculeata.
„ ephippium.
Pecten maximus.
„ opercularis.
„ varius.
Lima Loscombii.
„ subauriculata.
Mytilus edulis
Arca tetragona.
Pectunculus glycimeris.
Nucula nucleus.
Cardium echinatum.

Chemnitzia elegantissima.
Eulima distorta.
Cerithium reticulatum.
„ adversum.
Turritella communis.
Cœcum glabrum.
Scalaria communis.
Skenea planorbis.
„ rota.
Rissoa cingillus.
„ labiosa.
„ inconspicua.
„ parva.
„ punctura.
„ rubra.
„ striata.
„ ulvæ.
Lacuna vincta.
„ puteolus.
„ pallidula.
Litorina litoralis.
„ litorea.
„ neritoides.
„ rudis.
„ saxatilis.
Cardium fasciatum.
„ Norvegicum.
„ pygmæum.
Lucina borealis.
Kellia suborbicularis.
Turtonia minuta.
Montacuta ferruginosa.
Venus casina.
„ fasciata.
„ ovata.
„ striatula.
„ verrucosa.
Artemis exoleta.
„ lincta.
Tapes aurea.
„ virginea.
Lutraria elliptica.
Tellina donacina.
Syndosmya alba.
Saxicava rugosa.
„ „ var. arctica.
Lyonsia Norvegica.

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## ADDITIONAL LIST OF DREDGED SHELLS.

Gastrochœna modiolina.
Thracia convexa (valve.)
„ distorta (living.)
Solen siliqua.
Solecurtus candidus.
Psammobia vespertina.
„ ferroënsis.
Tellina crassa.
„ tenuis.
„ solidula.
Mactra solida.
Tapes decussata.
Circe minima (living.)
Lucina flexuosa.
Modiola modiolus.
Crenella marmorata.
Arca tetragona (living.)
Anomia patelliformis.
Chiton discrepans.
„ asellus.
„ cinerarius.
„ cancellatus.
Emarginula reticulata.
Trochus lineatus.
Rissoa Beanii.
Buccinum undatum.
Philine aperta.
„ catena.

# PROCEEDINGS

OF THE

# LITERARY AND PHILOSOPHICAL SOCIETY

OF

# MANCHESTER.

## VOL. V.

SESSION 1865–66.

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# NOTE.

The object which the Society have in view in publishing their Proceedings, is to give an immediate and succinct account of the scientific and other business transacted at their meetings, to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

# INDEX.

**AINSWORTH THOMAS.**—On a Predisposing Cause of Cattle Disease, p. 21.

**ALCOCK THOMAS**, M.D.—Questions regarding the Life History of the Foraminifera, suggested by Examinations of their Dead Shells, p. 15. Foraminifera from Dogs Bay, p. 99. On Foraminifera from a Shell of Halia Priamus, p. 123. Embryonic Shells of Mollusca, p. 166.

**BATES** Rev. J. C., M.A., F.R.A.S.—Results of Rain Gauge and Anemometer Observations made during the year 1865, at St. Martin's Parsonage, Castleton Moor, p. 168.

**BAXENDELL** J., F.R.A.S., Hon. Sec.—Auroral Phenomena, October 19 and 26, 1865, p. 15. On a Probable Cause of the Cattle Disease, p. 21. Note on the Variable Star S Delphini, p. 28. On Meteors, p. 59. Note on the Variable Star T Aquilæ, p. 88. Cattle Plague, p. 91. Storm Warnings in India, p. 101. Note on the Variable Star S Coronæ, p. 109. On the Determination of the Mean Form of the Light-Curve of a Variable Star, p. 112. On the Fall of Rain during the Different Hours of the Day, as deduced from a series of Observations made by the Rev. J. C. Bates, M.A., F.R.A.S., at St. Martin's Parsonage, Castleton Moor, p. 129. New Variable Star R Crateris, p. 191.

**BINNEY** E. W., F.R.S., F.G.S., V.P.—On the Difficulties of Working Deep Coal Mines, p. 12. On Fossil Wood found in calcareous nodules in the lower coal seams of Lancashire and Yorkshire, p. 61. On Fossil Wood in calcareous nodules found in the Upper Foot Coal, near Oldham, p. 113. On a singular Mineral found in a nodule of clay ironstone from the North Staffordshire coal field, p. 147. On the Humming-bird Hawk Moth, p. 161.

**BOTTOMLEY** J.—On the Probable Effect of the Exhaustion of the Coal Fields upon the Condition of the Atmosphere, p. 45.

**BROCKBANK** W.—Notes on a Section of Chat Moss, near Astley Station, p. 91. On the Liassic and Oolitic Iron Ores of Yorkshire and the East Midland Counties, p. 119.

Brothers A., F.R.A.S.—Photographs of the Eclipse of the Moon, Oct. 4, 1865, p. 3. On Celestial Photography, p. 68. On Mr. Rogerson's Method of Cleaning Glass Plates, p. 134. On an Experiment Illustrating the Appearances of Sun Spots, p. 171. On a Meteoric Body Crossing the Moon's Disc, p. 172. Note on the First Use of Hyposulphite of Soda in Photography, p. 181.

Clifton Prof. R. B., M.A., F.R.A.S.—An Attempt to Refer Some Phenomena Attending the Emission of Light to Mechanical Principles, p. 24.

Cockle Chief Justice, M.A., F.R.A.S., F.C.P.S.—On Coresolvents, p. 13.

Dancer J. B., F.R.A.S.—On the Eclipse of the Moon, Oct. 4, 1865, p. 9. Illumination of Opaque Objects under the High Powers of the Microscope, pp. 31, 42, and 55. On the Boulton and Watt Pictures, p. 134, 157.

Dickinson W. L.—Eclipse of the Sun, Oct. 19, 1865, p. 2. Eclipse of the Sun, Oct. 8, 1866, and Occultations of Aldebaran, Sept. 28 and Nov. 22, 1866, p. 170.

Dyer J. C., V.P.—Breaking of the Atlantic Cable, p. 1. Notes on the Origin of Several Mechanical Inventions, and their Subsequent Application to Different Purposes, Part I., p. 5; Part II., p. 48; Part III., p. 102. Notes on Cotton Spinning Machinery, Part II., Roving Frames, p. 148.

Greaves George, M.R.C.S.—On the Internal Heat of the Earth as a Source of Motive Power, p. 1.

Hardy Mr.—On a Large Cetacean Vertebra found in the Valley of the Don, p. 32.

Heelis Thomas, F.R.A.S.—On a Coal Basin between Mount Olympus and the Bay of Oraniska, p. 13. On Meteors, p. 58.

Herschel Sir J. F. W., Bart., M.A., D.C.L., F.R.S., &c.—On a Method of Cooling the Workings of Deep Coal Mines, p. 11.

Hull E., F.G.S.—The Raised Beach on the Coast of Cantyre, p. 13. On the Liassic and Oolitic Iron Ores of Yorkshire and the East Midland Counties, p. 119.

Hurst H. A.—On late Improvements in Illuminating Opaque Objects under the Higher Powers of the Microscope, p. 64.

Jevons W. S., M.A.—On a Logical Abacus, p. 161.

Johnson J. R.—On the Pantascopic Camera, p. 135.

**Joule** J. P., LL.D., F.R.S., V.P.—Camera for Outdoor Work without a Tent, p. 4. Effect of the Aurora of Oct. 19, 1865, on his Magnetic Needle, p. 15. Photographs of the Sun, p. 85. On a New Balance, pp. 145, 165.

**Knott George**, F.R.A.S.—On the November Meteors, as observed at Woodcroft, Cuckfield, Sussex, Nov. 12–13, 1865, p. 56. Light-curve of R Vulpeculæ, p. 59. Results of Observations of T Aquilæ, p. 90. On the Variable Star R Vulpeculæ, p. 124. Results of a Comparison of the Magnitudes of the Bedford Catalogue with those of the Mensuræ Micrometricæ and the Bonner Sternverzeichniss, p. 187.

**Latham Arthur** G.—On Two Beetles nearly Allied to Rhynchophorus Palmarum, p. 193.

**Linton James**.—On the Humming-bird Hawk Moth, p. 105.

**Mackereth** T., F.R.A.S.—Rainfall at Eccles for 1864, p. 10. Results of Rain Gauge and Anemometer Observations at Eccles during the year 1865, p. 107.

**Nasmyth James**, C.E.—On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes, Part. I., p. 181.

**Parry John**.—On Collecting Foraminifera on the West Coast of Ireland, p. 42.

**Roberts** W., M.D.—Injurious Effects produced by burning Pharaoh's Serpents in Close Rooms, p. 33.

**Roscoe** Prof. H. E., F.R.S., Hon. Sec.—Enlarged Photograph of the Moon by Mr. Rutherford, p. 1. Injurious Effects of inhaling Mercury Vapour, p. 33. Meteorological Registration of the Chemical Action of Daylight, p. 96. On the Didymium Spectrum, p. 147.

**Sidebotham** J.—Notes on Atlantic Soundings, p. 18. Notes on Acherontia atropos, p. 19. Discovery of Apion ononis, a species of Curculio, in the Isle of Anglesey, p. 20. On the Application of Measuring Rods to Photographic Pictures, p. 67. Cement for Use in Mounting Fluid Preparations of Objects for the Microscope, p. 98. On the Boulton and Watt Pictures, p. 134, 150.

**Smith R. Angus**, F.R.S., P.—Effect of Dews and Fogs in producing Epidemics, p. 22. Abrasion of Iron Rails on Railways, p. 23. On Air from off the Atlantic, and from some London Law Courts, p. 115.

**Sonstadt E.**—Note on the Purification of Platinum, p. 117.

**Thorpe T. E.**—On the Amount of Carbonic Acid contained in the Air above the Irish Sea, p. 33.

**Vernon G. V., F.R.A.S., M.B.M.S.**—Remarks on the Barometric Disturbances during the Months of October, November, and December, 1865, p. 85. Rainfall for 1865, p. 87.

---

*Meetings of the Physical and Mathematical Section.*—Annual, p. 124. Ordinary, pp. 9, 28, 56, 85, 107, 168, 187.

*Meetings of the Microscopical and Natural History Sections.*—Annual, p. 193. Ordinary, pp. 18, 42, 64, 98, 105, 122, 166.

*Meetings of the Photographical Section.*—Annual, p. 192. Ordinary, pp. 3, 67, 96, 134, 181.

*Report of the Council.*—April 17, 1866, p. 173.

# PROCEEDINGS

OF

# THE LITERARY AND PHILOSOPHICAL SOCIETY.

## Ordinary Meeting, October 3rd, 1865.

Edward Schunck, Ph.D., F.R.S., &c., Vice-President, in the Chair.

Dr. Roscoe exhibited an enlarged photograph of the moon, 21 inches in diameter, taken by Mr. L. M. Rutherford, of New York, on the 6th of March last. Mr. Brothers, Mr. Vernon, and other members, pronounced it to be the best lunar photograph they had seen, and decidedly superior to any yet produced in this country.

Mr. Dyer, referring to the breaking of the Atlantic Cable, expressed his surprise that no apparatus had been provided to seize and secure the end of the cable when the rupture took place, as contrivances for a similar purpose were in use in almost every cotton mill.

A paper was read "On the Internal Heat of the Earth as a Source of Motive Power," by Mr. George Greaves, M.R.C.S.

It has been very generally admitted that coal will not cease to be furnished because of the exhaustion of the stores of the mineral now existing in the coal measures; and further, that the obstacles to the continued working of the mines will not be engineering difficulties. The increased depth from which the coal will have to be brought may add to the cost, but at that increased cost it will still be for a long time obtainable. The author considered the real insurmountable obstacle to be the high temperature of the lower portions of the carboniferous strata. That temperature had been shown to be at a depth of 4,000 feet at least 120° Fahr., a degree of heat in which human beings cannot exist for any length of time, much less use any exertion. It had occurred to the author to inquire whether the very agency which will prevent the continued supply of fossil fuel might not be made the means of rendering that supply unnecessary—whether, in short, the internal heat of the earth might not to some extent be utilised. One or two modes of doing this had presented themselves to his mind. One of these might, he conceived, be the direct production of steam power by bringing a supply of water from the surface in contact with the heated strata by means of artesian borings or otherwise.

Mr. W. L. Dickinson read the following note on the eclipse of the sun which will take place on the 19th instant:

Calculation for Manchester (Royal Infirmary), Lat. N. 53° 29′, Long. W. 2° 14′.

At Manchester a partial eclipse is partly visible, and

| | h. | m. | s. |
|---|---|---|---|
| Begins ...........October 19th— | 4 | 8 | 12 |
| Greatest phase ...October 19th— | 5 | 4 | 2 |

Greenwich mean time.

At Manchester the sun will set at 5h. 2m. Greenwich mean time.

Magnitude of the eclipse (sun's diameter = 1) 0·293, on the sun's southern limb.

Angle, from north pole, of first contact 120° } towards the
Angle, from vertex, of first contact 153° } west
for *direct* image.

The situation of the point of first contact may be familiarly illustrated in the following manner. If we suppose a Victoria shilling to represent the sun, the moon will appear first to touch it on the right side near the bottom, at the letter D.

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## PHOTOGRAPHICAL SECTION.

October 5th, 1865.

J. P. Joule, LL.D., F.R.S., Vice-President of the Section, in the Chair.

Messrs. Coote and Rogerson exhibited a series of very fine pantascopic photographs of scenes in Switzerland, taken by M. Adolphe Braun, of Dornach. Each picture was about twenty-one inches in length, and the angle of view was stated to be one hundred and twenty degrees.

Mr. A. Brothers, F.R.A.S., exhibited an interesting series of photographs, taken during the eclipse of the moon, on the evening of Wednesday, October 4th. Commencing at 8.45, when the moon was nearly full, the negatives, twenty in number, were taken at intervals of about 12 minutes until 12.45, and they show the progress of the eclipse throughout. The effect of the penumbral shadow of the earth is distinctly visible on the negative taken at 9.15, and also on the one taken at 12.32. An attempt was made during the middle of the eclipse to obtain the photographic image of the entire surface of the moon; but it was found that the portion covered by the earth's shadow had no effect on the plate after an exposure of 15 seconds, although distinctly visible in the telescope. It was noticed that the southern limb of the moon showed the copper-coloured tint often seen during total lunar eclipses, and to this cause may be attributed the non-actinic effect on the sensitized plate. An exposure of about one or two tenths of a second gave the fully illuminated surface of the moon perfectly, but the parts covered by the penumbra were not defined, while an exposure of three seconds gave the outline of the earth's shadow with great distinctness, and an exposure of two seconds brought out some of the detail within the penumbra. Some of the negatives were obtained almost instantaneously.

The telescope with which these pictures of the moon were taken is an equatorial of 5 inches aperture and 6 feet focal length, driven by clockwork. This telescope gives the image of the moon about $\frac{11}{16}$ths of an inch in diameter, but by using a Barlow's lens this size is increased to $1\frac{1}{4}$ inch, and with this addition the eighteenth negative of the series was obtained in two seconds.

Dr. **Joule**, F.R.S., exhibited and explained the construction of a camera which he had contrived for outdoor work without a tent.

In this camera the operation was carried on by the successive introduction of the sensitizing and developing baths, the mode of the application of the baths being similar to that already described by the author. By a special arrangement the holders of the plate are preserved from contact with the developing solution.

## Ordinary Meeting, October 17th, 1865.

E. W. **Binney**, F.R.S., F.G.S., &c., Vice-President, in the Chair.

Mr. **Forrest** stated that he had in his possession a very extensive collection of Shakespeare memorials, which he would gladly allow any member of the Society to inspect.

A paper was read entitled "Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes," by J. C. **Dyer**, V.P.

### 1st.—*Lace Making Machine.*

The bobbin net trade at Nottingham had been carried on by women working on cushions or lace frames until about the beginning of this century, when the process was superseded by the lace making machine, invented by the late Mr. John Heathcoat, M.P. Mr. Heathcoat commenced his experiments by stretching common packing threads across his room for the *warp*, and then passing, by common plyers, the weft threads between the cords, delivering them into other plyers on the opposite side, and then, after giving them a sideways motion, repassing the threads back between the next adjoining cords, and thus effecting the intersecting or tying of the meshes in the same way as they were formed on the cushions worked by hand. His next step was to provide thin metallic discs to be used as "bobbins" for conducting the thread back and forth through the warp. These discs being arranged in carrier frames, placed on each side of the warps, were moved by suitable machinery so as to conduct the threads from side to side to form the lace. The limits of this abstract do not allow any detailed description of Mr. Heathcoat's beautiful invention; its effect, however, in superseding the hand work appears to have led to the Nottingham riots and the lace frame breaking, which took place about fifty years ago, when Mr. Heathcoat removed to Tiverton in Devonshire, where the patent lace making was re-established, and has been since carried on upon a large scale, and has thus proved an eminently successful invention.

The new principle of action conceived by Mr. Heathcoat was that of passing the threads of the weft through those of the warp and delivering them into conductors on the other side, to be repassed and delivered into the former conductors under mechanical control in place of hand working. He succeeded in working out this principle with marvellous perseverance and success, and this original conception of Mr. Heathcoat opened a new vista to other eminent mechanics, which led to the invention of many other valuable machines for widely different purposes, among which may be named that for passing back and forth the threads through fabrics for embroidering, as now exhibited in the beautiful machines for embroidering or ornamenting fabrics in the works of Messrs. Houldsworth. On the same principle is also based the cotton combing process now employed for separating the short and coarse from the long staples in fine cotton carding.

## *2nd.—Wire Card Making Machine.*

About the beginning of this century Mr. Amos Whitimore, of Boston, commenced his experiments for making cards by machinery. His first step was to examine the movements required to form and set card teeth for hand carding. He was for a long time engaged with trial machines, and ultimately succeeded in performing the operations for making and setting the card teeth by movements effected by eccentrics on a driving shaft, viz. (1) feeding the wire, (2) holding it, (3) cutting off and (4) bending the wires into staples, (5) piercing holes in the leather, (6) passing the staples through it, (7) pressing their crowns to the sheet, (8) crooking the teeth to the knee bend, and (9) advancing the leather to receive the next row of teeth. These complex and curious motions were effected by a series of cams or eccentric pieces fixed on the shaft and turned by a winch; therefore the invention of making wire cards by machinery was accomplished by Amos Whitimore, but it was many years thereafter before his machine could be made profitable in competing with the hand card makers.

The final development of this invention is explained at large in the paper, but cannot be given in this abstract. In this machine, as in that of Mr. Heathcoat's, a new principle of action was adopted to produce and govern the movements for making wire cards, viz., that of eccentric curves revolving on a driving shaft and guiding the motions of the traversing parts of the machines in their due order of succession for making and setting the card teeth as before stated. This application of curvilinear projections or cam pieces has since been extensively employed for giving intricate motions in many other machines invented during the last fifty years. Whence it appears that both Mr. Heathcoat and Mr. Whitimore became pioneers and guides to other able mechanicians in their several labours for the advance of mechanical science.

Among the inventions based upon that of the carding machine may be mentioned the machine for making the eyes or shanks of metal buttons, the machine for making wire reeds for weaving, and that for making pins. But without dwelling on other instances, it will suffice to say that the success of the lace machine and that of the wire card machine serve to spread the seeds of knowledge in practical mechanics, the germs of which, half a century ago, were widely cast forth from the fertile geniuses of John Heathcoat and Amos Whitimore.

### *3rd.—Cutting Furs from Pelt.*

In the year 1810 a model fur cutting machine was sent to me in London by a company in Boston, to be patented in England. It was stated to be the invention of a Mr. Bellows, who was unknown to me. The machine was adapted for shearing fibres from surfaces by the action of spiral cutters revolving and acting against a fixed straight cutter, so as to shear or cut fibres from the surfaces to which they are attached. I had a machine made and put into operation at a hat manufactory in the Borough; but the workpeople opposed its being used, which discouraged further attempts to bring it into use in that trade. The principle of it, however, was soon after patented for chopping straw, roots, &c., for which it was found valuable. Two or three patents were afterwards taken out for shearing the nap from cloth by the same action of spiral cutters revolving against a straight fixed edge, and many others have since appeared on the same principle, among which is that for mowing lawns.

## PHYSICAL AND MATHEMATICAL SECTION.

October 12th, 1865.

**Robert Worthington**, F.R.A.S., Vice-President of the Section, in the Chair.

**Mr. Dancer**, F.R.A.S., exhibited a small and cheap, but very effective induction coil, and a set of four Geissler's tubes, in which the stratification of the electrical light was very distinctly shown when a small battery of only one pair of elements was employed to produce the primary current.

**Mr. Brothers**, F.R.A.S., exhibited a beautiful series of enlarged photographs of the moon from negatives taken during the progress of the lunar eclipse on the night of the 4th instant (see *ante*, page 3).

**Mr. Dancer** stated, with reference to the eclipse, that he and his son, Mr. James Dancer, had noticed some irregularities on the border of the earth's shadow which, as they maintained their forms and relative positions whilst the shadow passed over the moon's disc, could not, he thought, be due to differences in the reflective power of different portions of the moon's surface.

**Mr. Baxendell** suggested that these irregularities might be owing to the prevalence of extended masses of clouds in certain portions of the earth's atmosphere and their absence in others.

The following table of rainfall at Eccles for 1864, was communicated to the Section by G. V. **Vernon**, F.R.A.S., at a meeting held April 13th, 1865:—

## RAINFALL AT ECCLES, NEAR MANCHESTER, FOR 1864.

By Thomas Mackereth, F.R.A.S., M.B.M.S.

Rain gauge 3 feet above the ground, 112 feet above the sea.

| Days of Rain Fall, 1864. | Fall of Rain in inches for 1864. | Average Fall of Rain for 4 years at Eccles. | Average Fall of Rain for 71 years at Manchester | Difference from 4 years' fall at Eccles. | Difference from 71 years' fall at Manchester | |
|---|---|---|---|---|---|---|
| 15 | 1·725 | 2·196 | 2·460 | — ·471 | — ·735 | January. |
| 13 | 3·160 | 1·887 | 2·379 | +1·273 | + ·781 | February. |
| 15 | 2·375 | 3·144 | 2·310 | — ·769 | + ·065 | March. |
| 12 | 1·722 | 1·977 | 2·088 | — ·255 | — ·316 | April. |
| 11 | 2·982 | 2·325 | 2·351 | + ·657 | + ·631 | May. |
| 20 | 2·878 | 3·390 | 2·913 | — ·512 | — ·035 | June. |
| 10 | 2·280 | 2·917 | 3·569 | — ·637 | —1·289 | July. |
| 15 | 2·462 | 2·959 | 3·592 | — ·497 | —1·130 | August. |
| 21 | 4·075 | 5·311 | 3·235 | —1·236 | + ·840 | September. |
| 13 | 1·910 | 3·382 | 3·818 | —1·472 | —1·908 | October. |
| 18 | 3·299 | 3·104 | 3·473 | + ·195 | — ·174 | November. |
| 17 | 2·006 | 2·815 | 3·260 | — ·809 | —1·254 | December. |
| 180 | 30·874 | 35·407 | 35·398 | —4·533 | —4·524 | Total. |
| Jan.<br>Feb.<br>Mar. | 7·260 | 7·227 | 7·149 | + ·033 | + ·111 | |
| April<br>May<br>June | 7·582 | 7·692 | 7·302 | — ·110 | + ·280 | |
| July<br>Aug.<br>Sept. | 8·817 | 11·187 | 10·396 | —2·370 | —1·579 | |
| Oct.<br>Nov.<br>Dec. | 7·215 | 9·301 | 10.551 | —2·086 | —3·336 | |

The above table shows that the rainfall of 1864 was 4½ inches below the average fall at Manchester for 71 years, and the average fall at Eccles for four years. Both averages appear to show that the largest rainfall, in this district, takes place from July to December, which is also, to some extent, confirmed by the rainfall of last year, though there was so great a deficiency in the summer and autumn months.

## Ordinary Meeting, October 31st, 1865.

R. ANGUS SMITH, Ph.D., F.R.S., &c., President, in the Chair.

The following communication from Sir J. F. W. Herschel, Bart., M.A., D.C.L., F.R.S., &c., Honorary Member of the Society, was read by Mr. Baxendell:—

Collingwood, October 18, 1865.

In the printed proceedings of the ordinary meeting of the society on the 3rd inst., I observe a notice of a paper by Mr. Greaves, "On the Internal Heat of the Earth as a Motive Power," in which the high temperature of the carboniferous strata, at the depth of 4,000 feet (120° Fahr.) is spoken of as likely to oppose an insuperable obstacle to the extraction of coal from that depth. On reading this it occurred to me that by employing condensed air, conveyed through conducting pipes, as a mode of working machinery at that depth—provided the air immediately on its condensation, and before its introduction into the pit, were drained of the heat developed in the act of condensation by leading it, in pipes exposing a large external surface, through a sufficiently large supply of cold water (or in winter time of snow) — the workings below might be sufficiently reduced in temperature by the re-expansion of the air on its escape, when given out below in the act of working the machinery, to admit of workmen remaining there in comfort; at the same time that ventilation would be supplied.

If you think that this suggestion would be worthy the notice of the author of the paper referred to, or of those members of the society who may have been present at its reading, or in any other way available, it is quite at your service for that purpose.

P.S.—Water at 120° Fahr., or even much higher, would, I fear, afford but an inefficient moving power unless some means could be devised (without the expense of more power than the gain expected) of concentrating the heat of a large quantity of warm water into a smaller. This might perhaps be done through the intervention of air alternately rarefied and condensed.

Mr. Binney, F.R.S., F.G.S., said that at the present time little is known as to the difficulties we should experience in working coal mines at a depth of 4,000 feet from the surface. The exact increase of temperature in deep mines is not by any means well ascertained. All we can say is that no great difficulties have been found in working at a depth of 2,100 feet. It must always be borne in mind that the deeper a mine is the greater will be the natural ventilation, that is the current caused by the air of the mine, at say a temperature of 80° Fahr., ascending the upcast shaft, while the air at the surface, of 40°, descends by the downcast shaft. No doubt a mine might be cooled by the expansion of compressed air, but it could not, so far as at present known, be done economically. In most deep mines a considerable cooling of the air takes place by the expansion of the compressed gas (light carburetted hydrogen) as it escapes from the coal, where it has been long imprisoned under great pressure; and this has not always been allowed for by observers of temperature in such places. In newly-opened mines this pent-up gas forces off large pieces from the face of the coal, and it sometimes makes a noise like water rushing over a weir. In sinking a deep shaft at Wigan some years since the compressed gas in the coal forced up about four yards of strong bind and made its way through it into the shaft. The rising of the roof of the coal as the shaft approaches it is well known to sinkers in deep and newly-opened coal fields.

Mr. **Edward Hull**, F.G.S., exhibited some etchings of caves, fissures, and isolated rocks on the coast of Cantyre, intended to illustrate three classes of phenomena belonging to the raised beach and coast, known as "the 30-feet beach," from the fact that its mean elevation is about 30 feet above the present tides. This raised beach has been described by several authors, from Mr. Smith, of Jordan Hill (1836), downwards, and is part of the same beach which has been traced all along the western coast of Scotland, and the vestiges of which remain in a state of remarkable freshness to the present day.

Mr. T. **Heelis**, F.R.A.S., called attention to the proceedings of a scientific commission recently issued by his Highness the Viceroy of Egypt, who had succeeded in finding a tertiary coal basin in the valleys between Mount Olympus and the Bay of Oraniska, in the Gulf of Salonica, and also on the mainland of Asia Minor, near the Island of Samos; and mentioned some particulars of the coal there found, such as its specific gravity, in which it slightly exceeds the ordinary coal of the coal measures, and the results of experiments upon its combustion, which give twenty per cent of ash.

A paper "On Coresolvents." by the Honourable Chief Justice **Cockle**, M.A., &c., President of the Queensland Philosophical Society, was communicated by the Rev. **Robert Harley**, F.R.S., &c.

Whatever function X may be of $x$ equation (3) of my last paper (*Proceedings*, vol. IV., pp. 37—40), is reducible to an equation with constant coefficients by changing the independent variable as indicated at p. 38 (*ibid.*), and the order of the symbolical factors of (3) is always indifferent.

Mr. Harley gives

$$3^3x(2-x)\frac{d^2y}{dx^2} + 3^2(1-x)\frac{dy}{dx} + y = 1 \; . \; . \; . \; \text{(i)}$$

as the differential resolvent of

$$y^3 - 3y^2 + 2x = 0 \ldots\ldots \text{(ii)}$$

The sinister of this resolvent is of the form of the sinister of (3) of my last paper, and the symbolical decomposition of (i) of this paper is ($i$ denoting as usual $\pm\sqrt{-1}$)

$$\left(3\sqrt{x(2-x)}\,\frac{d}{dx} + i\right)\left(3\sqrt{x(2-x)}\,\frac{d}{dx} - i\right)y = 1 \ldots\ldots \text{(iii)}$$

and the resolvent of this form of cubic, like that of the other, is soluble by change of the independent variable.

The evanescence of the dexter of a resolvent does not necessarily indicate that all its algebraic coresolvents are wanting in their second term. Thus, for instance,

$$3^2(1 - x^2)\frac{d^2y}{dx^2} - 3^2x\frac{dy}{dx} + y = 0 \ldots\ldots \text{(iv)}$$

is the differential resolvent not only of

$$y^3 - 3y + 2x = 0 \ldots\ldots \text{(v)}$$

but of every algebraical equation whose roots are homogeneous linear functions of the roots of (v), that is to say of any two of them, for any root of (v) is such a function of the other two. Let $\alpha$, $\beta$, and $\gamma$ be roots of an equation whereof any one root is a linear and homogeneous function of any two other roots. Then we may put

$$\gamma = m\alpha + n\beta \ldots\ldots \text{(vi)}$$

and, from the identities

$$\alpha\left(m\frac{d\alpha}{dx} + n\frac{d\beta}{dx}\right) - \left(m\alpha + n\beta\right)\frac{d\alpha}{dx} = n\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right)$$

and

$$\left(m\alpha + n\beta\right)\frac{d\beta}{dx} - \left(m\frac{d\alpha}{dx} + n\frac{d\beta}{dx}\right)\beta = m\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right),$$

we infer that

$$\alpha\frac{d\gamma}{dx} - \gamma\frac{d\alpha}{dx} = n\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right)$$

and

$$\gamma\frac{d\beta}{dx} - \beta\frac{d\gamma}{dx} = m\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right);$$

and if $m = n = -1$, we have

$$\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx} = \beta\frac{d\gamma}{dx} - \gamma\frac{d\beta}{dx} = \gamma\frac{d\alpha}{dx} - \alpha\frac{d\gamma}{dx},$$

relations which must be satisfied by the roots of (v) or of any cubic whereof the coefficient of the second term vanishes. (Compare art. 73 of my "Notes," &c., in the *Messenger of Mathematics* for November, 1864.) We have, indeed, generally

$$\left(m\alpha + n\beta\right)\left(p\frac{d\alpha}{dx} + q\frac{d\beta}{dx}\right) - \left(m\frac{d\alpha}{dx} + n\frac{d\beta}{dx}\right)\left(p\alpha + q\beta\right) = \left(mq - np\right)\left(\alpha\frac{d\beta}{dx} - \beta\frac{d\alpha}{dx}\right)$$

a relation which of course holds when we replace $\alpha$ and $\beta$ respectively by $a$ and $b$, two independent particular integrals of the differential resolvent.

"Oakwal," near Brisbane, Queensland, Australia,
August 1, 1865.

Mr. BAXENDELL drew attention to the auroral phenomena which occurred on the 19th and 26th instant, and showed sketches of the arches, &c., taken by Mr. R. P. Greg, F.G.S., at Prestwich.

Dr. JOULE, F.R.S., said he had observed the effect of the aurora on the former date, on his sensitive magnetic needle. The needle was violently agitated, as many as 36 changes of deflection, varying from 10″ to 1′ 40″ occurring per minute. The cause of the movements seemed to be instantaneous in its action. It was remarked that when the beams preponderated on the west of the magnetic north the needle took an easterly direction.

A paper was read entitled "Questions regarding the Life History of the Foraminifera, suggested by Examinations of their Dead Shells," by THOMAS ALCOCK, M.D.

The author said that though shore sand did not generally yield specimens in sufficient abundance or perfection to be worth examination, yet here and there very rich shore deposits are met with, and these ought to be specially noted when observed, both because of the exhaustless supply of specimens they afford for investigation, and the peculiarity of circumstances which must exist at these places to cause their accumulation. One such spot is at Dogs Bay, Roundstone, on the coast of Galway, where a great part of the shore is entirely composed of the remains of various microscopic creatures, by far the largest proportion consisting of shells of Foraminifera. Among other samples of sand from this place a very interesting one had been supplied by Mr. Parry, who had skimmed the material from the surface of pools left by the tide; it consisted entirely of fine and perfect specimens of the lighter kinds of Foraminifera, broken shells and the heavier sorts having sunk to the bottom. This naturally selected sample had furnished some very interesting results by supplying abundance of certain varieties comparatively rare in the rough sand, and examinations of some of these had led to the suggestions which he would now offer as to the formation of the shell in some kinds of Monothalamous Foraminifera. He considered in the first place that the soft body of the Foraminifer must act as a mould upon which the shell is formed, and that it must remain still and without change of shape while the process goes on; therefore that this, so far as the foundation layer of shell is concerned, will be a single act, and probably one requiring no great length of time. But if the shell be moulded on the surface of the animal, it will from the first be only just large enough to hold it, and consequently whatever growth afterwards takes place must be continued outside the shell, and must constantly increase the quantity of external sarcode which has been observed coating the shells of living Foraminifera, and which according to this view must be looked upon as more than the mere result of a coalescence of the bases of the Pseudopodia. Orbulina universa and Globigerina bulloides are two forms found in great abundance in this Dogs Bay sand; they are interesting as being the prevailing shells in deep-sea dredgings and also as always occurring together, a fact which has been noticed by Professor Williamson, who has long entertained the belief that there is some very close relationship between them. The Orbulinæ are found of very different sizes, the largest being as much as six times the diameter of the smallest, with every possible intermediate gradation; and considering that the shell can only be made of the exact size of the body on which it is moulded, while that body will continue to grow, he could not avoid the conclusion that in this case at least, as often as a larger shell is required, the animal must withdraw itself entirely from the old one and cast it off. But a consequence of this view that the single-chambered Foraminifera cast their shells at intervals to form new ones is that they will occasionally be freed for certain periods from the restraint of the shell, and be in a condition to effect that spontaneous division which is so striking a feature in the Rhizopoda.

There are other specimens of Orbulina found occasionally in the Dogs Bay sand which have a very special interest, since they appear to show most clearly that Globigerina is merely a younger state of this species,—a fact which was first announced by Mr. L. F. Pourtales, (Ann. and Mag. Nat. Hist. 1858), illustrated by specimens obtained from dredgings in the Gulf Stream. Dr. Carpenter mentions the observations but expresses doubt as to their correctness. The Dogs Bay specimens however seem to corroborate them most fully, for they show the perfect Globigerina inside the sphere of the Orbulina.

## MICROSCOPICAL SECTION.

October 16th, 1865.

A. G. Latham, Esq., President of the Section,
in the Chair.

This being the first Meeting of the Session, the President delivered an address reviewing the past proceedings of the Section, and referring with satisfaction to the proposal to extend its objects to subjects of Natural History generally.

Mr. Sidebotham read "Notes on Atlantic Soundings."

He said that in the unsuccessful attempt made to raise the Atlantic Cable after it had unfortunately parted, the ropes and grapnels brought up from the bottom small portions of ooze or mud, some of which was scraped off and preserved, as stated at the time in the newspapers. Believing that a careful examination of this deposit might prove of considerable interest, he wrote on the subject to Dr. Fairbairn, who, after considerable trouble, obtained for him a fine sample, mounted specimens of which he now presented for the cabinet and to each member of the Section. In appearance the deposit resembles dirty chalk, and under the microscope reminds one much of the chalk from Dover, indeed it has all the appearance of being a bed of chalk in process of formation. It is composed entirely of organisms, chiefly in fragments. In the short examination he had made, he observed several forms which give promise of interesting results, and he thought it would be desirable to frame a complete list of the species found, which would be best accomplished by two or three members taking temporary possession of all the slides, and preparing a report on their united observations. The sample now distributed was obtained at Dr. Fairbairn's request by Mr. Saward from Mr. Temple one of the Engineering Staff, who states that it was got in grappling for the Cable, August 11th, 1865, Lat. 51° 25′ 15″ N. Long. 38° 59′ W.

Mr. SIDEBOTHAM also read the following "Notes on Acherontia atropos":—

The Death's Head Moth, which was in former times an object of such dread that the appearance of a specimen of it was, like a comet, considered the precursor of some dreadful event, appears to be gradually becoming more common. If I remember rightly, it was Stothard the Artist who was so fortunate as to capture a specimen for his collection when a genuine British specimen was exceedingly rare. Even in my recollection a guinea or two was not considered too much to pay for a fine example. This season the insect has been unusually abundant, at least a score of larvæ having been found about my own neighbourhood. It has also been found at Bowdon, Middleton, Oldham, Strines, and other places round Manchester; in Middleton about 170 have occurred. Between Lytham and Blackpool it has been remarkably common; among those I obtained at Lytham was a very remarkable specimen, of which I made a rough drawing; it was so unlike the usual form that many who saw it fancied it must be some other species, but the same has been noticed by Stanton as occurring now and then, and Mr. Harrison obtained another somewhat similar at Bowdon. My specimen is still in the pupa state, and I shall carefully note whether or not the moth produced varies from the usual form.

Dr. ALCOCK read a paper entitled "Questions regarding the Life History of the Foraminifera, suggested by Examinations of their Dead Shells."

This paper was afterwards read at the Ordinary Meeting of the Society, held on the 31st October. See page 15.

Dr. ALCOCK also exhibited specimens of Eozoon Canadense, from Canada, and also from Ireland, lent by Mr. H. B. Brady.

Mr. SYMONDS CLARK, of Adelaide, South Australia, exhibited a series of skulls, of small Marsupial animals, beautifully prepared by him; also the skins of several species.

Mr. SIDEBOTHAM recorded the discovery of Apion ononis, in the Isle of Anglesey, a species of Curculio, which he stated to be new to Britain.

## Ordinary Meeting, November 14th, 1865.

R. Angus Smith. Ph.D., F.R.S., &c., President, in the Chair.

Mr. Charles Bailey, and Mr. Thomas Barker, M.A., Professor of Mathematics, Owens College, were elected Ordinary Members of the Society.

The following extract of a letter from Thomas Ainsworth, Esq., of Cleator, near Whitehaven, Corresponding Member of the Society, accompanying a copy of his meteorological observations for October, was read by Mr. Baxendell:

The great peculiarity of the season has been the very heavy dews we have had—great luxuriance of pasture nourished by dews and not by rain. I have drawn Professor Simmond's attention to this as being a predisposing cause of the present cattle disease. Not that it really engenders the malady, but predisposes the animal to take this peculiar type of disease—my own experience from the readings of my instruments some twelve or fourteen years ago having shown that disease of the same type attacked my cattle and destroyed them, and each time when the high temperature of the day and the low temperature of night gave us such heavy dews as to render the herbage quite indigestible.

Mr. Baxendell considered it very probable that cattle would be injuriously affected by feeding on herbage which had not been well washed by occasional showers of rain. Dew had little or no washing effect, and it could not remove the impurities which were deposited upon the leaves of plants during long periods of dry weather. The cattle plague is said to have had its origin in Central Asia, and in this region there is very little rain, and the daily range of temperature is very great. The herbage is therefore seldom well washed, and moreover, the cattle that feed upon it are exposed to frequent and violent changes of temperature. We have no report of any cattle plague breaking out among the herds on the pampas of South America, where rain falls more abundantly and the changes of temperature are much less violent.

The PRESIDENT said that the idea of deriving the cattle plague or any similar epidemic from the organic matter brought down by dew was at least in harmony with much that we had learnt. The dews and fogs of evening over certain lands were known to produce colds, agues, or fevers which could be avoided by rising to a certain height from the ground. There seems little doubt that the moisture in such cases is not the cause of disease, but only the means of conveyance. These diseases were produced by breathing the impure air. We know less of the effect when the matter is condensed and conveyed into the stomach, but the effect of impure water made this use of it also suspicious. He was not aware that it could be shown that in aggravated cases another class of disease might not be produced. In Manchester we can see the accumulation of matter taking place in the fog to such an extent that it lies like a cap over the whole town, and so increases that every sense is affected, whilst the lungs and eyes suffer severely. The matter in solution in this case is not putrefactive, although injurious, or it would probably sweep us off instantly. Probably no accumulation of putrefactive matter equal in amount ever occurred in the natural atmosphere. It illustrates, however, the mode by which the emanations of the soil are collected in the atmosphere and presented in a concentrated form for us to breathe. He had for many weeks collected dew on a grass lawn in a garden, and from it had obtained organic matters unquestionably collected from surrounding objects, as it was known on one occasion to smell of flowers. If this entered into putrefaction it would of course be unwholesome, but what kind of disturbance of health it would cause it must be for others to find. The evening air of a rainy country like this is less dangerous than that of some other climates where there is more both evaporated and condensed, and neither wind nor rain to remove it. Notwithstanding this, he believed that more than the dew was required, especially in northern climes.

The PRESIDENT also said, that when sitting in a railway carriage with his friend, Mr. James Young, of Bathgate, that gentleman observed that the particles of dust which floated in the air seemed to shine with a metallic lustre. Dr. S. immediately collected some, and found that the larger class were in reality rolled plates of iron which seemed to have been heavily pressed and torn up from the surface. Another and smaller class were less brilliant, and when looked at with a considerable power shewed many inequalities of surface which would be interesting to study. Probably these were the particles which were not torn up but rubbed off. The dust enters the mouth and lungs, and has to be taken as one of the evils of railway travelling, although we do not know that these small particles are worse than those of sand. At any rate, it is clear that some kind of iron will wear down more readily than others, and we ought to have that which will wear down least. By observing what takes place in the carriages on a dusty day, every man may to some extent compare the iron of different railways. Those which give off the largest pieces in greatest quantities, are to that extent the worst, as regards health.

A paper was read entitled "An Attempt to refer some Phenomena attending the Emission of Light to Mechanical Principles," by R. B. CLIFTON, M.A., Professor of Natural Philosophy in Owens College.

The author attempted to show, by analogical arguments, that it is possible to give some account of certain phenomena attending the Emission of Light, by assuming principles closely resembling, if not identical with, those adopted by Professor Clausius in his well known paper on "The Nature of the Motion which we call Heat."*

Matter is assumed in all cases to have its atoms grouped together into *molecules*, an assumption which seems necessary when the different allotropic states of certain substances are considered.

These molecules are assumed to be in motion, and also the atoms to be vibrating in the molecules; for, whatever may be the laws of the forces which bind together the atoms in the molecules, it is impossible to imagine the molecules to be in motion, and to be subject to mutual actions, without causing motion of the component atoms.

In *solids* and *liquids* the molecules are supposed to remain continually within the spheres of action of neighbouring molecules, so that the internal motion in a molecule is never due to the undisturbed action of the interatomic forces—the only difference between solids and liquids being that in the former the same molecules are constantly neighbours, while in the latter a molecule may completely change its place in the liquid, and also that in liquids a molecule may perform complete rotations round axes through its centre of gravity, while in solids this is not generally possible.

In a *perfect gas* a molecule is supposed to be under the action of other molecules, only for a portion of time indefinitely small with respect to the whole time of motion, and its centre of gravity describes a polygonal path, only changing its direction of motion upon the near approach of the molecule to another molecule, or to a containing vessel, which may be considered as equivalent to an impact.

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* *Phil. Mag.*, vol. xiv., for 1857.

In an *imperfect gas* a molecule is supposed to be under the action of other molecules during a finite portion of the whole time of motion, this portion increasing as the gas approaches its state of saturation.

Between the molecules of a body, and the atoms of a molecule, the *luminiferous ether* is supposed to exist.

The vibrations in the ether which constitute radiant heat and light, are considered due to the vibrations of the atoms in the molecule, and not to the motion of the molecule as a whole; the latter bearing some such relation to the ether, as a bell or a stretched string does to the air, the internal vibrations only in the two cases causing the vibrations in the surrounding media, which give rise respectively to light and sound.

It appears obvious that as the motion of a molecule of a body as a whole increases, that is, as the temperature rises, the internal motion in the molecule also increases, considering that the action of one molecule upon another must be due to the mutual action of atoms, or to the interatomic forces, it seems probable that the internal vis viva in a molecule, to which the light is due, is proportional to the vis viva of the molecule as a whole, to which heat is to be referred. Thus, as the temperature of a body rises, the internal vis viva in the molecules increases, and the vis viva communicated to the ether also increases; hence the intensity of the vibration in the ether increases, and at the same time the period of vibration diminishes, or waves of shorter length are continually produced with increasing intensity.

Hence as the temperature of a body rises radiant heat is given off, the intensity corresponding to a given wave length constantly increasing, at last then vibrations in the ether, with wave lengths corresponding to the extreme red of the spectrum, will be caused with sufficient intensity to be visible, and thus the body will begin at first to glow with red light; as the temperature still rises, and vibrations of shorter and shorter wave lengths become of visible intensity, the light emitted will gradually change from red to white.

From Draper's Law, that all bodies become incandescent simultaneously, as well as from other considerations, it seems probable that in *all bodies* the internal vis viva in the molecules bears the same ratio to the vis viva of the molecule as a whole.

In solid and liquid bodies, the molecules being constantly under their mutual actions, and these actions being subject to constant change from the varying relative positions of the molecules, the atoms cannot assume any definite periods of vibration, but are constantly changing the time of vibration; hence the vibrations in the ether will be constantly, and with extreme rapidity, changing their periods. This change having apparently no limit, and the effect upon the eye continuing for a finite time, light of all wave lengths will appear to be given off simultaneously by such bodies when the temperature is sufficiently high; in other words, incandescent solids and liquids will appear to give off white light, which when analysed by a prism will yield a continuous spectrum.

In the case of an incandescent Gas or Vapour sufficiently removed from a state of saturation to be considered perfect, the atoms will be left to vibrate under the action of the interatomic forces only, and will thus assume periods of vibration all included in a certain set; these vibrations will consequently cause vibrations in the ether corresponding only to certain definite wave lengths. Hence the spectra of such incandescent vapours will be broken, and consist only of a series of fine lines.

With imperfect gases, or vapours not far removed from their points of saturation, the intermediate phenomenon of spectra broken, but consisting of bands, is to be expected:

when however the temperature of such vapours is sufficiently increased, a change from spectra consisting of bands to spectra consisting of fine lines is to be looked for. This change has been observed in many cases.

When a solid body is incandescent, the light emitted so as nearly to graze the surface may be considered due mainly to the surface molecules, but these being free on the side of the surface, but affected by other molecules on all other sides, the internal vibrations in these surface molecules will have a bias in a direction perpendicular to the surface. Thus the vibrations caused in the ether, which are propagated nearly grazing the surface, will preponderate in a direction perpendicular to the surface, or considering the vibrations in plane polarised light to be perpendicular to the plane of polarisation, the light emitted by such a body, so as to pass close to its surface, will be partially plane polarised, the plane of polarisation being parallel to the tangent plane to the surface of the body at the point of emission.

In the case of an incandescent gas, the surface molecules are continually changed, and as a molecule may arrive at the surface in any position and is equally free on all sides, all trace of polarisation in this light will be destroyed.

The fact that incandescent metallic plates do emit partially plane polarised light in directions nearly grazing the surface, the plane of polarisation being parallel to the surface, and that incandescent gases emit unpolarised light, has been observed by Arago.

As the molecules at or near the surface of solids or liquids can cause vibrations in the ether, giving rise to emitted light, it is to be expected that, in some cases at least, it will be possible for light, if of sufficient intensity, when incident upon a body, to cause vibrations in the atoms constituting the molecules near the surface, but considering the difference of mass of the atoms of the body and of those of the ether, that the atoms of the body will vibrate slower than those of the ether, the actual times of vibration depending however upon the molecular forces in the body. As these atomic vibrations will again affect the ether, such bodies will or may become luminous, the wave lengths of the emitted light being however longer than those of the incident light which causes the disturbance in the body.

This emitted light will necessarily last some time after the incident light is removed, for the vibrations in the body will not cease as soon as the cause of disturbance is removed, but in general it is to be expected that this emitted light will speedily disappear, though cases may occur in which it will continue for a considerable time.

These probable deductions from the assumed principles coincide exactly with the phenomena of Fluorescence and Phosphorescence (not including in this term cases in which light is emitted by bodies undergoing slow combustion), all Fluorescent bodies being Phosphorescent for times of different, though in all cases at present observed, of very short duration.

## PHYSICAL AND MATHEMATICAL SECTION.

November 9th, 1865.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

Mr. BAXENDELL, F.R.A.S., read the following "Note on the Variable Star S Delphini."

Since the discovery of this variable in October, 1863, it has gone through two complete periods of change, and my observations have enabled me to fix with tolerable exactness the dates of three maxima, and to determine the form of the light-curve for that portion of the period during which the star is visible with telescopes of moderate power. Comparing the light-curves for 1864 and 1865 with the curve laid down from the observations made in 1863 after the discovery of the variable, I conclude that a maximum occurred about the 14th of October, 1863, the magnitude being 8·5. The next maximum occurred on the 12th of September, 1864—magnitude, 8·3; and the third on the 9th of August, 1865—magnitude, 8·9. The interval between the first and second maxima is 334 days, and that between the second and third, 331 days. The mean period is therefore about 332 days. In the Astronomische Nachrichten, No. 1523, Dr. Schönfeld states that this star was observed on the 8th of September, 1855, in zone 724 of the Bonn "Durchmusterung," and estimated to be of the ninth magnitude, but was invisible to Professor Argelander with the telescope of the Bonn meridian circle on the 9th, and with the heliometer on the 20th of November following, and was therefore not inserted in the "Bonner Sternverzeichniss," its existence evidently being considered doubtful. Guided by the course of the star's light-curve, I conclude from the Bonn observations that a maximum occurred about the 12th of August, 1855. The interval between this date and that of the last maximum, August 9th, 1865, is 3,650 days, during which time the star passed through eleven complete periods of change. We find therefore that the value of the mean period is 331·8 days, a result agreeing very closely with that derived from my own observations alone.

An inspection of the diagrams which accompany this communication will show that S Delphini increases in brightness much more rapidly than it diminishes, and that the course of its light-curve is more irregular after than before a maximum. During the last apparition it rose from the thirteenth magnitude to a maximum in 48 days, but was 89 days in descending again to the same magnitude. When at minimum it is below the 13½ magnitude, and it remains invisible with telescopes of ordinary power for more than the half of its entire period.

Mr. G. Knott, F.R.A.S., of the Woodcroft Observatory, Cuckfield, Sussex, has kindly favoured me with a copy of his observations of S Delphini, made during the present year, and of the light-curve laid down from them. He has obtained for the date of maximum August 11, 1865—magnitude, 8·8. Considering the nature of the observations, and the form of the light-curve at its maximum, this result agrees very fairly with that derived from my own observations. It will however be seen that there is a slight difference in our modes of drawing a curve through the points laid down; Mr. Knott evidently regarding apparent irregularities as being principally owing to errors of observation, while, on the other hand, I have regarded them as being mainly due to actual changes in the brightness of the star. Treating his observations precisely as I have treated my own, I do in fact, obtain the same date of maximum, August 9, 1865.

The colour of S Delphini is decidedly "reddish," and my observations seem to indicate that this colour becomes sensibly more intense as the star approaches its minimum. The difference of 0·1 between my own and Mr. Knott's estimations of magnitude at the last maximum is doubtless due to the colour of the star, and to the fact that while my observations have been made with a telescope of five inches aperture, Mr. Knott's have been made with one having an aperture of seven and one-third inches.

ERRATUM.—Page 20, last paragraph, for *apion ononis*, read *apion ononidis*.

## Ordinary Meeting, November 28th, 1865.

R. ANGUS SMITH, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Francis Hampson, Solicitor, was elected an Ordinary Member of the Society.

Mr. DANCER, F.R.A.S., said that in a paper "On the Illumination of Opaque Objects under the high powers of the Microscope," read before the Microscopical Section of this Society, November 20th, he had described a method of employing the oblique body of the binocular microscope with Wenham's prism, for illumination of opaque objects, and he had also exhibited an instrument fitted up for this purpose, giving the members present a practical demonstration of the advantages which this mode of illumination afforded under certain circumstances. He wished now to describe another method of illuminating opaque objects, and as it is equally applicable to monocular and binocular microscopes, it appears worthy of some consideration.

In the method of Mr H. L. Smith, of Kenyon College, (which was briefly described in the paper before mentioned), and also in the use of the Wenham's prism there is a considerable loss of angular aperture, (which is a very important consideration): it occurred to the author that by modifying Mr. Smith's contrivance this loss might be diminished in some degree; this has been attempted in the following manner.

Instead of placing the mirror immediately over the opening at the back of the object glass, a small speculum ⅛ of an inch in diameter is introduced into the front of the body of the microscope, 2½ inches above the top of the objective. A lateral opening is made in the body at right angles to the speculum, for the admission of light to be reflected down through the objective to the object below.

The interposition of the small speculum does not produce any disagreeable effect in the field of view, and in the examination of objects it is easy to use that portion of the field which is between the centre and the edge. With proper manipulation very good definition can be obtained by this method, when the speculum is of the proper curvature. This contrivance can always remain attached to the microscope without interfering with the general appearance of the instrument, and when the use of the speculum is not required, it can be withdrawn or turned aside out of the field of view, and the aperture at the side of the body may be closed by a small shutter. It is obvious that the use of the binocular body is not interfered with by this arrangement.

A binocular and a monocular microscope with this arrangement were exhibited to the members at the close of the meeting.

Mr. **Hardy** (on behalf of Edward Ross, Esq.) exhibited a large cetacean vertebra which had been found in the valley of the Don, about two miles from Tinsley and four from Rotherham, in Yorkshire, on a line of railway now in course of construction. The bone was met with in excavating, at a depth of 14 feet below the surface of the ground, in a bed of gravel overlaid by the alluvium of the valley. In answer to questions put by Messrs. Binney and Hull, Mr. Hardy described the bone as one of the lumbar vertebræ of a species of whale, probably identical in genus with the balæna of the present seas. The bone measured on its largest diameter a little over ten inches, in thickness seven inches, and in circumference about three feet. It presented every appearance of having lain in the earth for a very considerable length of time; but as it only reached Manchester on the day previous, the geological character of the gravel in which it was found had not been ascertained.

Dr. ROBERTS drew attention to the injurious effects produced by burning Pharaoh's serpents in close rooms, and gave the particulars of a case which had lately come under his notice.

Dr. ROSCOE stated that in his opinion persons could not be too careful respecting the inhaling of even small quantities of mercury vapour, and he alluded in support of this opinion to the fact that two German gentlemen who were engaged in a London laboratory, in the preparation, for a scientific purpose, of volatile organic mercury compounds, had recently been poisoned by the accidental absorption through the lungs or skin of very small quantities of the vapours of these substances. The symptoms characteristic of this form of mercurial poisoning are of the most painful and distressing kind, the first patient dying in a state of mania shortly after his admission into the hospital, and the second, on whom the effect became first perceptible three months after he had ceased to work with the substance, now lying in a hopeless state of idiocy.

A paper was read "On the Amount of Carbonic Acid contained in the Air above the Irish Sea," by Mr. T. E. THORPE, Assistant in the Private Laboratory, Owens College, communicated by Professor H. E. ROSCOE, F.R.S.

The determination of the amount of carbonic acid contained in the atmosphere over the land has been made the subject of investigation by many experimenters, and from the results obtained by Theodore de Saussure, Brunner, Boussingault, Angus Smith, and others, we are acquainted with the exact proportion of this gas contained in the atmosphere under varying circumstances of situation and weather.

But hitherto, the influence which, *a priori*, must necessarily be exercised by large bodies of water on the proportion of carbonic acid in the atmosphere has scarcely been sufficiently studied. The fact that a considerable influence is exercised has certainly been noticed, but beyond the incomplete results of one or two observers, we have no numerical data from which to judge of the extent of this influence, and we therefore know but little of the changes in the comparative amount of the atmospheric carbonic acid as effected by the waters of the ocean.

Dr. Roscoe therefore suggested that I should undertake some experiments on this subject, and kindly placed the necessary time and apparatus at my disposal. I may here be allowed to express my thanks for this kindness, and for the advice I have received from him during the prosecution of these experiments.

It appeared from the observations of Vogel on the air of the Baltic and of the Channel that the sea abstracts to a very considerable extent the carbonic acid from the atmosphere; and this conclusion was apparently confirmed by the experiments of Emmet on the air over the Atlantic and at Bermuda, and by the determinations of Watson at Bolton, made on air blowing from the seaward.

These experiments were however, for the most part, merely qualitative, and the circumstances under which they were made, together with the inaccurate nature of the methods employed, render such a conclusion premature. In fact, the experiments of Lewy and Morren on the nature of the gases which sea water holds in solution at different periods of the day and during various seasons of the year would appear to show that the sea may possibly act in quite the opposite direction, and cause a sensible *increase* in the comparative amount of atmospheric carbonic acid.

The air contained in sea water consists of variable quantities of free carbonic acid, oxygen, and nitrogen, and Morren and Lewy have shown that the changes in the relative proportion of these gases depend:—(1) upon alteration of temperature affecting the relative amounts of the dissolved gases in accordance with the laws of gaseous absorption; and (2) upon the variations in intensity of direct and diffused solar light, producing a corresponding effect upon the vitality of sea plants and animals, and hence altering the composition of the dissolved gases.

Some further experiments by Lewy, on the composition of the atmosphere above the Atlantic ocean in the tropics, tend to confirm the above supposition of the possible increase in carbonic acid in the atmosphere above the sea. In fact, if it is possible that the composition of the air above the sea in our latitudes can be sensibly altered by this phenomenon of the variation in the nature of the gases in solution in sea water, as Lewy and Morren assert, we might expect that the atmosphere above the tropical oceans would manifest to a much larger extent variations in the relative amounts of carbonic acid and oxygen, since infusoria exist, as is well known, in enormous quantities in these oceans, and the composition of the air in their waters must necessarily undergo rapid variation, and a considerable evolution of the dissolved gases must consequently occur. At the instance of the French Academy Lewy collected air at different times during a voyage from Havre to Santa Marta, and on subsequent analysis not only did it appear that the mean quantity of carbonic acid was sensibly greater in the air of the Atlantic ocean in the tropics than in the air of the land, but also that the air of the day was appreciably richer in carbonic acid and oxygen than air collected in the night.

On comparing the means of each series we have, in 10,000 volumes of air, for the

| | Day (mean of 7 expts.) | Night (mean of 4 expts.) |
|---|---|---|
| Carbonic acid...... | 5·299 | 3·459 |
| Oxygen ............ | 2105·801 | 2097·412 |

and this variation appeared to increase in proportion as the middle of the ocean was approached.

This remarkable phenomenon, of the variation in composition of the air above the tropical oceans, may doubtless be accounted for, without any reference to the direct action of infusoria, by the heating effect of the sun on the sea water and the consequent disengagement during the day of gas proportionately rich in carbonic acid and oxygen. During the night, on the other hand, as this source of action is removed, the disengagement may be assumed not to occur; and, following Lewy, one may perceive that this difference would become more appreciable and easier to trace in air at great distances from any continent than in air collected nearer the coasts, and consequently, liable to be mixed with the air of the land.

Although the precision of these results is certainly remarkable, they still require confirmation. The air was collected in glass tubes of about 100 cbc., and analysed eighteen or twenty months after in the eudiometric apparatus of Regnault and Reiset. The fact pointed out by Regnault that air which has remained for any great length of time in glass tubes invariably exhibits notable diminutions in the amount of carbonic acid, since the glass absorbs a portion of this gas; and the difficulty generally experienced in accurately noting contractions so minute as the absorption of the carbonic acid from a small volume of atmospheric air, are circumstances which may possibly influence the reliability of the results.

The kind permission of the Honorable Board of Trinity House has enabled me during the vacation of last summer to make some additional experiments in this direction on board the "Bahama Bank" Light-vessel, situated in the Irish sea, latitude 54° 21′ and longitude 4° 11′, seven miles W.N.W. of

Ramsey, Isle of Man, and consequently nearly equidistant from the nearest shores of England, Scotland, and Ireland. The ship is placed to mark the proximity of a dangerous bank, by which, for the greater part of the day, a strong current, setting in from the southward, flows through the North Channel and thence into the Atlantic.

These experiments were made in the early part of August, at the same periods of the twenty-four hours, namely about 4 a.m. and 4 p.m., or nearly the times of minimum and maximum temperature.

Pettenkofer's method of analysis was adopted, with the improvements in the practical details suggested by Angus Smith. This method is in principle similar to the one adopted by Watson and Emmet, but admits of far more delicacy and precision in practice. Baryta is substituted for lime water, and oxalic for sulphuric acid. The solution of oxalic acid for these experiments was made so that one cubic centimetre of it corresponded to one milligramme of carbonic acid; it thus contained 2·864 grms. of pure crystallised oxalic acid per litre. Twenty-five cubic centimetres of the baryta solution were originally made to correspond to about twenty-eight of oxalic acid, but of course the exact strength of the baryta water was ascertained previous to each experiment. The bottles were generally filled with the air by means of the bellows, but sometimes when the wind was strong it sufficed to hold them up for a minute or two in such a manner that the air could circulate freely within. The baryta water remained in contact with the enclosed air for three quarters of an hour to one hour, during which time the bottles were frequently agitated. Although even this is longer perhaps than is actually required for the complete absorption of the carbonic acid, still, for the sake of conclusiveness, in experiment 4 the bottles were allowed to stand for three hours, and in experiment 13 for six hours, before the solutions were tested. The capacities of the two bottles which served for all the experiments were 4815 cbc. and 4960 cbc. The burette was Mohr's modification, for which a table of calibration had been constructed by weighing and interpolating in the ordinary way.

The fact that the various meteorological changes influence to such a remarkable extent the nature and amount of the gases dissolved in sea-water renders it necessary, in any investigation on the constitution of the atmosphere over the sea, to take particular account of these changes. Accordingly the temperature, pressure, and degree of humidity of the air; direction and force (estimated—Beaufort's system) of wind; amount (estimated—overcast=10) and nature of clouds, and general appearance of the day, together with the temperature of the sea water and amount of sea disturbance (1 to 9), were noted at the time of each experiment.

The following table shows the results of these observations, together with the amount, in volumes, of the carbonic acid in 10,000 volumes of air. All the experiments which were made are here given. The hours of observation, as before stated, were 4 a.m. and 4 p.m.

## TABLE OF RESULTS.

| No. | Aug. 1865. | Night or Day. | Bar. Mm. | Temp. of Air. |  | Temp. of Sea. | Direction and Force of Wind. | Sea Distrb. (1 to 9) | Amount and Nature of Clouds. (Overcast =10.) |  | Carbonic Acid. |  | Remarks. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
|  |  |  |  | D. Bulb. | W. Bulb. |  |  |  |  |  | 1st Ex. | 2d Ex. |  |
| 1 | 4th | D. | 762·5 | 16·4° C. | 11·1° C. | 16·0° C. | N.W. by W. v. light | calm | cirrus | 1 | 2·66 | 3·07 | Day very fine and clear. |
| 2 | 4th | N. | 762·0 | 13·9 | 12·9 | 15·0 | S.W. by S. light br. | 1 | cirro-cumuli | 9 | 2·92 | 3·05 |  |
| 3 | 5th | D. | 761·2 | 16·1 | 14·4 | 15·0 | S.W. by S. light br. | 1 | cirro-cumuli | 9 | 3·08 | 3·21 | Not much sun. |
| 4 | 6th | N. | 753·4 | 14·2 | 13·8 | 15·0 | S.W. by W. light | 1 | cirro-cumuli | 9 | 3·30 | 3·22 | Baryta water exposed 3 h. |
| 5 | 7th | D. | 757·5 | 17·2 | 15·1 | 15·6 | N.W. light | 1 | cirro-cumuli | 6 | 3·20 | 3·15 | Sunny, very fine. |
| 6 | 7th | N. | 760·2 | 18·6 | 12·2 | 15·0 | N.N.W. moderate | 3 | cirro-cumuli | 8 | 3·06 | 3·19 |  |
| 7 | 8th | D. | 761·0 | 18·3 | 13·1 | 16·0 | N.N.W. light breeze | 1 | cirrus | 1 | 3·32 | 3·02 | Fine and sunny. |
| 8 | 8th | N. | 758·7 | 13·3 | 12·2 | 15·0 | S.W. by W. light br. | 1 | cirrus | 7 | 2·93 | 3·10 |  |
| 9 | 9th | D. | 756·4 | 15·0 | — | 15·0 | S. by W. moderate | 1 | cirro-cumuli | 7 | 3·09 | 3·23 | Rain. |
| 10 | 10th | D. | 749·3 | 15·0 | 13·9 | 14·5 | S. by W. fresh | 4 | nimbus | 9 | 3·11 | 3·11 | Very wet, rain all day. |
| 11 | 11th | N. | 750·5 | 18·4 | 11·9 | 14·5 | S.W. by W. strong | 5 | nimb. & cirro-cum. | 7 | 3·09 | 3·10 | Very windy and much rain from the 11th to the 16th. |
| 12 | 16th | D. | 752·3 | 14·7 | 12·8 | 15·0 | N.W. by W. light | 2 | cirro-stratus | 8 | 2·93 | 2·95 |  |
| 13 | 16th | N. | 753·1 | 13·9 | 12·8 | 15·0 | W.S.W. fresh | 4 | cirro-stratus | 9 | 3·12 | 2·94 | Baryta water exposed 6 h. |

Day Mean of 14 Determinations .................................................... 3·086
Night „ 12 „ .................................................... 3·085

In comparing these results with the following determinations of the carbonic acid contained in land air, it is seen that the air of the Irish Sea contains a much smaller proportion of carbonic acid than the air of the neighbouring land. The most extensive observations on the land air have given as means:—

| *Observer.* | *Locality.* | *No. of Expts.* | *Vols. in 10,000 of air.* |
|---|---|---|---|
| Th. de Saussure, | Chambeisy, | 104 | 4·15 |
| Boussingault, | Paris, | 142 | 3·97 |
| Verver, | Groningen, | 90 | 4·20 |
| Roscoe, 1st series, | London & Manchester, | 108 | 3·97 |
| „ 2nd series, | Manchester, | 53 | 3·92 |
| Smith, | ditto. | 200 | 4·03 |
| General mean of land air | | | 4·04 |
| Mean of 26 expts. on sea air | | | 3·086 |

It would also appear that no difference is discernible in the amount of carbonic acid in the air of day and night over the Irish Sea. On the other hand, from Saussure's observations a decided difference may be traced between day and night air on the land—a conclusion subsequently confirmed by several experimenters.

In noting the above mean 3·08, and the apparent identity in the amount of carbonic acid in the air of day and night over the sea, it should be borne in mind that July and August are, in general, the hottest periods of the year, (these months were unusually hot this year, 1865) and that, consequently, all the influences may be supposed at work which would tend to increase the relative amount of carbonic acid, and render appreciable any difference in the air of night and day.

The conclusions therefore to be drawn from these experiments are:—

1. That the influence of the sea in our latitudes in abstracting the carbonic acid from the atmosphere is not so great as the old experiments of Vogel and others would lead us to suppose.

2. That the sea in our latitudes does not act in increasing the amount of carbonic acid in the air above the ocean, as found by Lewy over the Atlantic near the equator.

3. That the differences observed in the air of night and day by Lewy on the Atlantic, are not perceptible in the air above the Irish Sea.

4. That in the month of August 1865, the mean quantity of carbonic acid in the atmosphere of the Irish Sea was 3·08 in 10,000 volumes of air.

In conclusion, I beg to acknowledge the kind attention which I received from Captain Temple, and from his crew during my stay on board his ship.

## MICROSCOPICAL SECTION.

November 20th, 1865.

A. G. Latham, Esq., President of the Section, in the Chair.

Mr. Parry read a paper on "Collecting Foraminifera on the west coast of Ireland." He said that in June last he visited the coast of Connemara, for the purpose of collecting Foraminifera, more especially at Dogs Bay; he was accompanied by Mr. Burns, of Doohulla Lodge, who gave him much assistance. After he had procured a considerable quantity of the shell-sand in the usual way, he noticed some white floating material on the surface of the advancing tide; he collected a quantity of it by means of a muslin net, and on examination found it nearly all composed of perfect dead shells of Foraminifera. On a second visit to the bay Mr. Burns discovered a pool near high water mark, covered with the floating shells, and of these Mr. Parry collected a large quantity, portions of which he had since distributed to members of the Section. He observed that the underside of the rocks forming the pool were covered with foraminifera, and he therefore concluded that these minute creatures live there, and from what he saw he was led to believe that Dogs Bay is a breeding ground for them, and that they may also be found living in "Burns' Pool."

Mr. Dancer, F.R.A.S., read a paper "On the Illumination of Opaque Objects under the high powers of the Microscope."

The Author's attention was drawn to a paper on this subject, which appeared in the Scientific American, and was copied into the Mechanics' Magazine of October 20th, 1865.

Mr. H. L. Smith of Kenyon College, had contrived a plan for the illumination of opaque objects, by placing a small mirror in a rectangular box, which could be attached to any ordinary microscope, this mirror was made adjustable immediately over the opening of the back of the objective, a light was placed at the side of the box and reflected down through the objective on to the object. In this manner the object could be illuminated when the high powers were used.

Mr. Hurst suggested that a discussion on this subject would be of interest to the members of the Microscopical Section. The Author not having time to make one of Mr. Smith's apparatus, thought it possible to arrive at similar results by the employment of the binocular microscope, an instrument which is now more common than a monocular instrument. The trial quite answered his expectations. The simplest method and one which gave good results, is to remove the eye piece from the oblique body and fix a reflector on the top of the body in such a manner as to throw the rays of light down to the Wenham's prism, and thence through the object glass on to the object.

If a plane mirror is employed, a lens of suitable focal length should be placed in the body in order to get the field of view entirely illuminated.

A concave mirror or lenticular prism can also be used for the same purpose, providing the focal length is adapted to the length of the body and object glasses.

Various modifications can be adapted so as to vary the character of the illumination to suit the particular object to be viewed. In some cases the Wenham's prism may be withdrawn a little, to produce the proper effect.

Uncovered objects only can be seen to advantage, owing to the light reflected from the surface of the covering glass. The surface on which the objects are mounted should reflect as little as possible, and be a marked contrast in colour to the object.

## Ordinary Meeting, December 12th, 1865.

R. Angus Smith, Ph.D., F.R.S., &c., President,
in the Chair.

Mr. J. Bottomley said that a recent paper upon the employment of the internal heat of the earth led him to consider what might be the condition of the atmosphere when coal, lignite, anthracite, and all other forms of vegetable fuel should be so exhausted that the human race would be compelled to resort to this source of heat. The numbers obtained led him to the conclusion that the exhaustion of the coal fields implied more than the depriving of the human race of a ready source of warmth, namely, the alteration of the atmosphere to an extent that would ultimately prove fatal. As the latter assumption seemed to him to be repugnant to reason, he would infer that long before the exhaustion of the coal fields, the carbonic acid in the atmosphere beyond the limits of safety to life, would have been decomposed by vegetation; moreover, as plants decomposed water, there would always be some combustible compound of carbon and hydrogen; in other words, there will and must be abundance of fuel in the world in all ages, if not of so dense a character as anthracite and coal, yet of some nature intermediate between those fuels and vegetable tissue, the origin of all varieties. The effect of vegetation in maintaining the purity of the atmosphere has long been known. The assumption that this agency is sufficient to furnish an abundant and perpetual supply of fuel to mankind involves no new principle, but it tends to establish a new inference upon principles already acknowledged. Liebig states, in his "Chemistry in its Application to Agriculture and Physiology," (3rd edition), that the quantity of carbon existing in the atmosphere amounts to more than the weight of all the plants and of all the strata of mineral and brown coal existing on the earth. This would seem to favour the notion that the mineral and brown coal available for combustion would not affect the atmosphere to any serious extent if consumed. He has assumed more carbonic gas to exist in the atmosphere ($\frac{1}{1000}$ by weight) than many authorities would allow. Moreover, since the book was written enormous deposits of fuel have been discovered. In another passage Baron Liebig seems to favour an opposite view, for he states—"In former ages, therefore, the atmosphere must have contained less oxygen, but a much larger proportion of carbonic acid, than it does at the present time, a circumstance which accounts for the richness and luxuriance of the earlier vegetation." Dumas and Boussingault say, in their book on the chemical and physiological balance of organic nature, "If we suppose, then, that the whole of the carbon was diffused through the atmosphere in the shape of carbonic acid prior to the creation of organised beings we shall see that the atmosphere, instead of containing less than the one-thousand part of its bulk of carbonic acid as at present, must have contained a quantity which it is not easy to estimate, but which was perhaps in the proportion of 3, 4, 5, 6, and even 8 per cent." Mr. Hull, in his "Coal Fields of Great Britain," taking 4,000 feet as the depth capable of being worked, estimates the supply from the English and Welsh coal fields at 60,000,000 tons for 1,000 years. In the same book it is stated that the American coal fields are 72 times greater than the English and Welsh.

In the reports furnished to the Admiralty some years back, by Dr. L. Playfair and Sir H. De la Beche, there is given a table showing the average composition of Welsh, Newcastle, Lancashire, Scotch, and Derbyshire coal. The mean of these numbers is as follows:—Carbon, 80·40; oxygen, 7·16; hydrogen, 5·19. Subtracting from the amount of hydrogen the quantity corresponding to 7·16 of oxygen there remain as combustible material in one part of coal—carbon, 0·8040; hydrogen, 0·0421. For combustion it requires 2·4828 parts of oxygen, and produces 2·9480 parts of carbonic acid. Sir J. Herschel, in his "Meteorology," takes as the approximate weight of the atmosphere $11 \times 10^{18}$ pounds. If we take as the amount of oxygen in the atmosphere 23·04 per cent by weight, and as the amount of carbonic acid ·05 per cent by weight, the following numbers are obtained (assuming $1 \times 10^{14}$ pounds as the unit of measurement):—Weight of atmosphere 110000; oxygen contained 25344, carbonic acid contained 55; weight of coal, 98·112; oxygen required for combustion, 243·59; carbonic acid from combustion of coal, 289·23; total carbonic acid, 344·23; ratio of oxygen to carbonic acid at present, 460·8 to 1; ratio after combustion of assumed quantity of fuel, 72·9 to 1.

The last ratio can of course only be regarded as an approximation, but when we take into account all the available fuel in the world—wood, peat, lignite, coal, anthracite, also the quantity of carbonic acid evolved from volcanic districts—and remembering the opinion of a member of this society, that we know little about the difficulties likely to be encountered in mining operations at a depth of 4,000 feet—there seems little reason to doubt that the ratio of the oxygen to carbonic acid would be reduced considerably below the number above stated, and that the quantity of carbonic acid in the atmosphere would reach a point much beyond that at which it becomes deleterious to human life. It seems then more reasonable to take the alternative and maintain that the carbonic acid will be de-oxidized, and that there will always be an abundance of fuel.

A paper was read entitled "Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes." Part II. By J. C. DYER, V.P.

*The Employment of Steel for Transferring Engravings.*

At the beginning of this century, upon the death of Washington, medals to commemorate that event being called for, Mr. Jacob Perkins (then a silversmith at Newbury Port, near Boston) undertook to supply them, and, as they were required in large numbers speedily, he devised a summary process of transferring the engraved design, from prepared steel dies or stamps, by which he obtained several from one original die, and thus a vast number of medals were rapidly produced. Shortly after Mr. Perkins applied the same principle of transferring engravings for bank notes, on which very elaborate designs were printed to prevent or render their being forged very difficult by the hand of the engraver. To effect this he procured cast steel plates, and decarbonated their surfaces to the depth of about one-sixteenth of an inch, which were thus converted into *very soft* and *pure iron;* the letters and designs for the notes being then engraved upon them they were case hardened and tempered for *use*, but in lieu of printing from these plates they were used as dies for making others to print with. His next process was to prepare a cast steel cylinder, which in like manner was decarbonated at the surface, and then under a strong traversing pressure it was rolled over the letters and figures engraved on the hardened plate, and these engravings were taken up in relief on the surface of the soft cylinder. This cylinder being then hardened and tempered, was used to transfer, by means of the same traversing pressure, the entire work upon its surface, to any number of copper or soft steel plates for printing with.

The adoption of this plan by several banks, for having very elaborate engravings on their notes, turned the counterfeiters upon other banks whose notes would be so much more readily forged, which led to an extended demand by the other banks.

In the year 1809 Mr. Perkins communicated to me the details of his process of transferring engravings, with a view to having the invention patented in England for our joint account. From the success of his plan in America its adoption here was anticipated, and still further development of it looked for from the higher state of the graphic arts in London. With this view I took out patents, and minutely specified "the method of carrying the invention into effect." A very beautiful design was then obtained from the classic pencil of the late Sir R. Smirk, R.A., which was engraved by Reimback, on prepared steel, for printing bank notes. But I could not succeed at that time to induce the Bank of England or any other bank to adopt the plan, nor could the booksellers then be made to perceive the importance of the transferring system for illustrating books, for which it has since been so extensively used. The time had not arrived when public attention could be drawn to the bank note forgeries as a national evil and the disgrace of hanging men for a feat so readily performed as that of forging the one pound notes then in general circulation. If any excuse can be offered for this apathy, it may be said that the passions and interests connected with the war, together with those yet more embarrassing that arose from the transitions from war to peace, caused such disturbances in the circulating medium and in the general interests of commerce and industry, that it became very difficult to awaken public attention to the great scandal of relying solely upon the gallows for preventing forgeries.

It has been above shown that Perkins' invention was not for engraving on steel plates for printing, nor for engraving upon steel at all, but rather for engraving on soft iron of homogeneous structure. It was found that all wrought iron is more or less fibrous and unfit to receive delicate engravings, and that by decarbonating the surface of cast steel a pure iron surface was obtained, and this being engraved on, was case hardened and used for transferring and printing as before stated. This should be kept in view, because many persons have supposed that the invention of Perkins was merely the substitution of steel in the place of copper for engraving upon; such a substitution of the one metal for the other would not be an invention in any fair sense of the word. But his method of obtaining soft iron surfaces to receive the work, converting these surfaces back into steel, and then transferring the engravings to other plates for printing, comprised together a series of novel processes which confer lasting honour upon the inventor.

After the transition period, having better hopes of success, I recommended Mr. Perkins to come over himself to explain his system and aid the artists here in putting it into operation. Accordingly in the year 1820 Mr. Perkins came to England, and being over sanguine, brought a large staff of able artists, mechanics, &c., but he could not bring any money to aid in establishing his intended works in London. He had assumed that capital could always be obtained in England for conducting any safe and profitable schemes. Now the matter of proving his to be such was not easy to establish with the *monied class;* so to me alone, not of that class, he had to look for the entire expenses of his mission, and this I could only bear for a few months. After some time the late Mr. Charles Heath, the eminent engraver, was induced to join Mr. Perkins and become a partner in the engraving works which were then commenced in Fleet-street, and are still continued by their successors.

Besides the printing on paper, Mr. Perkins' system of transferring has been since very extensively employed for calico printing, and in later years we have also seen his process employed to a vast extent in many other departments of the graphic art, such as post office and receipt stamps, and other prints that are required in greater numbers than could be produced by other than steel plates or stamps. His system of engraving on steel has at length become a great artistic power, the wide-spread increase of which has given employment to labour and capital to a vast extent in the several branches of art before stated, and from which I believe many large fortunes have been made, but little other than "toil and trouble" ever accrued to the inventor of them.

When any important discoveries in physical science are made they never die, whatever may chance to their authors. The new facts brought before the public go forth like seeds cast upon a fertile soil, yielding the fruits of continual progress among the families of men who seek improvement. It seems only just then that each generation should transmit to the next some record of the names of those contemporaries to whose genius and talents all nations are indebted for such discoveries. Wherefore, in addition to the four distinguished inventors brought to the notice of this Society in my former papers, I have in the present one aimed to place that of Jacob Perkins as a worthy contributor to the advance of those branches of art to which his inventions have been applied.

## APPENDIX I.

### *On the Compression of Water.*

In tracing the progress of steel engraving I had no thought of giving a general account of Mr. Perkins' researches in physical science, yet it may not be out of place to notice one or two other of his discoveries.

(1) His experiments on the compressibility of water (made some time before he left America) were to test the correctness of the doctrine founded on the Florentine experiments, that water was a non-elastic body, which was then generally taught in the schools and elementary works. At that time

Mr. Perkins had never heard of the experiments of Canton, (made some fifty years before) which had established the compressibility of water. Although by Mr. Perkins' experiments its discovery was not *strictly new* yet they were of high scientific value, because of the widely different compressing forces employed by him and by Canton, the latter having applied the pressure from half an atmosphere to two atmospheres, say from seven and a half to thirty pounds a square inch, whilst that employed by Perkins was from fifty to four hundred atmospheres, or from 750 lbs. to 6,000 lbs. per inch. The same rate of compression appeared in all his experiments, which corresponded with that shown by Canton's experiments, and in all of which the water was compressed in volume directly as the compressing forces.

The apparatus employed by Perkins was first a cast iron cylinder, about three inches thick, with a movable top of equal strength; this, filled with water, had a force pump (as in the hydraulic press) to measure the pressure within by the leverage and size of the induction pipe. 2nd. A small brass cylinder, with a piston to slide in it, water tight, about three quarters of an inch diameter, and to have a column of water ten inches long under the piston. The piston rod, graduated to divisions of a hundred to the inch, had a sliding ring on it, to be pressed upon the rod as it was forced down upon the enclosed water, thus marking in the hundreths of an inch the descent of the rod; the brass cylinder, being under the same pressure inside and out, was not subject to any strain to alter its capacity. 3rd. When the external pressure was removed, the water in the brass cylinder expanding to its original length and raising the piston rod, would of course mark the greatest compression effected. In each experiment the diminished bulk of water was directly as the pressure applied, and under the pressure of 100 atmospheres the bulk of the water was reduced one part in a hundred, and, as before mentioned, this rate proved to be the same as that shown by the experiments of Canton. Some time after Professor Oersted made similar experiments by employing a stout glass vessel and using a column of mercury to give the pressure, having the like inside cylinder as that of Perkins to mark the result, which confirmed the same rate as that shown by Perkins and Canton.

## APPENDIX II.

### *On Perkins' Steam Gun.*

Mr. Perkins conceived the idea of employing steam at a very high pressure for discharging projectiles with greater rapidity and effect than could be done by the common use of gunpowder. To effect this object he devised a plan for heating water more intensely than could be done by any boilers then known, viz.—that of employing a great number of iron or copper tubes, with their ends fastened into plates, with chambers or cavities for receiving the water at one end and emitting the steam at the other.

This apparatus was placed in the midst of a furnace for the heat to act directly on the water in the tubes, and thus, as Mr. Perkins phrased it, the water could be made red hot and flash into steam with a force exceeding that of gunpowder. Then a gun barrel, with its breech opposite the valve opening from the steam chamber, and an apparatus for conducting the balls into the space between the breech of the barrel and the outlet of the steam, and, the valve opening at the same time, the steam issued and propelled the balls through the gun in rapid succession with a force about equal to common powder, which could be continued as long as the heat of the furnace could keep up the pressure. He found that from fifty to a hundred balls per minute were shot forth to a target, about one hundred yards, with a force nearly equal to that of a common musket, and of course by having ten such guns fixed to the same furnace, from 500 to 1,000 balls might be discharged per minute. His experiments were witnessed by the

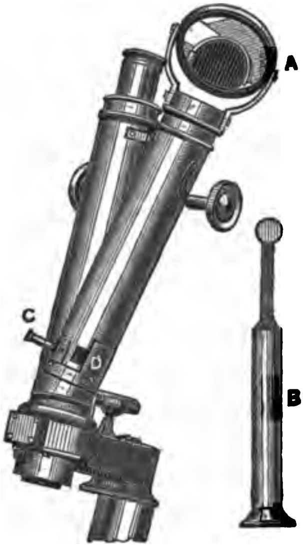
Duke of Wellington and many other eminent men, who took much interest in them.

The non-adoption of his system arose from several radical defects in it—first, the danger from fire of having such a cumbrous furnace on a ship; second, the time required to get up the steam in case of sudden encounter with an enemy, which might lead to a surrender before a shot could be thrown from the steam battery; third, by unequal heating the tubes sometimes gave way, allowing the water to escape, and though the quantity being small would not cause explosions, leaks would deaden the fire and stop the action of the guns, and any suspension in the midst of action must be fatal to the ship using such a weapon. Still the after interest attached to the plan of the tubular boilers came from reversing the scheme used by Perkins, viz.—employing the tubes as flues for the fire, to convey the heat through the tubes to the water surrounding, then, in an outer boiler, by this method, without damaging the tubes, it is found that high pressure steam can be employed with safety and advantage, so that Mr. Perkins' invention was not barren to the outer world, since his tubular boilers led to their extended employment in railway and steamboat engines, and were, I believe, first employed by Stephenson a few years after the steam gun experiments had been put "hors de combat."

## NOTE.

Although it is needless to describe the process of case hardening, so generally known, it may be well to explain that of decarbonising the steel plates for engraving. This process is as follows:—The prepared steel plates are placed in a cast-iron box, and covered about an inch deep with an oxide of iron, prepared by subjecting iron filings to alternate wetting and drying until they are mostly converted into red oxide. Over this covering a clay luting is placed so as to exclude the air, and the box is then placed in a furnace and kept at a red heat for about sixty hours, when the oxide in contact with the steel will have taken up the carbon from its surface to the depth of about a sixteenth of an inch, and thus convert the surface into pure iron, as mentioned in the text.

Engraving of Mr. J. B. Dancer's method of illuminating opaque objects under the high powers of the microscope.



At A is shown a concave mirror, having a vertical and horizontal movement, mounted over the oblique body of a binocular microscope. By this mirror the light is reflected down to the Wenham's prism, and thence through the objective to the object.

*Another method.*

B is a representation of a small speculum fixed at the end of a brass wire. This is inserted into the vertical body just over the fine motion tube at C. The speculum receives light at the side of the microscope at D, and reflects it down through the objective to the object. If there is any obstacle in the way of attaching the small speculum in the vertical body as shown at C, it could be fitted to an adapting ring between the body and the objective.

## PHYSICAL AND MATHEMATICAL SECTION.

December 7th, 1865.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

A paper was read "On the November Meteors, as observed at Woodcroft, Cuckfield, Sussex, November 12–13, 1865," by GEORGE KNOTT, Esq., F.R.A.S., communicated by Joseph Baxendell, F.R.A.S.

The night of November 12th being fine, Mrs. Knott and myself were enabled to watch under favourable circumstances for the meteor-shower, of which warning had been given at the last meeting of the Royal Astronomical Society.

An occasional examination of the sky during the earlier part of the night did not reveal a single meteor, but as a systematic watch was not commenced before midnight, too much weight must not be attached to this circumstance. Our station commanded a clear view of the southern half of the horizon, but towards the north the view was obstructed by the house. Between 12h. and 1h. a.m. we counted 39 meteors, giving an average of rather more than 0·6 per minute; the next 55m. added 61 to the number, giving an average of 1·1 per minute. After half an hour's interval we resumed our watch at 2h. 25m. a.m., and between that hour and 3h. 5m., when we ceased observing, we noted 55 meteors, showing that the average had risen to 1·4 per minute. The observations of the last 40 minutes showed clearly that the *radiant point* was in the immediate vicinity of the star ζ Leonis, or, perhaps, between that star and ε and μ of the same constellation, the neighbourhood in fact of what the Rev. C. Pritchard happily terms the "*apex of the earth's way.*" The paths of a few meteors seemed to suggest a second *radiant point* in the neighbourhood of β Tauri, but the observed flights were too few to afford satisfactory evidence on the point.

We remarked a strong tendency of the meteors *to occur in groups*, four or five, and in some cases more, appearing one after the other in quick succession, followed by a lull, during which none were seen. We did not notice any of *very remarkable brilliancy*, they ranged for the most part from that of stars of the first magnitude downwards, in the majority of instances leaving a train behind them, which in several cases remained visible for some little time after the main body of the meteor had disappeared.

Among such numbers it would hardly be possible, in all likelihood, to identify individuals; I may just notice, however, that at 2h. 42m. 30s. a bright meteor passed precisely over α Orionis, leaving a train which remained visible for a few seconds, on which the star had the curious appearance of being *threaded*. The meteor passed in the direction of γ Orionis, its time of flight being about one second, and the length of its path, of which α Orionis was about the middle point, 10° or 12°. Of course, in the case of an unpractised observer, these data must of necessity be very rough, and I much regret that I was not at the time acquainted with Mr. Herschel's ingenious alphabetic chronometer.

In the last number of the Abbe Moigno's serial "Les Mondes," I find the following observations by M. Coulvier-Gravier;—"Night of the 12–13 November. First hours of the night up to 4 a.m., 237 meteors observed, or about 29·7 per hour; from 4h. to 5h., 96 meteors, or on the average 1·6 per minute; from 5h. to 6h., 43 meteors, or 0·7 per minute." A comparison of these results with those of our own observations, would seem to indicate not only that there was no very material increase in the average number of meteors per minute as the morning advanced, but also that the display was already on the decline before daylight put an end to observation. At the same time it is to be noticed that, according to the observations of M. Coulvier-Gravier, meteors in more than average numbers, indeed very considerably so, were to be seen on the night of the 13–14 November; he observed, "although the sky was almost constantly clouded, 72 meteors, of which 36 appeared between the hours of 4 and 6 a.m., when only 0·2 of the sky was clear." But in any case, so far as present accounts are concerned, it would appear that the display this year was by no means a very extraordinary one.

The position of Mr. Knott's Observatory is Lat. 51° 0′ 41″ N., Long. 0°. 0′. 34″. W.

In the discussion which ensued Mr. Thomas Heelis, F.R.A.S., pointed out the advantage first suggested by Quetelet, in 1841, of recording not only the position of the radiant point but also the mean distance from such point at which the meteors became visible; and Mr. Baxendell concurred in this, stating that he had himself, in the display of 1833, noted that the meteors had apparently lain in at least two strata, each stratum having its peculiar mean distance of apparition from the radiant point.

Mr. Heelis also called attention to the danger which appeared to him to exist of forming a theory of meteors from observations taken from a one-sided point of view, and urged that as the observations of M. Coulvier-Gravier, in France, had led him to the conclusion that all meteors were atmospheric, whilst the form adopted by the British Association had been framed by a committee the members of which regarded all meteors as cosmical, each form should in the interests of truth be so far altered as to inclose, when practicable, not only the particulars already noted, but also some of the atmospheric conditions, both at the time of observation and also at a given time afterwards, which Mr. Baxendell suggested should not be less than 24 hours.

Exception was likewise taken to the exclusion, by the members of the committee, of small meteors from their catalogue.

Mr. Baxendell added that to his mind one of the greatest objections to the cosmical theory was that the constant deposit during thousands of years of the remains of meteors upon the surface of the earth, had not altered the time of diurnal rotation.

The general opinion of the section appeared to be that no one origin ought to be exclusively assigned to meteors, and that both the cosmical and atmospheric theories, if pushed to the exclusion of each other, were wrong.

Mr. BAXENDELL referring to his paper "On the Variable Star S Delphini" (see page 28) read the following extract of a letter he had received from Mr. Knott:—

"I was much interested in your remarks on our different modes of drawing the light-curves of variable stars. You were quite correct in your statement that I have been in the habit of regarding deviations from an even curve as due mainly to errors of observation. I have, however, recently felt that this hypothesis was not quite a satisfactory one, and have been on the point of broaching the question to you once or twice; and the recent projection of observations of R Vulpeculæ has strongly confirmed my suspicions. I enclose a projection of my observations of this star for the last maximum and minimum. You will at once see the marked dislocation in the ascending curve. An even curve would give an apparent error of observation on August 24 amounting to about half a magnitude; yet my light estimates on that day were R Vulpeculæ$=e+2=f=g-3$, the magnitudes of the comparison stars being 9·5, 9·7, and 10·0, and the resulting values of the magnitude of R for the night being therefore 9·7, 9·7, 9·7. It is difficult to imagine I could have made an error of half a magnitude here! The descending curve is, you will see, very regular. It seems to me that this is rather an interesting point in variable star observations, and worthy of some study."

The projection of the observations of R Vulpeculæ referred to in this extract was exhibited to the meeting, and the general impression of the members present seemed to be that the irregularity in the ascending part of the curve could not fairly be attributed to errors of observation.

## Ordinary Meeting, December 26th, 1865.

R. ANGUS SMITH, Ph.D., F.R.S., &c., President,
in the Chair.

Mr. Henry Simpson, M.D., was elected an Ordinary Member of the Society.

Mr. BINNEY F.R.S., F.G.S., exhibited some singular calcareous nodules found in the lower coal seams of Lancashire and Yorkshire, full of beautiful specimens of fossil wood, showing structure even to the smallest striæ of the tubes. These nodules were found in several seams of coal, but were always associated, so far as yet known, with beds of fossil shells lying immediately above them.

In one instance the beds occurred in the following descending order, namely :—

| | Ft. | In. |
|---|---|---|
| 1. Black shale full of shells of the genera *Aviculopecten*, *Goniatites*, *Posidonia*, &c., and containing calcareous concretions enclosing similar shells ................................ | 1 | 6 |
| 1. Seam of caking coal with the nodules containing the fossil plants............................ | 2 | 0 |
| 3. Floor of fire clay and gannister, full of *Stigmaria ficoides* ................................ | 2 | 6 |

The fossil wood is found in nodules dispersed throughout the coal, some being spherical, and others elongated and flattened ovals, varying in size from the bulk of a common pea to eight and ten inches in diameter. In some portions of the seam of coal the nodules are so numerous as to render it utterly useless, and they will occur over a space of several acres, and then for the most part disappear and again occur as numerous as ever. For a distance of twenty-five to thirty miles the nodules occur in this seam of coal in more or less abundance, but always, so far as yet known, containing the same plants. Fossil shells are rarely met with in the nodules found in the coal, but they occur abundantly in the large calcareous concretions found in the roof of the mine, and are there associated with *Dadoxylon* containing *Sternbergia* piths, which plant had not been noticed in the coal, and *Lepidostrobus*. So far as his experience extended, the nodules in the coal were always found associated with the occurrence of fossil shells in the roof, and were probably owing to the presence of mineral matter held in solution in water and precipitated upon or aggregated around certain centres in the mass of the vegetable matter now forming coal before the bituminization of such vegetables took place. No doubt such nodules contain a fair sample of the plants of which the seams of coal in which they are found were formed, and their calcification was most probably in a great measure due to the abundance of shells afterwards accumulated in the soft mud now forming the shale overlying the coal. These shells, on their decomposition, would yield most of the minerals now found in the fossil wood, whilst the surrounding salt water and vegetables would supply the remainder.

The specimen of *Sigillaria vascularis* exhibited was of an irregular oval shape, one foot three inches in circumference, had the ribs and furrows well shown on the outside of the decorticated stem, and afforded evidence of the structure of the original plant from the centre to the circumference. In the middle was a light coloured cylinder of about an inch in diameter, which appeared to be composed of carbonate of lime and carbonate of magnesia. The remainder of the specimen was of a much darker colour. By the kindness of our President an analysis was made in his laboratory, by Mr. Browning, of a fair sample of the bulk of the dark part of the specimen. This gave

| | |
|---|---|
| Sulphates of potash and soda | 1·62 |
| Carbonate of lime | 45·61 |
| Carbonate of magnesia | 26·91 |
| Bisulphide of iron | 11·65 |
| Oxides of iron | 13·578 |
| Silica | 0·23 |
| Moisture | 0.402 |

The minutest vessels of the central axis and the internal radiating cylinder of the plant, with their finely striated sides, were preserved nearly as perfectly as in the living plant, without affording evidence of disarrangement from pressure or chemical change.

From the position where the calcareous nodules occur, namely in the middle of the seam of coal, they must have have been formed when the coal was in a soft and pulpy state and in the same shape and condition in which they are now found, something similar to such nodules in a peat bog of the present day. Instances have been known of hazel nuts placed in a damp calcareous deposit having had all their kernels removed and replaced by carbonate of lime while the woody portion of the nutshell remained little altered, but in this case the form of the starchy granules and original cellular tissue had not been preserved.

From the analysis previously given it is evident that the waters in which the nodules were formed contained a considerable amount of sulphuric acid, probably as much as would act on the cellular tissue and woody fibre of the vegetables so as to convert them into colloids. If this be assumed to be the case, then we might by the laws of liquid diffusion given by Mr. Thomas Graham, F.R.S., Master of the Mint, in his valuable paper printed in the *Philosophical Transactions* for 1861, suppose that the crystalloids now forming the light coloured cylinder in the middle of the specimen could have a free passage from the circumference to the centre, and replace molecule by molecule the particles of the original vegetable, and all its beautiful and delicate structure just as we now see it preserved in the stone. However, before dialysis could be held to account satisfactorily for the phenomena above stated, a good many experiments on recent woods would have to be made, and more attention devoted to the subject than he (Mr. Binney) would be able to give.

The specimens exhibited were a portion of those described in a paper in the *Philosophical Transactions* of this year.

## MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

December 18th, 1865.

J. B. Dancer, F.R.A.S., in the Chair.

Mr. Parry exhibited some sections of fossil wood and Echinus spines, most beautifully cut by Mr. John Butterworth, of Oldham, and presented some of the slides to the Section.

Mr. Parry also presented to the meeting, for distribution among the members, mounted slides of the contents of a shark's stomach, from the Madras coast, consisting almost entirely of Diatomaceæ.

Mr. Hurst then made a few remarks on late improvements in illuminating opaque objects under the higher powers of the microscope. He said they consisted of three different methods. Firstly, that of H. E. Smith, of Kenyon College, America, described in the "English" *Mechanics' Magazine* of the 20th October, 1865, in an extract from the American *Journal of Science and Arts.* This gentleman employed a box, or adaptor, between the object glass and the

Wenham's Prism of the binocular, with a side perforation opposite to which was a small silver reflector or a common thin glass cover, acting as a mirror and capable of adjustment to any angle—thus enabling it to throw the rays of light admitted by the side aperture through the object glass down on to the object itself.

The disadvantage of this method is that all adaptors cause unsteadiness, and however skilfully constructed injure the accurate centering of the object glass, and while on the one hand the thin glass cover appears to produce some distortion of the image, the reflector so near the object necessarily casts off a number of the rays proceeding from it. This plan also seems to require lamp light and the use of a condenser. Messrs. Smith and Beck appear to have patented the use of the thin glass cover.

Secondly, a modification of the foregoing by Mr. Dancer, of this Section, who places the thin glass or reflector between the eyepiece and the Wenham prism, cutting an aperture in the body of the microscope to admit the light. This dispenses with the objection inherent to adaptors, and theoretically seems the most perfect of these new methods; but Mr. Hurst's experience in its use was as yet too limited to form an opinion. He hoped however to report on the subject at the next meeting.

Thirdly, that invented by Mr. Dancer, who places a circular mirror over the oblique tube of the microscope, previously removing the eye piece: the light is thrown down to the Wenham's prism, and thence through the objective on to the object. The only disadvantage of this method was that of not admitting of binocular vision; otherwise its simplicity, cheapness, and great facility of adjustment render it far preferable to the others, while its effects are fully equal to theirs. It answers moreover equally well by day or lamp light, and does not require a condenser. to be used. Mr. Hurst thought every binocular microscope would be fitted with it when their owners had seen its working.

Mr. Hurst wished meanwhile to draw the particular attention of the members to the extraordinary beauty and clearness with which opaque objects—hitherto the despair of microscopists—were displayed by these methods of illumination, some being shown as clearly as if enlarged into a relatively gigantic model and viewed by the naked eye. Another peculiarity connected with them is, that as the object glass itself acts as a condenser, the amount of light is increased with the magnifying power of the object glass, contrary to the effect of other modes of illumination.

Mr. Hurst thought the subject was in its infancy and that great improvements would yet be made, but that the idea of Mr. H. E. Smith, of making the object glass its own illuminator, would prove to be one of the greatest steps in modern microscopic science, and, as improved upon by Mr. Dancer, it was one so costless in price and rapid in its adjustment that every microscopist, however economical either of time or money, could readily avail of its assistance.

Mr. Coward then exhibited some interesting plants from India, illustrating abnormal forms of different natural families, especially of Leguminoseæ.

## Ordinary Meeting, January 9th, 1866.

R. Angus Smith, Ph.D., F.R.S., &c., President, in the Chair.

Mr. **Eddowes Bowman**, M.A., gave an extended series of striking illustrations of the principal phenomena of polarised light.

## PHOTOGRAPHICAL SECTION.

### December 14th, 1865.

J. P. Joule, LL.D., F.R.S., Vice-President of the Section, in the Chair.

Mr. **Joseph Sidebotham** read a paper "On the Application of Measuring Rods to Photographic Pictures."

The author referred to a paper on this subject read last Session (*Proceedings*, vol. iv , p. 113), in which he advocated the use of graduated rods being placed in certain positions in buildings or landscapes of which photographs were to be taken, whereby correct measurements might be afterwards made on the finished picture. He now briefly alluded to certain corrections which would be necessary before these measurements could be considered perfectly accurate.

Professor C. Piazzi Smyth had made great use of this plan during his investigations at the great pyramid last winter, and had kindly allowed a selection of forty of his pictures to be exhibited to the members as illustrations. The pictures were exhibited by the oxyhydrogen light on the screen, and were admirable photographs. They consisted of a series of views of the exterior and interior of the tomb of King Shafra, recently excavated near the great sphynx. A series of four of these, taken to show the correct orientation of one of the passages, are very remarkable, having been taken two minutes before twelve, twelve o'clock, and two minutes past, true astronomical time. Views were also exhibited of the entrance to the great pyramid, the socket in which the corner stone had rested, also views of the niche in Queen's Chamber, and the mysterious coffer in the so-called King's Chamber, the object of so much interest and speculation. These interior views were taken by the aid of the magnesium light, and, considering the many difficulties to be overcome, are very good photographs. The divisions on the measuring rods surrounding the coffer are exceedingly plain, and by the application of a pair of compasses a tolerably correct measurement may be obtained.

A paper was then read "On Celestial Photography," by A. Brothers, F.R.A.S.

The credit of having produced the first photograph of a celestial object is generally given to the late Mr. Bond, of Cambridge, U.S.; but it appears from a paper by Professor H. Draper, of New York, published in April, 1864, that in the year 1840 his father, Dr. J. W. Draper, was the first who succeeded in photographing the moon. Dr. Draper states that at the time named (1840), "it was generally supposed the moon's light contained no actinic rays, and was entirely without effect on the sensitive silver compounds used in Daguerreotyping." With a telescope of five inches aperture Dr. Draper obtained pictures on silver plates, and presented them to the Lyceum of Natural History of New York. Daguerre is stated to have made an unsuccessful attempt to photograph the moon, but I have been unable to ascertain when this experiment was made.

Mr. Bond's photographs of the moon were made in 1850. The telescope used by him was the Cambridge (U.S.) refractor of fifteen inches aperture, which gave an image of the moon at the focus of the object glass two inches in diameter. Daguerreotypes and pictures on glass mounted for the stereoscope were thus obtained, and some of them were shown at the Great Exhibition of 1851, in London. Mr. Bond also proved the advantage to be derived from photographs of double stars, and found that their distances could be measured on the plate with results agreeing well with those obtained by direct measurement with the micrometer.

Between the years 1850 and 1857 we find the names of Father Secchi in Rome, and MM. Berch and Arnauld in France; and in England, Professor Phillips, Mr. Hartnup, Mr. Crookes, Mr. De la Rue, Mr. Fry, and Mr. Huggins. To these may be added the name of Mr. Dancer, of Manchester, who, in February, 1852, made some negatives of the moon with a four and a-quarter inch object glass. They were small, but of such excellence that they would bear examination under the microscope with a three-inch objective, and they are believed to be the first ever taken in this country. Mr. Baxendell and Mr. Williamson, also of Manchester, were engaged about the same time in producing photographs of the moon.

The first detailed account of experiments in celestial photography which I have met with is by Professor Phillips, who read a paper on the subject at the meeting of the British Association at Hull in 1853. Professor Phillips says :—" If photography can ever succeed in portraying as much of the moon as the eye can see and discriminate, we shall be able to leave to future times monuments by which the secular changes of the moon's physical aspect may be determined. And if this be impracticable—if the utmost success of the photographer should only produce a picture of the larger features of the moon, this will be a gift of the highest value, since it will be a basis, an accurate and practical foundation of the minuter details, which, with such aid, the artist may confidently sketch." The pictures of the moon taken by Professor Phillips were made with a six and a-quarter inch refractor by Cooke. It is of eleven feet focus, and produces a negative of one and a-quarter inches diameter, in thirty seconds. Professor Phillips does not enter very minutely into the photographic part of the subject, but he gives some very useful details of calculations as to what may be expected to be seen in photographs taken with such a splendid instrument as that of Lord Rosse. It is assumed that an image of the moon may be obtained *direct* of twelve inches diameter, and this when again magnified sufficiently would show "black bands twelve yards across." What may be done remains to be seen, but up to the present time the Professor's anticipations have not been realised.

We have next, from the pen of Mr. Crookes, a paper communicated to the Royal Society of London, in December, 1856, but which was not read before that society until February in the following year. Mr. Crookes appears to have obtained good results as early as 1855, and, assisted by a grant from the Donation Fund of the Royal Society, he was enabled to give attention to the subject during the greater part of the year following. The details of the process employed are given in the paper with much minuteness. The telescope used was the equatorial refractor at the Liverpool Observatory, of eight inches aperture, and twelve and a-half feet focal length, producing an image of the moon 1·35 inch diameter. *The body of a small camera* was fixed in the place of the eyepiece, so that the image of the moon was received in the usual way on the ground glass. The chemical focus of the object glass was found to be eight-tenths of an inch beyond the optical focus, being over-corrected for the actinic rays. Although a good clock movement driven by water power is applied to the telescope, it was found necessary to follow the moon's motions by means of the slow-motion handles attached to the right ascension and declination circles, and this was effected by using an eyepiece, with a power of 200 on the finder, keeping the cross-wires steadily on one spot. With this instrument Mr. Hartnup had taken a large number of negatives, but owing to the long exposure required he was not successful; but with more suitable collodion and chemical solutions, and although the temperature of the observatory was below the freezing point, Mr. Crookes obtained dense negatives in about four seconds. Mr. Crookes afterwards enlarged his negatives twenty diameters, and he expresses his opinion that the magnifying should be conducted simultaneously with the photography by having a proper arrangement of lenses, so as to throw an enlarged image of the moon at once on the collodion plate; and he states that the want of light could be no objection, as an exposure of from two to ten *minutes* would not be "too severe a tax upon a steady and skilful hand and eye."

In an appendix to his paper Mr. Crookes gives some particulars as to the time required to obtain negatives of the moon with different telescopes, from which it appears that the time varied from six minutes to six seconds. The different results named must, I conclude, have been caused not so much by the differences in the instruments as in the various processes employed, and in the manipulation. I must observe, also, that it is not stated whether all the experiments were tried upon the *full* moon, a point materially affecting the time.

Mr. Grubb read a paper on this subject before the Dublin Photographic Society on May 6th, 1857. After referring to the fact that he found the actinic focus of his object glass to be longer than the visual (thus agreeing with Mr. Crookes) he states it to be generally understood that in a compound object-glass made as nearly achromatic as possible, the actinic focus is shorter than the visual. The most valuable portion of Mr. Grubb's paper is the suggestion for a piece of apparatus to be attached to the part connected with the telescope for holding the dark frame, which he proposes may be so arranged as to follow the moon's motion in declination; and he gives the following description of a contrivance used by Lord Rosse, and which is suitable for telescopes not equatorially mounted:—"On a flat surface attached to the telescope, and parallel to the plane of the image is attached a sliding plate, the slide being capable of adjustment to the direction of the moon's path at the time of operating. The slide is actuated by a screw moved by clockwork, and having a governor or regulator of peculiar construction, which acts equally well in all positions. The clockwork being once adjusted requires no change; but the inclination of the slide must be effected by trial for the moon's path at the time of taking the photograph." This idea originated with Mr. De la Rue, Lord Rosse's share in it arose from his having applied a clock motion to the apparatus.

The telescope used by Mr. Grubb is $12\frac{1}{10}$ inches aperture and twenty feet focus, giving an image $2\frac{1}{16}$ inches diameter in from ten to forty seconds.

The next contribution on this subject is by Mr. Fry, who, in 1857, commenced his experiments on the moon with an equatorial telescope, the property of Mr. Howell, of Brighton. The object glass of this instrument is eight and a-half inches diameter and eleven feet focus, and gave an image of the full moon in about three seconds, but under very favourable circumstances a negative was made in a single second. The size of the image is not stated, but it must have been about one and a-quarter inches diameter. Mr. Fry appears to have removed the eyepiece of the telescope, and in its place a board was fixed having a screw adjustment, so that a plate-holder could be moved backwards and forwards on the board (graduated to tenths of an inch) for the purpose of finding the actinic focus, which was three quarters of an inch beyond the visual. He found that this position of the chemical focus was variable, owing, as he thought, to the varying distance of the moon from the earth, but, as suggested by Mr. De la Rue, it might arise from the length of the telescope tube having altered through change of temperature.

In 1858 Mr. De la Rue read an important paper before the Royal Astronomical Society, from which it appears that the light of the moon is from two to three times brighter than Jupiter, while its actinic power is only as six to five, or six to four. On December 7th, 1857, Jupiter was photographed in five seconds and Saturn in one minute, and on another occasion the moon and Saturn were photographed just after an occultation of the planet in fifteen seconds.

The report of the council of the Royal Astronomical Society for 1858 contains the following remarks:—

"A very curious result, since to some extent confirmed by Professor Secchi, has been pointed out by Mr. De la Rue, namely, that those portions of the moon's surface which are illuminated by a very oblique ray from the sun possesses so little photogenic power, that, although to the eye they appear as bright as other portions of the moon illuminated by a more direct ray, the latter will produce the effect called by photographers, solarisation, before the former (the obliquely-illuminated portions) can produce the faintest image."

And the report also suggests that the moon may have a comparatively dense atmosphere, and that there may be vegetation on those parts called seas.

At the meeting of the British Association at Aberdeen, in 1859, Mr. De la Rue read a very valuable paper on celestial photography. An abstract of this paper was published at the time in THE BRITISH JOURNAL OF PHOTOGRAPHY, and in August and September of the following year further details of Mr. De la Rue's method of working were given in the same Journal. The processes and machinery employed are so minutely described that it is unnecessary here to say more than that Mr. De la Rue commenced his experiments about the end of 1852, and that he used a reflecting telescope of his own manufacture of thirteen inches aperture and ten feet focal length, which gives a negative of the moon averaging about $1\frac{1}{10}$th of an inch in diameter. The photographs were at first taken at the side of the tube after the image had been twice reflected. This was afterwards altered so as to allow the image to pass direct to the collodion plate, but the advantage gained by this method was not so satisfactory as was expected. In taking pictures at the side of the tube, *a small camera box* was fixed in the place of the eyepiece, and at the back a small compound microscope was attached, so that the edge of a broad wire was always kept in contact with one of the craters on the moon's surface, the image being seen through the collodion film at the same time with the wire in the focus of the microscope. This ingenious contrivance in the absence of a driving clock was found to be very effectual, and some very sharp and beautiful negatives were thus obtained. Mr. De la Rue afterwards applied a clockwork motion to the telescope, and his negatives taken with the same instrument are as yet the best ever obtained in this country.

The advantage of the reflecting over the refracting telescope is very great, owing to the coincidence of the visual and actinic foci, but it will presently appear that the refractor can be made to equal if not excel the work of the reflector.

Mr. De la Rue's paper (as published in the report of the British Association,) contains some extremely interesting particulars as to the mode of obtaining stereoscopic pictures of the moon, and diagrams are given showing the effects of the moon's libration. The most beautiful stereoscopic prints of the moon are those by Mr. De la Rue. Mr. Fry also was very successful in this branch of celestial photography.

In this brief history of the subject of celestial photography, I have not referred to anything which has been done in making photographs of the solar spots, but the matter must not be altogether passed over. The first step in this direction appears to have been taken in France, in 1845, by MM. Fizeau and Foucault, but it is chiefly due to the efforts of Mr. De la Rue that so much useful work has been done in heliography. In 1860 Mr. De la Rue and his staff of assistants performed one of the greatest feats yet recorded in this branch of the art of photography, having succeeded in obtaining several beautiful negatives of the various phenomena seen only during total eclipses of the sun, and two negatives were obtained during the totality. One question of much interest to astronomers was determined by this great experiment. The red prominences or flames generally seen as issuing from the edge of the moon were proved to belong to the sun. Photographs of the sun are taken daily when the weather is favourable at the Kew Observatory, and also by Professor Selwyn, at Ely. With the Kew photoheliograph pictures of the sun spots have been made on the scale of three feet to the sun's diameter. Much, however, remains to be done. The light of thesun is much in excess of what is required to obtain a collodion picture, so that the loss of light consequent on the necessary interposition of lenses and the distance of the plate from the instrument can be no objection; and for these reasons I have very little doubt that with apparatus suitably arranged photographs of spots and groups of spots will be obtained of very much larger diameter than any yet taken.

The *Quarterly Journal of Science* for April, 1864, contains the next important paper on celestial photography. It is by Dr. Henry Draper, one of the Professors at the New York

University. On his return to America, after paying a visit to Parsonstown, where he had the advantage not only of making some observations with Lord Rosse's large reflector, but also of seeing the method there pursued in grinding and polishing mirrors, stimulated by what he had seen, it was determined to build an observatory, and to construct an instrument to be devoted solely to celestial photography. The speculum used by Dr. Draper is fifteen and a-half inches in diameter, and twelve and a-half feet focal length; but this was afterwards superseded by one of glass on Foucault's principle. The great labour involved in a work of this character may be judged of by the fact that Dr. Draper ground and polished more than 100 mirrors, varying in diameter from nineteen inches to a quarter of an inch; but he appears at last to have secured a good instrument. The chief points to be noticed in this article are, that instead of driving the telescope in the usual way by means of a clock, the frame carrying the glass plate was made to move on the plan previously referred to. Instead of clockwork to effect this motion, an instrument called a "clepsydra" was used. It has a weight and a piston rod, which fits into the cylinder filled with water, which is allowed to escape by means of a stopcock, and can be regulated with great exactness, so as to follow the object. The large number of 1,500 negatives are stated to have been taken at this observatory, some of which would bear magnifying twenty-five diameters (the paper says *times*, but I assume this to be an error, as a negative must be very bad if it will not bear more than five diameters, or twenty-five times.) As the average size of the negatives was $1\frac{4}{10}$ inch, an increase of twenty-five diameters would give an image of the moon nearly three feet in diameter. I have not seen the prints from these negatives, and have never heard anything of the quality of the work produced by this telescope; but it may be stated that Dr. Draper writes as if the negatives were of the best quality, and encourages others to follow his example.

Nearly a quarter of a century has elapsed since the moon was first photographed in America, and our friends on that side of the Atlantic have not been idle in the interval. To an American gentleman we are indebted for the best pictures of our satellite yet produced, and it is difficult to conceive that anything superior can ever be obtained; and yet with the fact before us that Mr. De la Rue's are better than any others taken in this country, so it may prove that even the marvellous pictures by Mr. Rutherford may be surpassed.

Mr. Rutherford appears, from a paper in the *American Journal of Science* for May of the present year, to have begun his work in lunar photography in 1858 with an equatorial of eleven-and-a-quarter inches aperture, and fourteen feet focal length, and corrected in the usual way for the visual focus only. The actinic focus was found to be seven-tenths of an inch longer than the visual. The instrument gave pictures of the moon, and of the stars down to the fifth magnitude, satisfactory when compared with what had previously been done, but did not satisfy Mr. Rutherford, who, after trying to correct for the photographic ray by working with combinations of lenses inserted in the tube between the object glass and sensitive plate, commenced some experiments in 1861 with a silvered mirror of thirteen inches diameter, which was mounted in a frame and strapped to the tube of the refractor. Mr. Rutherford enumerates several objections to the reflector for this kind of work, but admits the advantage of the coincidence of foci. The reflector was abandoned and a refractor specially constructed of the same size as the first one, and nearly of the same focal length, but corrected only for the chemical ray. This glass was completed in December last, but it was not until March 6th of the present year that a sufficiently clear atmosphere occurred, and on that night the negative was taken from which the prints were made, and through the kindness of Dr. Roscoe I now have the pleasure of showing them to you.

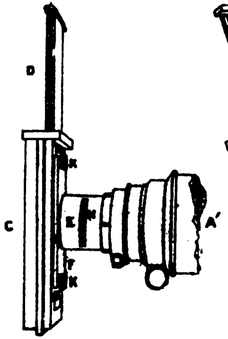
I have entered somewhat minutely into particulars of what has been done in this branch of our favourite art, in order that we may have before us a kind of summary or index of the work done up to the present time, so that those who desire further information may at once refer to the authorities quoted. It may be asked why it was thought necessary to draw up this paper, as Mr. De la Rue and others have said almost all that is necessary to enable anyone to take up the subject and to pursue it sucessfully.

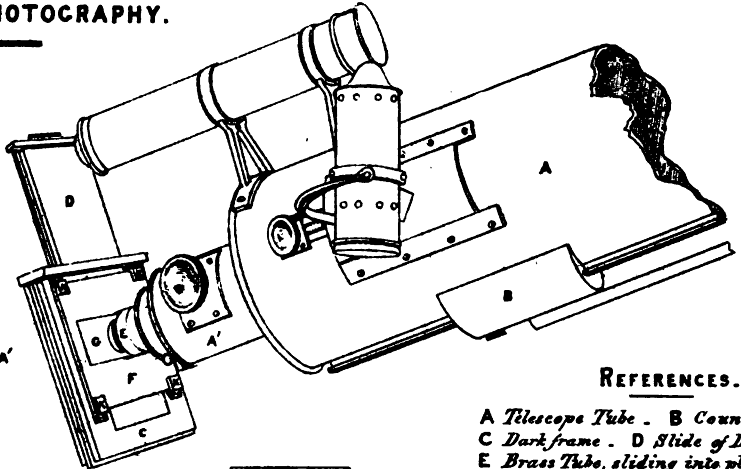
It is true there are very elaborate papers, and from their perusal I have derived much useful information, but at the same time it must be confessed their very elaborateness deterred me, for a long time after I possessed the necessary apparatus, from commencing the experiments which have since afforded me so much enjoyment.

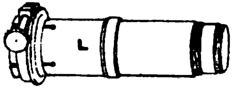
Every writer on this subject speaks of the difficulties encountered from optical, instrumental, and atmospheric causes; and to this may be attributed the fact that we have so few of our amateur astronomers giving their attention to the subject. Another reason may be that comparatively few of those who possess telescopes may have the necessary photographic knowledge; but surely some friend having this knowledge might be found who would be willing to spare a few hours occasionally to assist in taking negatives of the stars, planets, or of the moon. The reason, then, why this subject is brought before your notice this evening is, that it is believed that the apparatus I use is, in some particulars, more simple than any heretofore described, and as it can be used with any kind of telescope, a greater number of amateurs than are now engaged in it may be induced to follow this fascinating branch of photography.

It will have been noticed that when particulars of the apparatus have been given the writer has spoken of a *small camera,* which has been fixed at the eye piece end of the telescope. As to how this was effected I have seen no description,

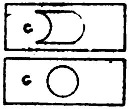
## CELESTIAL PHOTOGRAPHY.







BARLOW'S LENS.



DIAPHRAGMS.

### REFERENCES.

A *Telescope Tube*. B *Counterpoise Weight*.
C *Dark frame*. D *Slide of Dark frame*.
E *Brass Tube, sliding into place of Eye-piece*.
F *Plate of Metal screwed to* E. G *Diaphragm*.
H *Opening in tube for Diaphragm*. K *Clips for fixing* C *to* E. L *Barlow's Lens, sliding into place of Eye-piece*.

*Drawn & Photolithographed by*
A. BROTHERS, *Manchester*.

and as no camera box is required I need not enter into any supposition as to the mode in which it may be done. Before deciding what was necessary to be done, it occurred to me that the telescope tube itself is the camera, and all that was required was the means of fixing the dark frame or plate holder. If the telescope be pointed to the moon, the eye piece removed, and a piece of ground glass held between the eye and the aperture, the image will be seen on the glass, and we require then the means of holding the sensitive plate steadily near the same place. All that is needed is a brass tube about four or five inches long, of the size exactly fitting the tube of the telescope in the place of the eyepiece. In some cases the sliding tube of the eyepiece may be unscrewed and used for this purpose. At one end of this tube a thread is cut and is made to screw into a piece of metal plate (in the centre of which is a circular aperture of the same size as the tube), of the same dimensions as the dark frame. Attached to the plate holder are clips accurately fitting the brass plate, but so that the frame will easly slide off and on without disturbing the telescope. This is all the additional apparatus required to enable photographs of the moon or any other celestial object to be made. A separate frame for the ground glass is not necessary: it must be cut to fit the dark frame, and while in use can be held by slight springs fixed inside the frame at the sides.

The accompanying photo-lithographed copy of a drawing shows the arrangement of the apparatus when in its place for taking a negative, and renders further description unnecessary. The method for ascertaining the actinic focus may be stated in a few words. With the rack motion adjust the focus for distinct vision on the ground glass, and then mark the tube E, and also the sliding part of the telescope. Although very unlikely to be of the slightest use, unless taken with a reflecting telescope, a picture may now be taken; it will at least serve to give some idea of the proper exposure.

If the chemical and visual foci are not coincident the image will have a blurred appearance. Before exposing the next plate, turn the adjusting screw so as to lengthen the tube about the 16th of an inch, and so proceed until, by the greater distinctness of the image, it is seen that the chemical focus is found. At every change of the focus a slight mark should be made on the tube, and when the true focus is satisfactorily determined the marks should be made distinctly visible; and in all future experiments with the same instrument the focus will be always at or very near the same place. Should it be found that the indistinctness increases, it will of course be necessary to try in the other direction.

The appearances arising from atmospheric disturbances are very much the same as when the object is out of focus: experience alone will enable the operator to determine from which cause the defect proceeds.

It is assumed that the telescope is provided with a driving clock; when such is the case every care should be taken that all the parts are clean, and when necessary oiled or greased, so that the motions may be as smooth as possible.

In photographs of the moon in the phases, prior to and after the full, the side opposite to the sun is always too light, or "burnt up," while those parts near the terminator are often so dark that only the tops of the craters and peaks are visible, although in the telescope a clear and bright image can be seen. The cause of this must be that the exposure, if continued long enough to bring out all the eye can see on the darker side, would entirely obliterate the details on the brightly illuminated portions of the moon's surface. Mr. De la Rue's suggestion as to why the dark side of the moon has so little actinic effect, has been already referred to. I would suggest that, as the light of the full moon is 100,000 times weaker than that of the sun, the *twilight*[*] on the moon's surface must be very much less, and consequently the actinic effect of the light is lessened in the same way as at a corresponding time on the earth.

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[*] In the absence of an atmosphere on the moon there can be no *twilight* such as we have on the earth. The meaning, therefore, of this sentence may

The question, then, of photographing the terminator is only one of time, and in order to remedy the defect spoken of to some extent, I have used diaphragms such as are shown in the drawing. In the tube E openings are made on opposite sides, and wide enough to admit the diaphragms to be used without touching the tube. The diaphragm must be of the proper length and width to shut off the moon's light until the plate is ready for exposure. The shape of the diaphragm will suggest which form should be used according to the moon's age. The exposure should be made with the full aperture for as many seconds as previous experiments have proved to be necessary for the bright side, and the diaphragm then gently moved and kept in motion, gradually approaching the darkened side. By this means the exposure may be regulated, and the great differences in the light and dark sides of the moon may be modified.

As to the processes employed, each experimenter must adopt the one he finds in his hands gives the best result. It seldom happens that two operators can produce the same effects with, *apparently*, the same chemicals. Experience has shown me that the ordinary patent plate glass (carefully selected, so as to be free from scratches and other defects,) is preferable to the white patent plate, having found that after a time the surface becomes covered with a kind of dew or "sweat," as it is termed, owing to the decomposition of some of the salts used in the manufacture. The collodion used was made for me by Messrs. Huggon and Co. of Leeds; it is very quick, free from structure, and suitable for iron development.

be misunderstood. It was intended to say that on those parts of the moon, enlightened only by the oblique rays of the sun, the light is so diminished that the actinic effect is lessened as it is on the earth shortly before sunset and during twilight, when it is well known that a much longer time is required to obtain a photograph.

I prefer to develope with an iron solution, using only sufficient to cover the plate; and with this developer and collodion, when the plate has been properly exposed, a negative can be obtained which will not require intensifying afterwards. Not having had sufficient experience with pyrogallic acid I cannot speak with confidence as to any advantage it may possess in giving fine texture to the negative. With the bath and collodion exactly in the proper state, there is no doubt that with this acid negatives may be had as quickly as with iron; but it is extremely difficult to have everything constantly in the best working order. Unless the greatest attention be given to this matter, the time of exposure is so much increased that iron, for this reason, must have the preference.

Upon the character of the image after development entirely depends the value of the enlargement to be made from it, and in this direction there is much room for improvement. Even in the best negatives yet made defects from this cause are very apparent. The microscopic photographs by Mr. Dancer have the finest texture, and will consequently bear greater magnifying than any other photographs I have ever seen, but the process by which they are made is not published.

The weather in this country is so very uncertain, and success in this branch of photography is so entirely dependent on the state of the atmosphere, that it is necessary to be always prepared to take advantage of a favourable night. I have a small cupboard placed in a convenient part of my house where there is a supply of water, and the temperature is always much above the air outside. This cupboard is just large enough to hold a small glass bath fixed at the proper angle ready for use, also the bottles for collodion, bath, developing and fixing solutions, and other little requisites. This arrangement is so convenient that when there is a prospect of getting a negative I can set the telescope, prepare the plate, and take a negative in less than ten minutes. But when there is a chance of two or three hours' work an assistant is desirable, as the best results can only be obtained when one's attention is chiefly devoted to the careful adjustment of the apparatus connected with the telescope.

The convenience of the plan adopted may be judged of by the fact that on the evening of the partial eclipse of the moon, the 4th October last, in four hours I succeeded with the help of two assistants in taking no less than twenty negatives, and the telescope was several times disturbed to oblige friends who desired to see the progress of the eclipse through the instrument, but the apparatus was quickly re-adjusted, although possibly in some cases with slight loss of definition in the negative, through haste. At a previous meeting, I described how these negatives were made, but it may be interesting to refer to the fact that while the fifteenth of the series was taken the telescope was at rest. The clock had been disconnected for re-adjustment and it was forgotten when the plate was ready for exposure, consequently the moon had moved partly off the plate, and the negative shows a portion only; but the exposure was so short that the eye fails to detect any difference in the sharpness of this and the others, which were all taken when the clock had been watched and carefully regulated for the moon's motion. This fact is, I think, of some interest, as it shows that about the time of full moon when the light is of the greatest intensity, pictures may be made with telescopes not equatorially mounted.

My telescope is a refractor of five inches aperture and six feet focal length, giving an image of the moon averaging about eleven sixteenths of an inch in diameter. The actinic focus is one tenth of an inch longer than the visual. The object glass is of Munich manufacture, and is mounted by Mr. Dancer on the Sisson's or English plan with double polar axis. The hour circle is twenty-six inches in diameter, and is used also as the driving wheel, having teeth cut in the edge, in which a screw works connected with the driving clock by a rod, and which can be instantly disconnected by means of a cam. The object glass is an excellent one, and the mounting is everything that can be desired.

The negative taken direct in the telescope is but one step towards what we require, that is, the enlarged copy on paper. From the small negative a positive on glass must be made, say of twice or three times the diameter of the original. It will be quite unnecessary here to explain how the enlargement is to be made; but I may remark that the negative should be placed with the film side towards the copying lens, and the resulting positive copy must also be placed in the same way. The enlarged copy or negative will then give the true telescopic appearance of the moon. In the print of the full moon by Mr. Rutherford a mistake has been made, arising from the negative or positive copy having been placed the wrong way, and consequently the moon looks as if it had been photographed from the opposite side. The print is a very beautiful one in other particulars, but entirely worthless as a picture of the moon, as the eye can never see it as there represented.

I have sometimes taken two negatives on the same plate. It will be seen in the drawing that the dark slide is not quite central with the telescope, so that by reversing the plate after one exposure a second picture can be taken. In photographing the planets Mr. De la Rue has allowed the object to move on for a few seconds, the telescope meanwhile being at rest, and thus four or five negatives can be taken in a very short time on the same plate. It has occurred to me that by having a frame made "landscape way" instead of upright, and in place of having four clips such as K, there might be a kind of groove at top and bottom, so that after taking the first negative, and the light shut off, by moving the plate about an inch, at least three negatives might be taken on the same plate—or a "shifting back" might be adapted. The advantages of this plan are that different exposures might be tried, and the development continued for the one or two which promised the best results. This method would effect a great saving of time, which on a fine night is of much importance.

With the Barlow lens I have made some negatives which have shown that when the same care has been taken to find the actinic focus negatives of a much larger size may be made, and in a very short time. The image is increased from eleven-sixteenths to one and a quarter inch, and the time of exposure at full moon was two seconds. The fittings of the lens are so arranged that three different sized negatives may be taken.

There are several other matters which it might have been desirable to refer to had time permitted, but they must stand over to a future occasion.

Mr. **Baxendell** said that some years ago Dr. Joule had obtained several beautiful negatives of the sun. The time of exposure was about $\frac{1}{500}$ of a second, and the apparatus for admitting and cutting off the light was not attached to the telescope, as in the Kew heliograph, but was mounted on a separate stand.

## PHYSICAL AND MATHEMATICAL SECTION.

January 4th, 1866.

E. W. **Binney**, F.R.S., F.G.S., President of the Section, in the Chair.

Mr. G. V. **Vernon**, F.R.A.S., M.B.M.S., was elected Honorary Secretary of the Section.

Mr. **Vernon** communicated the following "Remarks on the Barometric Disturbances during the Months of October, November, and December, 1865."

At the last meeting of the Physical Section attention was called to the great barometric disturbances in October and November of this year; and in order that their character may be seen at a glance I have drawn out my observations made twice daily in the subjoined curves.

The amount of oscillation in October was not remarkable for its excessive amount, but for the very sudden changes which took place towards the end of the month, say from the 23rd to the 31st.

For November the reading was high until the 15th, when a sudden change commenced, the great depression of the 22nd showing the extremely low reading of 28·240 inches. The amount of oscillation during the first half of the month only amounted to about half an inch per week, leaving more than six inches for the amount of oscillation during the last half of the month, against an average of 5·851 inches for the last seventeen years for the entire month.

The amount of oscillation for the two months of October and November for the last five years, in continuation of the values given in my paper in vol. i., 3rd series, of the Memoirs of the Society, are as follows: —

| | OCTOBER. | | | NOVEMBER. | | |
|---|---|---|---|---|---|---|
| Year. | Number of Oscillations | Amount of Oscillation. | Rainfall. | Number of Oscillations | Amount of Oscillation. | Rainfall. |
| | | Inches. | Inches. | | Inches. | Inches. |
| 1861 | 13 | 5·087 | 1·230 | 15 | 7·491 | 3·878 |
| 1862 | 18 | 6·569 | 5·035 | 13 | 5·323 | 1·685 |
| 1863 | 12 | 4·648 | 6·242 | 14 | 7·496 | 2·904 |
| 1864 | 14 | 5·110 | 1·898 | 20 | 8·504 | 3·255 |
| 1865 | 14 | 6·361 | 5·005 | 15 | 7·390 | 2·770 |
| Means of 17 Years.. | 15·5 | 5·851 | | 13·8 | 5·804 | |

In the paper last alluded to I pointed out at page 3 that an excessive amount of rainfall was accompanied also by a large excess of barometric oscillation in every month but October, stating also that perhaps a longer period of observation might remove this apparent anomaly. The last five years, however, give exactly the same abnormal values for October, as reference to the small table annexed will show. October, 1863, with the least barometric oscillation, had the largest rainfall out of the five years. November conforms to the law, which applies to all the months but October, the table showing that generally the rainfall increased with the amount of oscillation. Of course leaving 1863 out of the question would make October conform to the rule also; but it is these very abnormal months which are so difficult to account for, and which seem to bear out Mr. Baxendell's suggestion of some secondary disturbing cause operating during the month of October especially. (Vide Mr. Baxendell's paper on "Periodic Disturbances of Atmospheric Pressure," Memoirs, 3rd series, vol. i. p. 263.)

The amount of oscillation in December was 5·468 inches, the oscillations being thirteen in number. This is somewhat below the average, although the month was marked by such wide barometric readings as shown by the diagram.

The following Table of Rainfall for 1865 was also communicated by Mr. VERNON.

### OLD TRAFFORD, MANCHESTER.

Rain gauge 3 feet above the ground, and 106 feet above the sea.

| Quarterly Periods. | | 1865. | Fall in Inches. | Average of 72 Years. | Differences: | No. of Days' Rainfall in 1865. | Quarterly Periods. | | Quarterly Periods for 72 Years. |
|---|---|---|---|---|---|---|---|---|---|
| 1864. | 1865. | | | | | | 1865. | 1864. | |
| Days. | Days. | | Inches. | Inches. | Inches. | | Inches. | Inches. | Inches. |
| | | Jan. ... | 3·112 | 2·469 | +0·643 | 18 | | | |
| 38 | 48 | Feb. ... | 2·357 | 2·379 | —0·022 | 17 | 7·143 | 7·722 | 7·149 |
| | | March. | 1·674 | 2·301 | —0·627 | 13 | | | |
| | | April... | 1·082 | 2·025 | —0·943 | 8 | | | |
| 39 | 36 | May ... | 3·187 | 2·368 | +0·819 | 21 | 5·226 | 7·722 | 7·279 |
| | | June ... | 0·957 | 2·886 | —1·829 | 7 | | | |
| | | July ... | 2·996 | 3·561 | —0·565 | 15 | | | |
| 45 | 36 | Aug. ... | 3·840 | 3·596 | +0·244 | 18 | 7·502 | 8·063 | 10·357 |
| | | Sept ... | 0·666 | 3·200 | —2·534 | 3 | | | |
| | | Oct. ... | 5·005 | 3·834 | +1·171 | 20 | | | |
| 49 | 44 | Nov. ... | 2·770 | 3·464 | —0·694 | 16 | 8·518 | 7·133 | 10·523 |
| | | Dec. ... | 0·743 | 3·225 | —2·482 | 8 | | | |
| 171 | 164 | | 28·389 | 35·308 | —6·919 | 153 | 28·389 | 30·640 | 35·308 |

The rainfall for 1865 has been 6·919 inches below the average of the last seventy-two years, and 2·251 inches below the fall for 1864, which was also comparatively a dry year. Although the first three months had about an average fall, the second three months had a fall greatly below the average. The same remark applies to the third and fourth quarters of the year. The fall of rain was remarkably small in June, September, and December; and it will also be seen that with the exception of the four months of January, May, August, and October, every month had a rainfall below the average. As compared with any other month, the rainfall in October was excessive. The number of days upon which rain fell was also greatly below that of many previous years.

Mr. BAXENDELL, F.R.A.S., read the following "Note on the Variable Star T Aquilæ."

In the communication made to this Section on the 12th of November, 1863 (*Proceedings.* vol, iii. p. 195), announcing the discovery, at Mr. Worthington's observatory, of the variability of T Aquilæ, I gave the times of one minimum and one maximum, but was unable to give a trustworthy value of the length of the mean period. The observations which I have since made have however enabled me to determine with tolerable exactness the times of three more maxima and two minima, thus affording the means of ascertaining the value of the mean period within very moderate limits of error.

The observed times of maximum and minimum and the corresponding magnitudes are:—

*Maxima.*

| | | |
|---|---|---|
| 1863—Oct. 25. | 8·9 | magnitude. |
| 1864—Aug. 23. | 8·4 | ditto |
| 1865—June 30. | 10·1 | ditto |
| 1865—Nov. 22. | 9·4 | ditto |

*Minima.*

| | | |
|---|---|---|
| 1863—Aug. 24. | 11·3 | magnitude. |
| 1864—June 24. | 11·2 | ditto |
| 1865—Aug. 31. | 11·5 | ditto |

The times of minima appear to be more irregular than those of maxima; but the magnitudes at maximum vary more than those at minimum. As the times of maxima are evidently better suited for the determination of the mean period than those of minima, I have treated them in the usual way, and have obtained the following elements:—

Period = 152·4 days.
Epoch = 1865, January 24·1.

Calculating the times of maxima from these elements, and comparing them with the observed times, we have the following differences:—

| C.—O. |
|---|
| days |
| −0·1 |
| +1·7 |
| −4·5 |
| +2·9 |

The greatest difference is only 4·5 days, or about one *thirty-fourth* of the entire period, and the mean difference is 2·3 days, or one *sixty-sixth* of the whole period. T Aquilæ therefore belongs to the class of variables whose periods are moderately regular.

The diagram which accompanies this communication shows the mean form of the light-curve of T Aquilæ as derived from all the observations I have yet made. An examination of this curve has yielded the following results:—

Mean magnitude at maximum ................. = 9·20
Mean magnitude at minimum ................. = 11·20
Mean range of variation...... .................... = 2·00 magnitudes
Mean magnitude.................................. = 10·40
Interval from minimum to maximum ......... = 64·0 days
Interval from maximum to minimum ......... = 88·4 days
Interval from minimum to mean magnitude... = 43·0 days
Interval from mean magnitude to maximum... = 21·0 days

Interval from maximum to mean magnitude... = 40·4 days
Interval from mean magnitude to minimum... = 48·0 days
Interval from mean magnitude before to mean magnitude after maximum.................. = 61·4 days
Interval from mean magnitude before to mean magnitude after minimum.................. = 91·0 days

A faint secondary maximum is indicated at 63 days after the principal maximum.

Mr. Knott, F.R.A.S., has kindly favoured me with the results of his observations of T Aquilæ for the past year, and from the following comparison it will be seen that, with the exception of the time of the August minimum, they are very closely accordant with my own:—

*Maxima.*

| BAXENDELL. | KNOTT. |
|---|---|
| June 30—10·1 mag. | July 2—10·1 mag. |
| Nov. 22—9·4 mag. | Nov. 20—9·4 mag. |
| *Minimum.* | |
| Aug. 31—11·5 mag. | Aug. 22—11·4 mag. |

The place of T Aquilæ for 1865 is 20h 5m 25·4s + 15° 13·4′, and the calculated times of its maxima for 1866 are April 26·3 and September 25·7.

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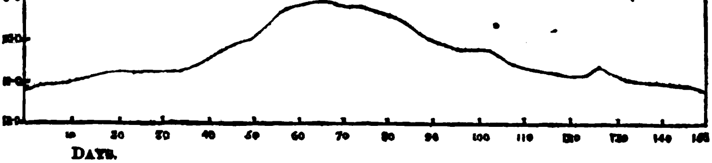
### MEAN LIGHT-CURVE OF T AQUILÆ,

AS DERIVED FROM ALL THE OBSERVATIONS MADE AT MR. WORTHINGTON'S OBSERVATORY DURING THE YEARS 1863–5.



### January 23rd, 1866.

R. Angus Smith, Ph.D., F.R.S., &c., President, in the Chair.

M. le Marquis Anatole de Caligny, of Versailles, Civil Engineer, was elected a Corresponding Member of the Society.

Thomas Graham, F.R.S., &c., Master of the Mint; A. W. Hofmann, F.R.S., &c.; Joseph Prestwich, F.R.S., &c.; and Andrew Crombie Ramsay, F.R.S., &c., were elected Honorary Members of the Society.

A conversation took place respecting the cattle plague, in the course of which Mr. **Baxendell** stated that the results of inquiries he had made had led him to believe that the total mortality among cattle from plague and all other diseases, during the past year, had been very little, if at all, above the average rate of the last ten years; thus indicating that the plague had, to a great extent, displaced pleuro-pneumonia and other dangerous diseases, and that therefore no just cause at present existed for the feeling of alarm which prevailed throughout the country.

A paper was read entitled "Notes on a Section of Chat Moss, near Astley Station," by W. **Brockbank**, Esq.

By the kindness of my friend Henry Mere Ormerod, Esq., I have had the opportunity of examining a section of the strata at Chat Moss, exposed in the excavations at present being made to obtain marl for the reclamation of the moss.

A large area has been excavated to a depth of 45 feet from the surface, down to the red rock, giving the following thicknesses of the beds, viz.—

| | |
|---|---|
| *a.* Peat Moss.......................... | 17ft. 0in. |
| *b.* Sandy Clay or Loam ........... | 1ft. 6in. |
| *c.* Boulder Clay or Till.............. | 26ft. 6in. |
| *d.* Soft Red Rock .................... | —— |

Commencing with the red rock, the following particulars appeared worth recording:—

*d.* The soft red rock forming the base of the section is the lower member of the trias. The surface of the rock was covered with water, which prevented an examination of it, but a large detached fragment was found lying upon it, which was not at all worn.

*c.* The boulder clay or till had a thickness of 26ft. 6in., and contained boulders in abundance. It had no parting of sand or gravel, but small patches of red sand occurred about the middle, and of small gravel near the base. Small cubical fragments of coal were very plentiful, and many pieces of black shale. The following list of boulders was made from those found in the excavations, viz.—

*Greenstone.* One very large boulder, much striated, and many smaller ones. The largest boulders were of this rock.

*Granites.* Four varieties, all much waterworn; the grey and green granites decomposed and very friable.

*Limestones.* Dark limestone boulders plentiful, one containing a good specimen of "Productus Gigantens" on the unworn surface, the other side being deeply striated and much worn.

Light grey limestones, much worn.

Reddish limestones (permian), very little worn, and pronounced by the workmen to be "Bedford limestone," because worked at Bedford, in the neighbourhood. One fragment of this limestone occurred within four feet of the surface, and was very little worn, the corners quite sharp.

*Red Marlstone Rock* (permian), little worn.

*Hæmatite Iron Ores* (probably permian), worn into round lumps. These are eagerly sought by the workmen for "red raddle." They resemble the richest specimens of iron ore as found at the Patricroft workings, but are apparently much purer, and more like the hœmatites of North Lancashire.

*Coal and Coal Shales.* Abundant. The coal in small cubical fragments, but in great quantity. One fine specimen of coal shale with "Anthracosia robusta," well preserved, and one fine frond of "Neuropleris." Oval nodule of pyrites or "Brass lump" from the coal measures.

*Sandstones.* White sandstone boulders very frequent, containing fossils, probably calamites and sigillariæ, and chiefly resembling the white sandstones of the middle coal measures.

*Ironstone.* Nodules of claybound ironstones frequent; one very good example with fine septaria in centre.

The above list of boulders possesses some features of considerable interest. The granites and greenstones are very much the same as those found in the clays around Manchester, and which are generally supposed to have come from Cumberland and Westmoreland. These are much worn and have deep striæ.

The limestones are such as occur in the Pendle district, and their presence confirms the author in his belief that the current which denuded the coal of the high-lying seams of Lancashire and as far north as Ingleborough, and scattered the boulders which we find in this clay, came from the northwards, and probably from the neighbourhood of Ingleborough. The quantity of coal in small fragments was very remarkable, and evidences very considerable denudation of the coal measures, as also the presence of so many boulders of coal measure sandstones and shales, such as occur throughout the district lying to the north of Chat Moss. The unworn fragments of the permian limestones and marlstones is an additional evidence, their outcrop being in the neighbourhood immediately to the northwards.

Mr. Ormerod informs me that the surface of the boulder clay is in ridges or long undulations, and that it frequently sinks into deep hollows, making the thickness of its moss covering very variable.

*b*. The clay is capped by a seam 1ft. 6in. thick of loamy sand, which contains the roots and stumps of oak trees, birches, and hazels, the stumps being broken off about a foot from the surface, and the trees lying near, embedded in the moss. Leaves, branches, and hazel nuts are found in abundance in this thin stratum, upon which rests the moss.

In an area of about a quarter of an acre laid bare by Mr. Ormerod's excavations were found at least 200 cubic feet of oak timber, which was carted away for fuel, forming six cart loads. Some of the trees were two feet in diameter, the roots *in situ* in the loam.

On one of the trees was found a fine specimen of a fungus attached to the stem at a foot or two above the root. It is a fungus now found growing on oak trees, "Polyporus igniarius," and is in beautiful preservation, the pores being quite visible to the naked eye.

It would appear as if the level valley plain on which Chat Moss now lies was formerly covered over with a forest of oaks with a dense undergrowth of birch, hazel, and other brushwood. How these trees were thrown down, and the forest became covered up by moss, is subject for speculation. It was probably a gradual decay, owing to the lowlying, undrained soil, which favoured the growth of sphagnum and of such fungi as we have seen attached to the oaks now found lying in the moss.

The occurrence of this thin stratum of loam containing the roots of trees *in situ*, underlying the moss, is strikingly like the underclays containing roots of the stigmaria in the coal measures.

*a. Peat Moss.* The general thickness of the moss, as exposed in the excavations and drains made by Mr. Ormerod, is from 17 to 25 feet. In some parts of Chat Moss the thickness is much greater, owing to the irregularities in the surface of the boulder clay or other stratum forming the base of the moss. In one place a depth of at least 180 feet has been reached with boring rods, which would place it below sea level, and would point to the existence of water—possibly an inland arm of the sea—since filled up by moss.

When the water is drained off, the moss is very considerably reduced in thickness. We measured it at two places where drains had been cut through to the loam, and where the moss had originally been 17 feet thick. It is now reduced to 8 feet, having sunk above one half. Mr. Ormerod believes he has thus lowered an area of 300 acres more than 3 feet on an average. It is quite evident from this circumstance that the bulk of the moss is very greatly influenced by the water contained in it, and it is probably from this cause that mosses are usually highest at the centres. The water can get away from the margins, but it remains in the centres and swells up the bulk.

The existence of the boulder clay under Chat Moss affords a ready means for the reclamation of the surface for agricultural purposes. Mr. Ormerod proceeds in the first place by carrying forward deep drains down to the loam, having connection with the lowest outfall. These are connected by cross drains, which gradually draw off the water and leave a firm surface. The moss is then turned over by spade labour, after which it is covered with clay brought up from below at the extensive excavation which forms the subject of this paper. The land is next manured with town manure, and, being sown with clover and oats, is found to bear an excellent crop of oats the first year, and clover and grass crops the two years following. In this way it is expected the whole of Chat Moss will shortly be reclaimed.

## PHOTOGRAPHICAL SECTION.

January 11th, 1866.

Dr. J. P. JOULE, F.R.S., &c., Vice-President of the Section, in the Chair.

A note from Mr. JOSEPH SIDEBOTHAM was read, regretting his inability to attend the meeting, and giving particulars respecting some photographs which he had recently taken with Dallmeyer's new wide-angle lenses of five and seven inches focus. These prints were exhibited, and also one taken with an ordinary lens from the same spot, a comparison of the two showing the great advantage of the new over the old form of lens. The time of exposure was stated to be from two minutes to thirty seconds for collodio-albumen plates at this season of the year.

Professor ROSCOE explained the method of meteorological registration of the chemical action of light, as described in his paper in the *Philosophical Transactions* of the Royal Society, which has been chosen as the Bakerian lecture for 1865. Dr. Roscoe exhibited the apparatus needed, and showed the method of manipulation adopted in order to obtain curves of daily chemical intensity. He also detailed the results which have been obtained by the employment of his method at the British Association's observatory at Kew, during nine months of the year 1865, under the superintendence of the Director, Mr. Balfour Stewart, F.R.S.

Dr. JOULE observed that he considered Dr. Roscoe's investigations on this subject to be equal in importance to the systematic registration of the variations of temperature, as the growth of corn, &c., depended as much on the chemical effect of light as on the temperature of the air.

Mr. **Baxendell** regarded the subject as a new department in meteorology, and one likely to yield results of considerable interest and importance.

Mr. **Parry** suggested that it was desirable to have observations made at more than one station, and at a distance from Kew.

Dr. **Roscoe** stated that he hoped in a short time to perfect apparatus for effecting the registration of the chemical changes in the light by means of self-acting apparatus, so as to lessen the labour of making the observations.

It was suggested that a simple photometer would be useful to photographers working with dry plates, and Dr. Roscoe said he would endeavour to carry out the idea.

## MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

### January 17th, 1866.

A. G. **Latham**, Esq., President of the Section, in the Chair.

The following donations were announced: —

Roper's Catalogue of Microscopic Works, by the Author.
Kölliker's Manual of Human Microscopic Anatomy, by the Executors of the late George Mosley, Esq.
Beck on the Microscope, by the Secretary.
Six slides of seeds and fungi, by the Secretary.
Several slides of sections of a Cidaris from the Indian Ocean, by Mr. Parry.

The **Secretary** reported that he had made a catalogue of the collection of microscopical objects belonging to the Section.

Mr. **Sidebotham** remarked on the best cement to use in forming cells for fluid preparations, and stated that gold size appeared to prevent the entrance of air bubbles better than Japan varnish or Brunswick black, which latter in time became porous, and also, from the evaporation of its turpentine, brittle. He said he and Mr. Thwaite were perhaps the first to use this method of mounting objects, and that he possessed slides of gold size cells made in 1844, which were still quite perfect, while those he mounted with Japan black in 1850 were most of them spoiled, and that he had again reverted to the use of gold size for the formation of the cell, using Japan varnish for its final closing only.

The **Secretary** said gold size remained viscid for a long time, and that if the cells formed of it were not well dried for a considerable period, or even baked in an oven, the size was very liable to "run in" and spoil the preparation. He had re-varnished the Section's collection with a mixture of Japan varnish and gold size, and thought the gold size would prevent the Japan varnish from becoming brittle or porous, while the latter would prevent the gold size from running in; but he strongly recommended that all collections should be re-varnished every five years, and deprecated the use of covering papers on slides of fluid preparations, as it prevented this.

Mr. **Latham** recommended the addition of a solution of india-rubber, and Mr. **Parry** of wax, to Japan varnish, to obviate its tendency to become porous and brittle.

Mr. **Heys** showed a well mounted specimen of the exuvium of the larva of a Dragon Fly, and stated he found these insects were easily brought to cast off their skins by changing the water in which they were kept—if soft, to hard, and *vice versa*, or if muddy, to fresh.

Mr. **Parry** exhibited mounted specimens of an Ammonite.

Dr. **Alcock** said that among Foraminifera from Dogs Bay which he had lately mounted, he thought there were some slides likely to interest the members. Many of the deformed specimens of Lagena striata (Williamson) were very curious, and a double one, having the neck as well as the body double, deserved particular notice. He said that he was quite convinced the striated Lagena with a mucro at the base is not a mere sub-variety of Lagena striata, but is very distinct from it; there were many specimens of it, all agreeing in their peculiar characters, and he proposed for it the varietal name of L. mucronata. The Lagena with a collar at the base of the neck, described by him in a previous paper, was undoubtedly distinct from any of the named forms, and he proposed to call it Lagena antiqua. In his examinations of the Dogs Bay sand one specimen only of Lagena vulgaris typica (Williamson) had occurred, though L. clavata was comparatively common. Perhaps the most interesting find was a perfect and characteristic specimen of Lagena crenata, a form lately described and figured by Parker and Jones, from Australia, but he believed not hitherto observed as British in the recent state. The very magnificent specimens of Entosolenia Melo also deserved notice, and the curious specimens of Truncatulina lobata with the later chambers "run wide," and various monstrous forms of Miliolina, would be examined with interest.

## Ordinary Meeting, February 6th, 1866.

J. P. JOULE, LL.D., F.R.S., &c., Vice-President, in the Chair.

The CHAIRMAN adverted to the loss the Society had experienced by the unexpected death of Mr. Parry, who from the period of his election in 1833 had constantly promoted the success of the meetings by his intelligence and kindly intercourse with the members.

Mr. BAXENDELL said that it might interest some of the members of the Society, especially those who had commercial relations with India, to hear that the system of forecasting the weather, and particularly the occurrence of cyclone storms, had been introduced into that country by Mr. Pogson, the government astronomer at the Royal Observatory, Madras. The first trial was made on the 25th November last, when the indications of a distant but approaching cyclone induced Mr. Pogson to send a notice to the Governor, Sir William Dennison, who promptly despatched a number of mounted sepoys to convey the information to various officials and direct them to prepare for the coming storm. Next day, uprooted trees on shore and disabled ships in the roadstead showed that the indications had been rightly interpreted. On the 4th December the weather had again a very threatening appearance, and the officials of the Marine department ordered all the vessels in the roadstead to put to sea; but Mr. Pogson, guided by the principles which had served him so well on the 25th November, concluded that a cyclone was passing far to the south, but would not approach sufficiently near to cause a severe gale at Madras, and he therefore ventured to send the message, "No need, the storm will not come here," and the result fully justified the confidence he had placed in his conclusions, as the storm passed without the wind at Madras attaining a dangerous force, and unusual precautions proved therefore to be unnecessary.

A paper was read entitled "Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes." Part III. By J. C. Dyer, Esq., V.P.

### *On Nail Making by Machinery.*

About the beginning of this century the great consumption of nails in America, in wood buildings, and the high rate of skilled labour, had led to many attempts to make nails by a more summary process than that of the hammer and anvil, used for making those imported from England. Those called "cut nails," were made by cutting angular slips from iron plates by the common shears, and the heads were made by a separate process of the die and hammer, worked by hand; and to unite these two operations in one machine was the object sought. For this purpose a machine was invented and patented by Mr. J. Odiorne, and another machine by Mr. Jacob Perkins, about the year 1806. These two patents, though for the same object, differed so far in construction that they were held to be distinct inventions. A third machine was invented by a Mr. Reed, which, however, was a combination of the former two, and I believe was not held valid as a patent. In 1810, Messrs. Wells and Co., of the Charles River Iron Works, near Boston, having arranged with the patentees, sent out models of the nail machine to me in London, to be patented for our joint account. The operations of the machines were as follow :—

1st. Feeding-plates, of the width and thickness to form the nails, are pushed endwise over a fixed cutter, against a stop under the traversing cutter, and at such angle with the line of the cutters as to give the severed nail the head and point ends, and the plates turned over between each successive cut.

2nd. A fixed gripping die is placed just under the fixed cutter, the face of it and the cutter being in the same line, and the counter die moves forward to bring both together, so as to hold the nails firmly, a portion of the large end standing out beyond the dies to form the heads of the nails.

3rd. The heading die then advances and presses the end into the "rose," "clasp," or "clout" heads.

Success or failure often depends upon slight changes in power-driven machines. These nail machines answered very well for large nails, but failed as applied to very small ones, but as a means of saving the labour applied to making tacks and very small nails, it became important to adapt the machines to them also. This object, after some time, I succeeded in accomplishing, and thereby rendered the patent nail manufactory, established under the patents for the original invention and the improvements on it, a success.

For a full description of these inventions I must refer to the specifications of the patents, and to the paper in *extenso,* as they could not be made intelligible within the limits of this abstract. The average rate of working was about 100 per minute for nails and 120 per minute for tacks. In after practise the tack machines averaged about 80,000 a day, whilst the best hand-workers made about 1,200 to 1,400 a day. Each machine was tended by a youth, much like those employed in hand-making, so that one hand with the machine turned out as many tacks per day as would require about sixty hands working with hammer and anvil. I have been informed that the speed of these machines has been greatly increased of late years. Some of the movements in these nail machines have since been adopted and found very efficient in rivetting machines and others of recent invention.

## MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

### January 29th, 1866.

J. Sidebotham, Esq., in the Chair.

The following specimens were exhibited:—

Two figures of the gills of fungi on glass, made by the dropping of their spores, by Sir J. Herschel, exhibited by Mr. Sidebotham; also similar ones prepared by Mr. Sidebotham.

Small cyclophorus-like shells, with a considerable part of the later growth free and strangely contorted, from Borneo, Mr. Sidebotham.

Leaves of some Indian plants, mounted for the microscope, Mr. Hurst.

Mounted Foraminifera, from shore sand, Port Adelaide, South Australia, Dr. Alcock.

Mr. Linton said that the abundant appearance last year of the humming bird hawk moth had afforded many collectors and admirers of nature excellent opportunities of watching this interesting and amusing insect in pursuit of its food, and of observing its remarkable agility, darting from flower to flower, and then poising itself, apparently stationary, while its long proboscis is put out with almost instantaneous velocity. It is very shy, and when disturbed darts away to a great height in the air, but it returns after a time and often to the same spot. He had observed one in his garden, at Old Trafford, about a clump of sweetwilliams, and, after two unsuccessful attempts in one day, he succeeded on the following day in taking it. During the time when the insect was abundant, many letters referring to it appeared in the *Times* newspaper, and in these it was constantly stated that the insect visits only scarlet or bright-coloured flowers, such as the scarlet geranium and verbena; but if closely watched it will be found not to confine itself to any particular flower, and in the case just mentioned it chose the sweetwilliams, though these were at the time quite on the decline, and there was abundance both of geraniums and verbenas in the garden. While staying at Pensarn he observed one of these moths on two successive days resting during the hottest part of the day on a newly whitewashed wall. It might be supposed that the most likely place to find the moth would be hovering over the plants on which the larvæ feed, which are Galium verum and G. molluga; but, while in Wales in August, he visited a spot daily, for a fortnight, where these plants were growing in great luxuriance, and never once saw the moth at that place, though he found the larva and succeeded in rearing it. The pupa cannot be said to bury itself in the earth, for it is found with only a few leaves drawn together over it.

Mr. Linton also announced having bred the butterfly, Grapta C. album, from the larva taken by himself at Abergele.

## PHYSICAL AND MATHEMATICAL SECTION.

February 1st, 1866.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

The following "Results of Rain-gauge and Anemometer Observations made at Eccles, near Manchester, during the year 1865," were communicated by Mr. THOMAS MACKERETH, F.R.A.S., M.B.M.S.:—

During the past year I have been able to register the rainfall at Eccles, from four different gauges; two I have placed three feet above the ground, thirty-four feet from any dwelling-house, and fourteen feet from my astronomical observatory, which stands ten feet higher than the receivers of these gauges. This ten feet measures to the apex of the revolving roof of the equatorial room. The receivers of the rain gauges on the ground are of different shapes, one being round, 10in. diameter, the other 5in. square. The other two gauges are placed four feet above the ridge of my house, quite above the chimneys and free from the influence of any erections whatever. One of the gauges is 5in. square, like the 5in. one that is three feet above the ground, on Glaisher's plan, with an edge inclined inwards, as is usual with rain gauges. The other is also 5in. square, but with an edge inclined outwards. These gauges are thirty-two feet from the ground. The water falls into glass vessels, into which the receivers are made to fit, exactly like the 5in. gauge three feet from the ground, so that there is no tube required for it to flow through and thus to cause loss by evaporation. Below I present the monthly amounts of rain that fell into each gauge, and the number of miles of horizontal movement of the air.

| 1865. | Rainfall in 5in. square gauge with edge inclined outwards, 32ft. from ground. | Rainfall in 5in. square gauge with edge inclined inwards, 32ft. from ground. | Rainfall in 5in. square gauge with edge inclined inwards, 3ft. from ground. | Rainfall in 10in. round gauge with edge inclined inwards, 3ft. from ground. | Amount of horizontal movement of air in miles. |
|---|---|---|---|---|---|
| January ............... | 2·238 | 2·486 | 2·956 | 2·952 | 5,250 |
| February ............ | 1·872 | 2·137 | 2·582 | 2·489 | 4,309 |
| March .............. | 1·320 | 1·730 | 1·700 | 1·721 | 4,480 |
| April .................. | 0·993 | 1·025 | 1·083 | 1·102 | 4,724 |
| May .................. | 2·585 | 2·605 | 2·840 | 2·884 | 3,499 |
| June .................. | 0·830 | 0·840 | 0·943 | 0·943 | 3,215 |
| July .................. | 1·883 | 1·992 | 2·043 | 2·085 | 3,784 |
| August .............. | 4·808 | 4·832 | 4·967 | 4·977 | 3,615 |
| September........... | 0·481 | 0·548 | 0·563 | 0·571 | 3,089 |
| October............... | 4·353 | 4·621 | 4·491 | 4·550 | 4,286 |
| November............. | 2·596 | 2·830 | 2·744 | 2·818 | 4,237 |
| December ............ | 0·569 | 0·589 | 0·673 | 0·717 | 4,263 |
| Totals ......... | 24·528 | 26·235 | 27·585 | 27·809 | 48·761 |

The next table shows the fall of rain in each gauge under the same general direction of the wind, and the corresponding daily amount of horizontal movement of the air.

| General direction of the wind .................... | N. | N.E. | E. | S.E. | S. | S.W. | W. | N.W. |
|---|---|---|---|---|---|---|---|---|
| Average daily horizontal movement of the air in miles................. | 79 | 124 | 143 | 156 | 178 | 162 | 183 | 149 |
| Fall of rain in inches in 5in. gauge with edge inclined outwards 32ft. from ground ........... | 1·414 | 2·011 | 0·259 | 4·060 | 5·246 | 6·738 | 2·099 | 2·701 |
| In gauge with edge inclined inwards 32ft. from ground........... | 1·468 | 2·054 | 0·271 | 4·285 | 5·866 | 7·168 | 2·280 | 2·840 |
| In 5in. gauge with edge inclined inwards 3ft. from ground ........... | 1·472 | 2·425 | 0·317 | 4·550 | 6·000 | 7·258 | 2·515 | 2·949 |
| In ordinary 10in. gauge 3ft. feet from ground.. | 1·501 | 2·415 | 0·327 | 4·610 | 6·069 | 7·368 | 2·516 | 3·006 |
| Number of days ........ | 6 | 27 | 6 | 35 | 22 | 48 | 15 | 23 |

From the above table it appears that the least amount of rain falls with an east wind, and the greatest with a south-westerly wind. The strongest wind with rain is the west wind, and it is known that the strongest gales are from winds a little north of west. This shows, though the above table is framed from the observations of one year only, that the strongest winds are from nearly the same quarter, even when rain falls with them.

The following table shows the average daily rainfall in each kind of gauge when the velocity of the wind has ranged between the number of miles indicated in the first column.

| Daily Movement of Wind. | 32 feet from ground. | | 3 feet from ground. | |
|---|---|---|---|---|
| | 5in. square gauge with edge inclined outwards. | 5in. square gauge with edge inclined inwards. | 5in. square gauge with edge inclined inwards. | 10in. ordinary round gauge. |
| 0 to 50 miles | ·029 | ·030 | ·042 | ·045 |
| 50 to 100 „ | ·103 | ·105 | ·110 | ·114 |
| 100 to 150 „ | ·220 | ·232 | ·231 | ·233 |
| 150 to 200 „ | ·108 | ·118 | ·122 | ·124 |
| 200 to 250 „ | ·151 | ·160 | ·174 | ·175 |
| 250 to 300 „ | ·127 | ·138 | ·154 | ·158 |
| Above 300 „ | ·174 | ·204 | ·197 | ·211 |

The President remarked with reference to the difference between the two elevated gauges that a Glaisher's gauge, with the rim inclined inwards, would register more than the true amount, as the spray from drops striking the rim on the windward side would be carried over into the gauge; while, on the other hand, a gauge with a rim inclining outwards would give less than the true amount, because the spray from drops falling on the rim on the lee side would be blown away and lost.

Mr. Baxendell, F.R.A.S., read the following "Note on the Variable Star S Coronæ."

This variable was discovered on the 5th of August, 1860, by Dr. Hencke, of Driesen, and a notice of it inserted in the *Astronomische Nachrichten*, No. 1281. So far, however, as I am aware, no account of any observations made since, has yet been published, and I have therefore thought it might be desirable to communicate to the Section the results of two series of observations which I have made at Mr. Worthington's observatory, and which have enabled me to determine its approximate mean period and epoch of maximum. The first series commenced on the 26th of May, and ended on the 31st of October, 1864. A projection of the observations shows that a maximum occurred on the 12th of August, 1864, the magnitude being 6·7. The second series began on the 2nd of April and was continued till the 12th of November, 1865, the star during this time rising from the 11·9 magnitude on April 2nd to a maximum, 6·6 magnitude, on July 17th, and afterwards declining to the 8·6 magnitude on November 12th. An inspection of the light-curves which accompany this note will show that only one period could have elapsed between the dates of these two maxima; and as Dr. Hencke's observations in 1860 indicate that a maximum occurred about the 1st of September in that year, we have the following data for the determination of the star's elements:—

| | OBSERVED MAXIMA. | INTERVAL IN DAYS. | NUMBER OF PERIODS. |
|---|---|---|---|
| 1860. | September 1. ......... | 1441 ......... | 4 |
| 1864. | August 12. ............ | 339 ......... | 1 |
| 1865. | July 17. ............... | ......... | |

Equating, and treating by the method of least squares, we have

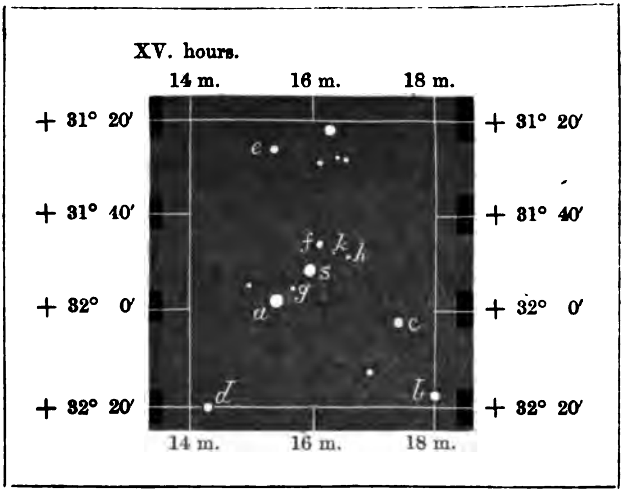
Mean Period............... = 357·2 days.
Epoch of Maximum...... = 1863, August 10·6.

The times of maxima calculated from these elements, compared with the observed times, give the following differences:—

C − O
days
+ 2·0
− 10·2
+ 8·0

I have not observed S Coronæ when at its minimum brightness, and cannot therefore give the lower limit of its range of variation, nor the interval from minimum to maximum. My observations show, however, that it belongs to the list of variables which increase in brightness more rapidly than they diminish. Thus in 1864 it rose through 4·5 magnitudes in 78 days, but fell through only 1·5 magnitudes in 80 days; and in 1865 it increased 5·3 magnitudes in 106 days, but diminished only 2·0 magnitudes in 118 days.

The place of S Coronæ for 1865·0 is 15 h. 15 m. 54·6 s. +31° 51·2′. The calculated time of its next maximum is 1866, July 17; and for the convenience of observers who may be disposed to watch its changes and record their observations, I append to this communication a small chart, and a list of comparison stars with their magnitudes.



MAGNITUDES OF COMPARISON STARS.

*a* = 6·8
*b* = 7·9
*c* = 8·2
*d* = 8·6
*e* = 8·8
*f* = 10·2
*g* = 11·3
*h* = 11·9
*k* = 12·6

Mr. **Baxendell** also read the following communication "On the Determination of the Mean Form of the Light-curve of a Variable Star."

Since the publication in the last number of the Society's *Proceedings* of my "Note on T Aquilæ," I have been requested to give an explanation of the method I employ to obtain the mean light-curve of a variable star. Many years ago I adopted a method which I afterwards found was very similar to, if not identical with, the one used by Professor Argelander for the same purpose; but I have recently employed another plan which appears to be decidedly preferable, as it saves time, renders unnecessary a great deal of tedious computation, and gives results of greater weight and value than those obtained by the former method. It is simply this: Having determined the elements of a variable star, calculate the times of all the maxima—or minima, as the case may be—that have occurred during the period over which the observations extend; then, taking the original light-curve, read off the values of the ordinates for each day, and arrange them in a table having as many columns as there are days in the star's mean period, or, in many cases, it will be sufficient to take every second, or even third day, always reckoning, of course, from the last preceding maximum or minimum, and then adding together the values in each column, and taking the means, we have the numbers from which to lay down the star's mean light-curve.

## Ordinary Meeting, February 20th, 1866.

R. Angus Smith, Ph.D., F.R.S., &c., President, in the Chair.

Mr. E. W. Binney, F.R.S., said that in the calcareous nodules found in the Upper Foot coal, a mine lying about fifteen yards above the Gannister coal at Moorside and other places near Oldham, he had met with a small stem of fossil wood showing structure in a very perfect state. It evidently belonged to the genus *Pinites* of Witham, since changed by Endlicher and Brongniart into *Dadoxylon;* but after comparing it with the species figured and described by those authors, and more lately by Professor Schimper of Strasbourg, he was of opinion that it was a new one. The specimen was also more complete than any other with which he was acquainted, although it was but of diminutive size.

It has generally been supposed that the coniferous woods found in the coal measures were only to be met with in sandstone rocks, and not in seams of coal or beds of shale, and had been drifted from high and dry lands into the waters in which such deposits had been formed, and had not grown on the places where they were discovered like *Sigillaria* and its root *Stigmaria*. The specimen of *Dadoxylon* now described, however, had equal claim to be supposed to have grown on the spot where it was found as any *Sigillaria* met with in the same seam of coal.

The stem is nearly cylindrical and enveloped in a matrix of limestone, so that we cannot see its external characters. Its diameter is about one half of an inch.

On examination of a transverse section of the stem we find a medullary axis composed of irregular polygonal cells full of dark carbonaceous matter separated by intervening spaces vertically, and thus forming a kind of discoid pith somewhat similar to that noticed by the late Professor Corda, and afterwards by Professor W. C. Williamson, F.R.S., in a paper published in vol. ix., second series, of the Society's memoirs. This pith in the present specimen appears to be separated from certain lunette-shaped bundles of hexagonal tubes of large size, arranged in a convex form towards the pith inwards, and lessening in size as they pass outwards into wedge-shaped masses of four-sided subhexagonal cellules arranged in radiating series and divided by large medullary rays or bundles which appear to originate in the lunette-shaped masses. On the outside of this internal radiating cylinder are other lunette-shaped bundles similar to those in the inside previously described and also with a convex outside. Then comes a narrow zone of lax tissue which has been a good deal disarranged. Outside this are some thin wedge-shaped bundles of cellules full of dark carbonaceous matter, and arranged in radiating series of varying sizes separated by lax tissue probably representing the bark of the tree.

In the longitudinal sections the cellules are seen to be greatly elongated and divided with oblique and transverse dissepiments placed at great distances. Two of the walls, namely those facing the medullary rays, are regularly reticulated with six, seven, and eight series of hexagonal areolæ arranged contiguously but not in a line.

In the tangential section the walls of the cellules also show a reticulated appearance something like that previously noticed, but not in so marked and distinct a manner, and the medullary rays or bundles in their section show numerous irregular series of small cellules of one to four, and more rarely much larger cellules.

The arrangement of the lunette-shaped bundles next the pith reminds us of similar masses described by Brongniart as occurring in his *Sigillaria elegans*, but the cellules in my specimen are much more irregular in size than those of

*Sigillaria*, and there are no traces of striæ upon their sides. The same may be said of somewhat similar lunette-shaped masses appearing on the outside of the internal radiating cylinder.

The areolæ on the walls of the cellules are more numerous than in any species of *Pinites* or *Dadoxylon* which have hitherto come under my notice, the *P. medullaris* of Witham having the walls of its elongated cellules reticulated with two, three, and four series of contiguous areolæ, and those only on the walls parallel to the medullary rays, while in my specimen they are reticulated with six, seven, and eight, and not only on such walls, but also on the walls at right angles to the medullary rays. The lunette-shaped bundles of cellules both in the inside and outside of the internal radiating cylinder are different from those seen by me in any other specimens of *Dadoxylon*. For the purpose of distinguishing it the name *D. Oldhamium* has been given to it.

A paper was read "On Air from off the Atlantic, and from some London Law Courts," by R. ANGUS SMITH, Ph.D., F.R.S., &c., President.

The specimens of air collected by Mr. Fryer when on his way to West Indies, and those collected in Antigua, are worth remarking, as the first agrees with the figures obtained previously when examining air on the sea shore and open heaths of Scotland, where the highest average was obtained, and the second agrees with the numbers obtained in more inhabited but not closely inhabited places.

Those from a law court are interesting; they are the most deficient in oxygen of any specimens found by me during the day in inhabited places above ground. The first is almost exactly the same as the average found in the currents of galleries in metaliferous mines; that from the lantern is nearly the same as the specimens found close to the shafts of the same mines, meaning of course the average of many specimens. I have not known any mills or workshops so deficient in air. I consider a room bad when it loses 1,000, and workshops very bad when they lose 2,000 of oxygen out of a million parts; here the loss is actually 5,000 less than the parks of London. The circumstance is strange, and I hope unusual. A scientific friend happened to call my attention to it and wished me to examine the air. The moisture from the window was collected and there were several ounces obtained, and more might have been easily found. It was perspiration in great part, the smell of it was distinct. It is putrefying, and decolorises more permanganate now than it did at first. Mere change of air will not purify a room like this—a current must pass through it for a long time until complete oxidation takes place.

### OXYGEN PER CENT IN SOME SPECIMENS OF AIR.

| 18ft. above water. Fine day.<br>2 30 p.m.<br>Lat. 43·05, W. 17·12. | St. John's, Antigua.<br>April 11th, 1865. 9 a.m.<br>Showery morning. |
|---|---|
| 21·0100 | 20·9600 |
| 21·0000 | 20·9100 |
| 20·9700 | 21·0000 |
| Mean 20·9900* | Mean 20·9500 |

| Law Court, Feb. 2nd, 1866. | Law Court, from the lantern, 4 30 p.m. just as the court was closing. |
|---|---|
| 20·6400 | 20·5000 |
| 20·6700 | 20·4800 |
| Mean 20·6500 | Mean 20·4900 |

* May be read 209,900 in a million, and so with the others.

## Ordinary Meeting, March 6th, 1866.

**R. Angus Smith, Ph.D., F.R.S., &c., President, in the the Chair.**

Mr. E. Sonstadt communicated the following "Note on the Purification of Platinum:"—

The tendency of platinum to alloy with other metals, at a temperature far below its fusing point, is sufficiently well known to every user of platinum crucibles. It is equally well known that iron, &c., which has been absorbed by platinum cannot be removed, except superficially, by the action of hydrochloric acid for instance, nor even by heating in acid sulphate of potassium. Stas, in his memoir on the atomic weight of silver, &c., states that he purified his platinum vessels from iron, by causing them to come in contact, at a red heat, with the vapour of chloride of ammonium. The process had to be repeated as often as any yellow sublimate was formed. This process is less effectual, or less conveniently and speedily effectual, than the modification of it that I have to propose; because, if the vapour of the sal ammoniac is generated from the solid salt in the vessel to be purified, the heat absorbed in the vaporization of the salt tends to keep the vessel at a temperature below that at which volatile metal chlorides are most readily formed. Instead of chloride of ammonium, I put dry double chloride of ammonium and magnesium in the platinum vessel intended for purification. The vessel is then heated to about the fusing point of cast iron for about an hour. I find a Gore's furnace convenient for this purpose. In this process, not only is chloride of ammonium vapour given off for a long while with the double salt, at a temperature much above that at which chloride of ammonium alone volatilizes, but when that salt is completely expelled, the chloride of magnesium remaining is perpetually being decomposed with evolution of free chlorine, and, frequently, the formation of a crystalline crust of periclase lining the crucible. Platinum thus purified is softer and whiter than ordinary commercial platinum. The method is not available solely for the removal of iron, but retrieves crucibles that have become dark coloured and brittle from exposure to gas flame, as well as crucibles that have been attacked by silicates during fusion of these with carbonate of sodium. I cannot conclude this note without remarking on the extreme facility with which platinum becomes impure by heating in contact with matters containing only a very small proportion of substance capable of attacking the metal. Thus, a platinum crucible becomes sensibly impure after prolonged ignition at a high temperature, bedded in commercial magnesia. On the other hand, I have kept a platinum crucible at a constant weight, to the tenth of a milligramme, over a series of intense ignitions, when the precaution has been taken to bed it in chemically pure magnesia.

A conversation took place on the cattle plague, in the course of which the PRESIDENT and Mr. SPENCE stated that the use of carbolic acid as a disinfectant had been quite successful as a preventive in the limited number of cases tried, none of the cattle on the farms where it had been regularly and properly used having yet been attacked by the rinderpest. People rarely used enough.

An opinion was strongly expressed by some members that the means which had been adopted to arrest the progress of the disease had, in fact, served to propagate it and extend its ravages, as numerous instances could be cited in which the infection had been carried by the official inspectors from unhealthy to healthy districts, and it was suggested that the inspectors ought to be compelled to cleanse themselves and undergo a disinfecting process before they were allowed to visit and inspect cattle not known for certain to be diseased. It was characterised as a most unwise proceeding to send forth an army of inspectors to invade every farmstead in the kingdom, and not to adopt and enforce stringent regulations to prevent the possibility of these men being the means of carrying the dreaded infection into healthy localities. It was remarked that official interference had virtually taken the management of the cattle plague out of the hands of those who, from their practical experience and the deep interest they had in the matter, might be held best qualified to deal with it, and there were grounds for thinking that if the farmers had been left to act for themselves without official interference, as they did some years ago when pleuro-pneumonia carried off large numbers of cattle, the rinderpest might never have assumed its present formidable aspect.

A paper was read "On the Liassic and Oolitic Iron Ores of Yorkshire and the East Midland Counties," by Messrs. EDWARD HULL, F.G.S., and WILLIAM BROCKBANK.

In this paper the authors gave the results of their observations on the nature, geological position, and qualities of the iron ores which are now being worked at intervals from the banks of the Tees to that of the Evenlode in Oxfordshire, extending through the counties of York, Lincoln, Rutland, Leicester, Northampton, Warwick, Oxford, &c.; at the same time embodying the opinions of previous observers.

Remarking that just at the time when some of the older iron-producing districts were giving evidence of approaching exhaustion, the enormous stores of iron ore in the newer formations were discovered; the authors commenced by a description of the geological position of the strata from which the ores are extracted, referring them to the middle lias or marlstone and the base of the great oolite; and it was shown that the ores of Cleveland in Yorkshire, Lincolnshire, and Oxfordshire, are derived principally from the lias, while those of Northamptonshire are extracted from the basement beds of the great oolite, called by the government geological surveyors "the Northampton sands."

The first district described was that of the East Riding of Yorkshire and the Cleveland hills, which, within the space of about sixteen years, has given birth to the iron trade of the Tees-side, of which Middlesborough may be considered the centre. The ore is here quarried in open and tunnel-works, and brought down by rail from the hills to the furnaces, which are erected along both sides of the river, and which are supplied with fuel from the Durham coal-field. The production of this district in 1865 was stated to be nearly one million of tons of pig-iron annually, drawn from 105 furnaces in blast. The ores in the valley of the Esk, near Whitby, and those of Guisborough were then described, and particularly the Rosedale ore, which is the richest in the district, and is magnetic. A branch railway is opened to the quarries, which in 1864 yielded nearly 300,000 tons of ore, having a per centage of 35·94 to 49·17 of metallic iron. The iron-stone was then traced to the banks of the Humber, near Hull, by Stokesley, Swainby, Northallerton, Easingwold, and Market Weighton.

The ores of North Lincolnshire were stated to be spread over a wide expanse of country—an area of not less than 100 square miles—having been proved to be quite close to the surface, and their local and geographical position in reference to the sea-ports on the one hand, and the South Yorkshire coal-field, together with the excellent quality of the iron they produced, satisfied the authors that this district was destined to rise in importance as a centre of iron manufacture. The pig-iron from North Lincolnshire is highly tenacious and fusible, and commands a higher price in the market than the Cleveland brands. This superiority appears to arise from the spathic nature of the iron-stone, and from the presence of manganese in considerable quantities interstratified with the ores, and which is mixed in the furnaces. The ore, with its accompanying strata, is very finely shown along the line of railway at Frodingham; its thickness in different parts of the district varies from twelve to thirty feet. It is being smelted at three works in North Lincolnshire, and it is also carried largely by rail to the Park Gate Iron Company's furnaces near Rotherham, and other works in the Yorkshire and Derbyshire coal-fields.

From North Lincolnshire the ironstone may be traced southwards along an indented line running parallel to the western margin of the great oolite into Northamptonshire. At the base of the oolite a series of yellowish sands occur—and, in these there are at intervals, beds of iron ore—sometimes extending for miles with considerable regularity, at other times thinning away rapidly. From these beds, the Northamptonshire iron is smelted. The furnaces and quarries are situated at Gayton and Blisworth, Blakesley, Maidford, and Litchborough, at which place they are more than usually ferruginous. At Duston, where it is extensively worked and smelted, it is more consolidated and is coarsely oolitic. The metal produced is similar to that of the Cleveland district in quality. Large quantities of this ironstone are sent by rail and canal into South Staffordshire, and even South Wales, where it is valued for mixing with the argillaceous carbonates of the coal measures.

The Oxfordshire district is as yet almost unopened, but promises one day to be productive of iron on a large scale, similar in character to the Lincolnshire brand. In the neighbourhood of Banbury and Chipping Norton, the ore occupies considerable areas—sometimes in the form of tabulated hills, intersected by valleys of no great depth. It occurs here (as already stated) in the middle lias, precisely the same formation as that to which the ores of Cleveland and Lincolnshire belong. The ironstone, however, is generally more calcareous than in the former district, and its richest portions are limited to a tract, of which the village of Bloxham may be considered the centre. As yet the ore has only been worked at Fawler, near Charlbury, and at Steeple Aston, to a very limited extent; but the railways, now in course of construction, will probably have the effect of bringing it within easier reach of the coalfields of South Wales and Staffordshire.

The authors remark, in conclusion, that every day's experience enlarges our acquaintance with the great mineral resources of our country, and that, in the case of coal and iron, it becomes a question which is more largely distributed and likely to outlast the other.

## MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

February 26th, 1866.

A. BROTHERS, F.R.A.S., in the Chair.

The following objects were exhibited :—

Mounted specimens of twenty-four species of Ostracoda, from Dog's bay shore-sand, collected by Dr. Alcock, and named by Dr. G. S. Brady.

Mounted specimens of many forms of Foraminifera, from a deposit discovered while sinking a well at Boston, Lincolnshire.—Mr. Sidebotham.

The skull and skin of a male Otter shot in Rostherne Mere, Feb. 16.—Mr. Harrison.

Dr. Alcock read a paper on Foraminifera from mud washed out from a shell of Halia Priamus in Mr. Darbishire's collection. He said that part of the interest of these specimens depended on the information they may give as to the nature of the sea-bed where this rare mollusk is found, the exact locality from which it is obtained by the Cadiz fishermen being still doubtful. He exhibited mounted specimens of about forty forms of Foraminifera, twenty-five of them agreeing with British ones as described by Professor Williamson, the others, so far as he knew, not British, and at present unknown to him. He said that the great abundance of Globigerina with fragments of large Orbulinæ, appeared to indicate a deep-sea deposit, but the most remarkable feature was the extraordinary profusion of Textularia variabilis, a very considerable proportion of the whole mass consisting of these shells. Bulimina pupoides was perhaps next in abundance, and next to it, in about equal proportions, Cassidulina lævigata, C. obtusa, and Nonionina elegans. Rotalina Beccarii was plentiful but small, as were all the forms of Rotalina which occurred Lagenæ were scarce and were very small hyaline varieties. Nodosaria radicula was rather common, and three very distinct varieties of it were met with, one of them remarkable for its large size and the raised rings upon its neck. Polymorphina was entirely absent; and Polystomella, Spiroloculina, and Miliolina were represented by only a very few extremely small individuals.

## PHYSICAL AND MATHEMATICAL SECTION.

### Annual Meeting, March 1st, 1866.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

The following gentlemen were elected officers of the Section for the ensuing year:—

President.
E. W. BINNEY, F.R.S., F.G.S.

Vice-Presidents.
ROBERT WORTHINGTON, F.R.A.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.
Mr. THOMAS CARRICK.

Secretary.
G. V. VERNON, F.R.A.S, M.B.M.S.

A paper was read "On the Variable Star R Vulpeculæ. $\alpha = 20^h\ 58^m\ 22{\cdot}9^s$. $\delta = +23^\circ\ 17{\cdot}2'$. Ep. 1865·0." By George Knott, F.R.A.S., communicated by Joseph Baxendell, F.R.A.S.

This star, which is No. 457, hour xx, in the Palermo catalogue, was first recognised as variable, so far as I am aware, at the observatory of Bonn. It appears to have been observed with some care by Dr. Winnecke at the Pulkowa observatory, and in a letter to the Rev. R. Main, printed in vol. xxii. of the Monthly Notices of the Royal Astronomical Society, p. 285, that able astronomer assigns the following elements, "which represent seven maxima observed in the course of three years, with reference to Piazzi's estimations of magnitude in August, 1803," viz.—

Period = 138·6 days.
Epoch = 1860, Nov. 6.

Having observed this star with more or less regularity during the past four years, it occurred to me that it would not be uninteresting to compare the elements resulting from a discussion of my own observations with those which had been deduced by Dr. Winnecke. The results of this discussion I have now the honour of presenting to the Manchester Literary and Philosophical Society.

Projecting my observations in the usual way, I obtain the following dates of maxima and minima, with the corresponding magnitudes—

| *Maxima.* | | | *Minima.* | | |
|---|---|---|---|---|---|
| 1861. | Dec. 30·0—8·4 | mag. | 1861. | Oct. 26·3—13·6 | mag. |
| 1862. | Oct. 5·0—7·8 | „ | 1863. | Sep. 18·0—13·2 | „ |
| *1863. | Nov. 19·4—7·6 | „ | 1864. | June 19·5—13·2 | „ |
| 1864. | Aug. 16·3—7·5 | „ | | Nov. 4·0—13·1 | „ |
| 1865. | Jan. 7·3—7·7 | „ | 1865. | Aug. 6·3—12·8 | „ |
| | May 25·5—7·8 | „ | | Dec. 14·3—13·7 | „ |
| | Oct. 5·5—7·5 | „ | | | |

Treating the seven observed maxima according to Mr. Baxendell's method, we obtain the following elements:—

Period = 137·59 days.
Epoch = 1864, April 4·95.

Comparing the observed times of maximum with those calculated from these elements, and also from those of Dr. Winnecke, we obtain the following differences between calculation and observation:—

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* The projection of a series of his own observations of this maximum obligingly communicated to me by Mr. Baxendell, yields the following results, in gratifying accordance with my own:—Date of maximum, 1863, Nov. 18·9, mag. 7·5.

| KNOTT'S ELEMENTS. | WINNECKE'S ELEMENTS. |
|---|---|
| Calc.—Obs. | Calc.—Obs. |
| Days. | Days. |
| + 1·41 | − 3·2 |
| − 2·41 | − 5·0 |
| − 0·01 | + 0·4 |
| + 4·24 | + 7·1 |
| − 2·23 | + 1·3 |
| − 2·78 | + 1·7 |
| + 1·81 | + 7·3 |

the sums of the squares of these numbers being 43·75 and 143·78 respectively. But while it thus appears that my own observations accord moderately well with Dr. Winnecke's elements, it must be confessed that these latter represent more satisfactorily than my own (as indeed might be expected) the magnitude estimates of Piazzi in the years 1807 and 1810, as given by Dr. Winnecke in No. 1224 of the *Astronomische Nachrichten*. At the same time it must be remembered that my own elements were deduced solely from my own observations, without reference to any of earlier date.

Treating the six observed minima in the same manner, we obtain the following elements, the period presenting a striking accordance with that deduced from the observed maxima:—

Period = 137·55 days.
Epoch = 1864, June 17·50.

The differences between the calculated and observed times of minima being:—

| Calc.—Obs. |
|---|
| Days. |
| + 2·35 |
| − 1·60 |
| − 2·00 |
| − 1·95 |
| − 2·15 |
| + 5·40 |

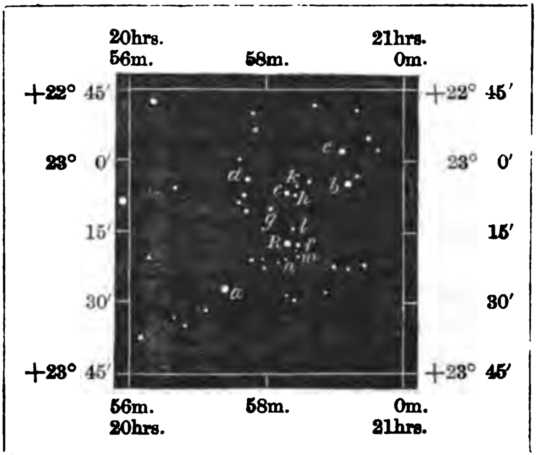
An examination of the mean light-curve (a copy of which accompanies this communication) which was laid down from the co-ordinates resulting from a discussion of all the observations I have obtained, yields the following results: —

| | |
|---|---|
| Mean magnitude at maximum | 7·77 |
| Mean magnitude at minimum | 13·14 |
| Mean range of variation | 5·37 magnitudes. |
| Mean magnitude | 10·32 |
| Interval from minimum to maximum | 66·0 days. |
| Interval from maximum to minimum | 71·6 days. |
| Interval from min. to mean mag. | 25·8 days. |
| Interval from mean mag. to max. | 40·2 days. |
| Interual from max. to mean mag. | 37·3 days. |
| Interval from mean mag. to min | 34·3 days. |
| Interval from mean mag. before to mean mag. after maximum | 77·5 days. |
| Interval from mean mag. before to mean mag. after minimum | 59·1 days. |

An examination of the various results of observation and calculation given in the former part of this paper suggests the following general remarks. Like many other variable stars R Vulpeculæ increases more rapidly than it decreases. The intervals between successive maxima and minima are subject to some little irregularity. And the observed magnitudes at maximum and minimum vary to the extent of some nine tenths of a magnitude. Still, as compared with some other stars, the movements of this variable must be regarded as tolerably regular. Although by no means so highly coloured as some variables, I have frequently noted the star in my observation book as "ruddy," or "decidedly ruddy." The maxima observable during the present year will fall, according to my own elements, on the following days: — July 17·0 and December 2·6. The observable minima will occur on May 6·3 and September 20·8.

The stars which I have used for comparison with R Vulpeculæ are shown in the small chart which accompanies this paper, and their magnitudes are as follow, the numbers in the cases of *a, b, c, d, g, l, m, n* being the means between my own values and those assigned by Mr. Baxendell:—

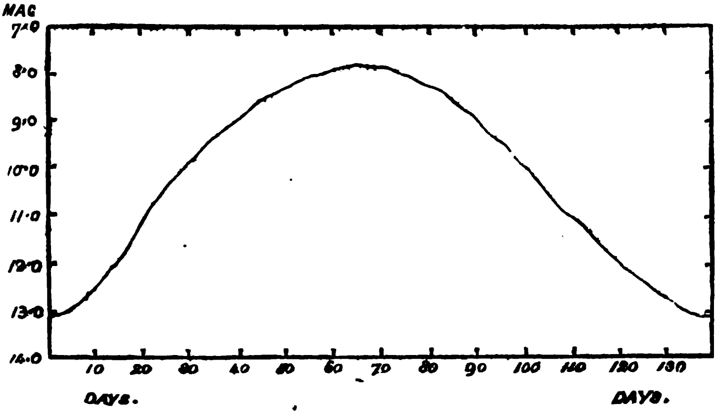
$a = 7{\cdot}1$
$b = 7{\cdot}3\pm$
$c = 8{\cdot}3$
$d = 9{\cdot}1$
$e = 9{\cdot}5$
$f = 9{\cdot}7$
$g = 10{\cdot}0$
$h = 10{\cdot}5$
$k = 10{\cdot}9$
$l = 11{\cdot}4$
$m = 11{\cdot}9$
$n = 12{\cdot}6$



The star *b* is variable to the extent of some few tenths of a magnitude, and may therefore with advantage be rejected as a comparison star; *g* is a double star, the magnitude assigned above being that of the two components seen as *one star.*

I cannot close this communication without gratefully acknowledging the courtesy and kindness of Mr. Baxendell in freely communicating to me his own methods, and in affording me all necessary explanations in cases of doubt or difficulty. It is to be hoped that the time is not far distant when the best methods of procedure in this branch of the science will find a place in our textbooks of practical astronomy.

MEAN LIGHT-CURVE OF R VULPECULÆ,
As derived from the Observations at the Woodcroft Observatory in the years 1861—1865.



A paper was also read, "On the Fall of Rain during the Different Hours of the Day, as deduced from a Series of Observations made by the Rev. J. C. BATES, M.A., F.R.A.S., at St. Martin's Parsonage, Castleton Moor," by JOSEPH BAXENDELL, F.R.A.S.

In the spring of last year, the Rev. J. C. Bates, M.A., F.R.A.S., of Castleton Moor, contrived a rain-gauge by which he is enabled to determine the amount of rain which falls during every quarter of an hour of the entire day. Observations with this instrument were commenced on the 24th of May last, but were interrupted for two days in October, owing to damage done by the violent gale of the 25th of that month, and again in December, while the driving clock was under repair. All the observations made up to the end of January have been kindly forwarded to me by Mr. Bates for examination and discussion, and although the series extends over a period of only a little more than eight months, and caution is, therefore, required in basing general conclusions upon it, yet the results I have obtained appear to me to be sufficiently remarkable and interesting to be worthy of being brought at once under the notice of meteorologists, in order that the law of daily variation of rainfall which they indicate may be tested by the results of similar series of observations made in other localities.

The number of days in each month on which the rainfall was registered by the new instrument was—May, 5; June, 4; July, 19; August, 20; September, 3; October, 14; November, 9; December, 5; January, 21: total, 100 days. The total amount of rain registered during the 100 days was 21·854 inches.

Arranging the amounts of rainfall in 24 groups corresponding to the 24 hours of the day, and, taking the sums of the amounts in each group, we have the following results:—

| Hour. | Number of Times Rain fell during the Hour. | Total Amount of Rain. Inches. |
|---|---|---|
| 1 | 21 | 0·536 |
| 2 | 27 | 0·619 |
| 3 | 28 | 0·774 |
| 4 | 29 | 1·124 |
| 5 | 26 | 1·042 |
| 6 | 33 | 0·650 |
| 7 | 35 | 0·728 |
| 8 | 33 | 1·263 |
| 9 | 32 | 1·132 |
| 10 | 34 | 0·909 |
| 11 | 38 | 0·854 |
| 12 | 41 | 0·879 |
| 13 | 34 | 0·734 |
| 14 | 37 | 0·921 |
| 15 | 35 | 0·907 |
| 16 | 35 | 1·068 |
| 17 | 30 | 0·884 |
| 18 | 34 | 0·964 |
| 19 | 32 | 1·162 |
| 20 | 36 | 1·033 |
| 21 | 32 | 1·350 |
| 22 | 23 | 0·658 |
| 23 | 25 | 0·828 |
| 24 | 24 | 0·835 |
| | | 21·854 |

Dividing the 24 hours into day and night periods of 12 hours each, we have—

| | Inches. | | Frequency. |
|---|---|---|---|
| Total rainfall from 6h. a.m. to 6h. p.m. = | 10·925 | ... | 337 |
| „ „ 6h. p.m. to 6h. a.m. = | 10·929 | ... | 417 |

It appears, therefore, that the average fall of rain during the night was sensibly the same as that during the day; but the frequency of rain was much greater during the night than the day.

Dividing the day into four periods of 6 hours each, we have—

| | Inches. | | Frequency. |
|---|---|---|---|
| Noon to 6h. p.m. ............... | 4·930 | ...... | 155 |
| 6h. p.m. to midnight ............ | 5·536 | ...... | 205 |
| Midnight to 6h. a.m. ............ | 5·393 | ...... | 212 |
| 6h. a.m. to noon.................. | 5·995 | ...... | 182 |

These results show that the greatest amount of rain fell in the fourth quarter of the day, reckoning from noon; and the least in the first quarter; and that the amount which fell in the second quarter was slightly in excess of that which fell in the third. The frequency of rain was least in the first quarter and greatest in the third.

The six consecutive hours in which the largest quantity of rain fell were from 4h. a.m. to 10h. a.m., the amount being 6·461 inches, and the amounts in the three following six-hourly periods were:—

| | |
|---|---|
| 10h. a.m. to 4h. p.m. .................. | 4·250 inches. |
| 4h. p.m. to 10h. p.m. .................. | 5·939 „ |
| 10h. p.m. to 4h. a.m. .................. | 5·204 „ |

If now, the numbers in the last column of the above table are arranged in eight groups, and the sums of the numbers in each group taken, we have the following remarkable results:—

| Hours. | Total Amount of Rainfall. | Frequency. |
|---|---|---|
| 1, 2, 3, | 1·929 | 76 |
| 4, 5, 6, | 2·816 | 88 |
| 7, 8, 9, | 3·123 | 100 |
| 10, 11, 12, | 2·642 | 113 |
| 13, 14, 15, | 2·562 | 106 |
| 16, 17, 18, | 2·916 | 99 |
| 19, 20, 21, | 3·545 | 100 |
| 22, 23, 24, | 2·321 | 72 |

Projecting these numbers on ruled paper, and drawing a curved line through the points thus laid down, we obtain a well-marked curve, having two maxima and two minima, which occur at the following hours:—

| | |
|---|---|
| Principal maximum at | 8½h. a.m. |
| Secondary ,, ,, | 8h. p.m. |
| Principal minimum ,, | 2h. ,, |
| Secondary ,, ,, | 1½h. a.m. |

This curve has no similarity to that of daily temperature, but has a strong resemblance to the curve of diurnal variation of the barometer, with a difference of about two hours in the times of occurrence of the maxima and minima. The regular diurnal variations of the barometer in this latitude are, however, so small that it is hardly conceivable they can produce the great differences in the amounts of rain falling at different periods of the day as indicated by Mr. Bates's observations. The curve of daily variation of the intensity of atmospheric electricity has also strong points of resemblance to the rain-curve, but its night minimum is the principal one, and most meteorologists will probably regard variations of intensity of atmospheric electricity as consequences rather than causes of the changes in the amount of condensed vapour precipitated in the form of rain. No other meteorological phenomenon is at present known having diurnal variations similar to those of the rainfall; but if we turn to the phenomena of terrestrial magnetism, we shall find that the curve representing the daily variations of declination of the magnetic needle has well marked points of similarity to that of the daily rainfall. Taking for data the mean results of ten years' observations of magnetic declination made at the Royal Observatory, Greenwich, (*Greenwich Observations for* 1859,) we have the following comparison of the times of maxima and minima of the two phenomena, the westerly deviations of the magnetic needle from the mean position, being taken with a negative sign.

| | Daily Rainfall. | | Daily Oscillations of Magnetic Needle. |
|---|---|---|---|
| Principal maximum at | 8½h. a.m. | ...... | 10h. a.m. |
| Secondary „ „ | 8h. p.m. | ...... | 7½h. p.m. |
| Principal minimum „ | 2h. „ | ...... | 1h. „ |
| Secondary „ „ | 1½h. a.m. | ...... | 3h. a.m. |

Owing to the shortness of the period over which Mr. Bates' observations extend, it is probable that the results derived from them may be open to considerable correction, when a more extended series becomes available. I find, however, that when this short series is divided into *two* or even *three* groups, the main features of the daily variation are still preserved in the results of each group, and I do not, therefore, anticipate that any corrections which may hereafter be found to be necessary, will be of such a character as to affect materially the probability of a close connection between the daily variations of rainfall, and the diurnal oscillations of the magnetic needle, and I hope to be able in a future communication to bring forward other facts and results bearing upon this interesting and important question.

Whatever views may be taken as to the cause of the unequal distribution of the rainfall in the different hours of the day, it could, at all events, hardly have been expected that the greatest amount of rain would fall at that period of the day when the rate of increase of the difference between the temperature of the dew point and that of the air, is greatest; and when, therefore, according to generally received meteorological principles, there would appear to be little probability of a condensation of vapour into the form of rain.

I cannot conclude this communication without gratefully acknowledging the kindness of Mr. Bates, in affording me an opportunity of examining his valuable observations which form, I believe, the only series of the kind ever yet made.

## PHOTOGRAPHICAL SECTION.

### February 8th, 1866.

Dr. J. P. Joule, F.R.S., Vice-President of the Section, in the Chair.

Mr. **Sidebotham** brought before the section a number of pictures, by Boulton and Watt, which had been supposed to be photographs. The examination he had made of them convinced him that they had been produced by a different process.

Mr. **Dancer** coincided in this opinion. He thought that the camera had been employed, but solely for the purpose of enabling the artist to trace the outline, and to enlarge or reduce the image to any required scale. The shading would be an after process, the crayon employed being made of some resinous or fatty substance mixed with the colour.

### March 8th, 1866.

Dr. J. P. Joule, F.R.S., Vice-President of the Section, in the Chair.

Mr. **Brothers**, F.R.A.S., stated that since the last meeting he had tried the use of wax dissolved in ether, as recommended by Mr. Rogerson, for the purpose of cleaning glass plates, and was quite satisfied it was an excellent method of cleaning glass.

A paper was read "On the Pantascopic Camera," by J. R. JOHNSON, Esq. Communicated by E. C. BUXTON, Esq.

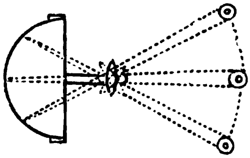
But a few years had elapsed since the introduction of the Daguerreotype, and photography had not yet passed beyond what may be termed "The Daguerreotypic Era," when the unsatisfactory nature of the "bits" of landscape produced by the lenses then in use became fully recognized. What could be more annoying to an artist desirous of producing pictures of some of the splendid architectural monuments of Paris than to find that from no possible point of view could the whole of many of the finest of these be embraced?

The desirability of not only effecting this, but of reproducing as pictures some of those striking "*coups d'œil*" which abound on the banks of the Seine, struck an eminent artist-engraver, Mr. Martens, so forcibly that he immediately set about finding a remedy, and he was not long before he had attained his object.

He found that if a lens be moved horizontally upon a pivot placed beneath its optical centre, the image is stationary notwithstanding the motion of the lens.

Now as all subsequent rotating cameras are based upon this principle it is desirable that we should clearly understand it, and the instrument I am about to describe, and which is intended to be used for viewing panoramically views produced by a rotating camera, will enable us to do so.

FIG. 1.



In fig. 1 we have a lens mounted in a socket so as to turn horizontally, and behind it we have a piece of cardboard bent to form part of a cylinder, of which the focal length of the lens is the radius.

Now, if in front of this we place a taper, an image of the taper is formed upon the cardboard screen. We may turn the lens backwards and forwards through a short arc, and yet you will see the image remains stationary. If we move the taper sideways keeping it at the same distance from the lens and turn the lens towards it, an image is produced on a different part of the screen. If a sufficient number of candles had been placed before the instrument and we had limited the action of the lens upon the screen to a narrow vertical band of light, the screen would have received consecutively the images of all the candles laid side by side, and perfectly distinct from each other.

Now if for the cardboard we substitute a bent Daguerreotype plate, and if for the ordinary reading glass we put a good photographic lens, and for the series of tapers any landscapes or series of lighted objects, we shall have an exact reproduction of Martens' camera. He took a number of beautiful views of Paris, from the bridges of the Seine, but the instrument failed in other hands than his. The difficulty of polishing and preparing large silver plates was of itself a serious obstacle, and a still greater want of success arose from the fact that Mr. Martens not being familiar with the practice of mechanics, did not attempt, or failed to produce the motion of the lens by self-acting means, and it requires the greatest skill and care to maintain a uniform motion for the length of time necessary for due exposure by means of the hand alone.

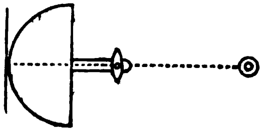
Even in the hands of Mr. Mayall, so successful in large Daguerreotype plates, and so careful and intelligent an operator, the instrument failed completely. He worked for a long time at Niagara, unsuccessfully, before obtaining the few views he brought away with him.

After the Daguerreotype give place to albumen and collodion on glass, Martens' camera was laid aside, but in 1854 he and a nephew made an effort to replace the bent plate at the back of the camera by a flat plate; and he contrived an apparatus for giving such a motion to the plate and lens respectively, that he succeeded in getting pictures upon such a plate more or less successfully.

To show what these motions are, we will go back to the instrument which has served to illustrate Martens' first camera. We will remove the bent cardboard, and making it straight, we will place it against the circular edge which previously held it bent.

If the taper be placed directly opposite the centre of the instrument, the flat paper must be at right angles to the ray of light, at a tangent to the curve on which the circular plate was bent, and the centre of the plate must be opposite the taper.

Fig. 2.



If the light be shifted to *b* fig. 3 the lens must be turned, and the plate must also be turned so that it is still at right-angles to the ray, and tangential to the curve as before, but the image must be received away from the centre. This will bring us to the position fig. 3 and if the one taper be moved in the opposite direction the lens and plate will be as shown in fig. 4.

Fig. 3.

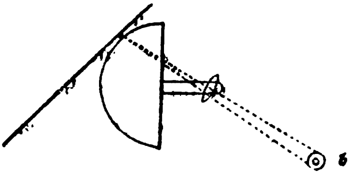
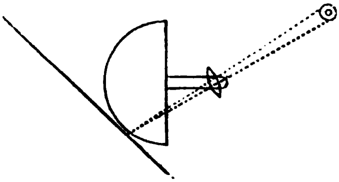


FIG. 4.



If the rotation of the lens and the change of position of the plate be made with due regularity perfect definition is obtained, but the greatest accuracy is necessary to obtain this.

To return to the history of this instrument. Mr. Martens by a note to the Academy registered the fact of having produced views in this manner, and his instrument was laid aside until it was disinterred for the first time we believe last summer. As far as we are informed, although Mr. Martens has yearly produced splendid views of Swiss scenery and has regularly published them, yet no product of the machine has been shown by him since 1854, the elaborately engraved panoramas of the Swiss alps having been produced by laying views taken by the ordinary camera side by side, and copying them juxtaposed.

Various other gentlemen subsequently undertook to solve the problem. Among others may be mentioned M. Garella, an engineer officer employed in Algeria; Mr. Sutton, so well known for his high attainments and inventive talent; Mr. Hosmer, &c.

The plans of these gentlemen have been published. Other plans have been heard of, as those of Mr. Rawlinson, of Keswick; Mr. Stuart, an Indian officer—but those plans have not been made known.

It would be an interesting subject to examine all these projects in detail, but to understand them would require numerous diagrams and perhaps models.

In general terms it may be stated that all of those known, including that of Martens, possessed two features which render them quite distinct from the instrument we are about to describe, and which will, we believe, serve to account for the fact of these projects remaining as such, or never at least passing from the first to a more perfect stage of development.

These features are,

1. The instrument was placed upon a large table upon which the pivot was fixed and upon which the plateholders rolled, the motion of the supporting rollers describing complicated curves upon the table, which curves Mr. Sutton recognised as being evolutes of the circle of rotation.

2. No direct mechanical means were employed for effecting the relative motion of the plate and lens, guide curves formed by trial or less efficient means being alone used.

I believe I may state without fear of contradiction that up to the year 1862 no views produced by any of the inventions alluded to had been publicly shown, nor views by any similar instrument purporting to produce pictures on true panoramic projection by continuous motion. Several tolerably successful attempts had been made to place two or more views side by side upon the same plate by means of a shifting back. But these compound pictures are merely views in plane perspective placed side by side upon the sides of a polygonal prism, and have no pretensions to be considered views in panoramic or any other correct projection.

It was not until the year 1862 that I succeeded in taking panoramic views with any degree of success. I had been employed in devising plans for effecting this object since 1860, aided by a working mechanic, Mr. Harrison.

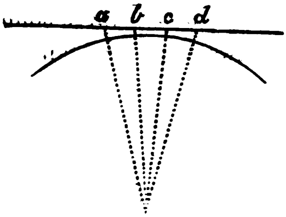
After passing over the same ground as some of the gentlemen above referred to without being aware of their labours, I succeeded in 1862 in perceiving that the complicated motion of the plate and lens might be really resolved into two simple movements, a circular motion of the whole apparatus, and a rectilinear motion of the plate; and that if the motion of the camera revolving be made to carry with it a strait rail at the back, the plateholder has only to traverse this in a right line to give the due motion required.

By forming a groove upon the edge of the circular base plate upon which the camera rotates, and turning this to the proper depth, the due rectilinear motion of the plate behind the lens may be given by means of two strings or wires, one end of each of which is attached to the disc or base plate and the other to the carriage containing the plateholder, so that if we remove the clockwork by releasing the spring which holds it up to the inner edge of the circular base, we can obtain all the necessary motions for working the camera without either wheel or pinion or any mechanism whatever unless the wires and disc and the five friction rollers upon which the apparatus moves can be called such. Views may be taken with the instrument in this state, but the accuracy necessary to produce an equal tint, particularly in the skies, is unattainable except by clockwork. The latter is extremely simple. It is a small clock movement moved by a spring, chain, and fusee, and is regulated by a fly or series of flys, the blades of which may be set at any angle from the horizontal to the vertical. It drives the instrument by means of a small steel pulley gearing into the milled edge of the circular base by a process which may be called a compromise between frictional and toothed gearing, and which has advantages that neither of these possesses singly.

In my rude attempts to illustrate the principle of the rotating camera I said that the ray of light passing through the lens must be limited. The reason of this is obvious. The true surface for receiving the image is a cylinder, but that substituted in the flat plate camera is a plane surface.

Now, the smaller the angular aperture of the narrow strip of light employed, the nearer the tangent approaches the curve.

FIG. 5.

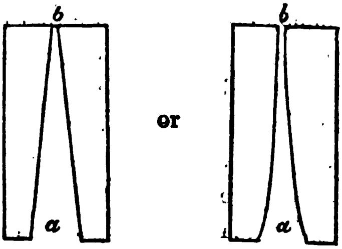


There is great discrepancy between the curve and straight line, when the width of the strip is equal to *a d*, fig. 5, but the difference is inappreciable when limited to *b c*.

The width of the active rays thus employed, is determined by a diaphragm, with shifting sides, placed at the back of the camera, near the surface of the plate, and advantage has been taken of this in order to produce clouds, and generally atmospheric effects not attainable in the ordinary camera.

By making the aperture taper thus

FIG. 6.



the narrow portion being opposite the sky, it is obvious that a totally different exposure will be given according to the width of the opening, and directly proportionate to the extent of that opening.

If the part (*a*) be fifty times the width of the part (*b*), then the time of the exposure of the first plane of the picture and that portion of the sky which approaches the zenith will be as 50 to 1.

Now we know that we can get the most delicate cloud by a sufficiently short exposure, and that we can also get parts in deep shadow by an exposure sufficiently long, but hitherto we have not been able to obtain these simultaneously.

Several hundred views upon the table will show to what extent of perfection this principle of unequal exposure has been carried.

As all of these views were obtained during last season, many of them at elevations of from eight to ten thousand feet above the sea; and as the parts of the larger instruments had to be detached and carried separately and then mounted together on arrival at the elevated station, it may be safely alleged, we think, that the pantascopic camera is efficient in producing what it professes to do, and that it is a practical instrument bearing the necessary amount of rough usage inseparable from a campaign like that undertaken by the photographer who produced these views.

Having already occupied so large a portion of your time, I will only add a few words as to the nature of the perspective in which these views are produced.

It is a very common error to suppose that there is only one sort of perspective, and I was surprised to see that when the pantascopic camera was first explained to the Photographic Society of London, the vulgar view was entertained by at least two members of the council of that learned body.

On that occasion, two gentlemen entered their protest against the assumption that the views shown or taken by the camera were true.

The inventor very pertinently asked, What is truth—what is your standard? Do you mean that the views are not in plane perspective? But I have already said they are panoramic views. How can they be in plane perspective if they are panoramas? It was evident that to them there was but one perspective; but it has been said by competent authority that there are as many kinds of perspective, as there are imaginable surfaces upon which to receive the visual rays passing from objects to the observer. At least three such systems of perspective are acknowledged and acted upon by artists. We may call these *spherical*, *cylindrical*, and *plane* perspective.

A painting upon the interior of a spherical dome, or a view taken by Sutton's beautiful spherical lens, is an illustration of the first system; Burford's or Selon's views on the walls of a circular apartment, or the pantascopic views, illustrate the second system; and an ordinary perspective drawing, or a view taken with a triplet or new doublet lens, furnishes us with an illustration of the third system, or that which is usually employed by artists.

Now all these are really true, but to be absolutely so, the pictures must be viewed upon surfaces similar to those upon which they have been projected, and from the same station point.

Mr. Sutton's pictures must be taken and viewed upon bowls of glass, the eye being in the position of the lens when the view was taken. With Burford's panorama we must place ourselves in the centre of the room and turn round so as to view the picture as we should view the landscape it represents.

This is equally true of a picture in plane perspective, so that if we really insist upon absolute truth in viewing a picture taken by a lens of four-inch focus, the picture must be brought within that distance from the eye, or the extreme lateral objects will not fall upon the retina at that angle which they would do in viewing the landscape itself.

As our object is usually to produce pleasing pictures, rather than an absolutely truthful representation of the scene, the discrepancy between the pantascopic pictures viewed flat is rarely objectionable, indeed, if we examine the numerous views before us, we shall see that in many of them the widening of the base line is advantageous, and often adds to the value of the picture, by giving better arrangement or composition. Perfect truth may at all times be obtained by curving the picture, or by mounting it in an instrument I have devised for the purpose, and which I propose to call a Pantascope or Orthoscope. In that instrument the curved picture and lens have a definite relation to the lens and camera employed, so that the images are seen exactly in *situ*, and if for the lens we substituted a theodilite or similar instrument, the horizontal bearings of each object could be taken as in nature. As only a narrow strip of the field is employed, the view is free from the distortion which we should have had with the same lens covering a large field.

It will be seen, on examining pictures in this instrument, that from the fact of the lens being thus truly placed we have on looking through it such admirable light and shade, and such perfect modelling of the objects, that the effect is artistic relief in the highest degree, almost in fact competing with the solidity due to binocular vision. As the lens magnifies the objects considerably, beautiful detail is obtained, the geological character of the distant rocks and the peculiar character of the ice of the glaciers being represented with most striking effect.

These views have been shown to the Alpine Club and to Professor Forbes of Edinburgh, whose labours on the glaciers are so well known. All concur in considering them the most perfect representations of the subject which have been yet produced.

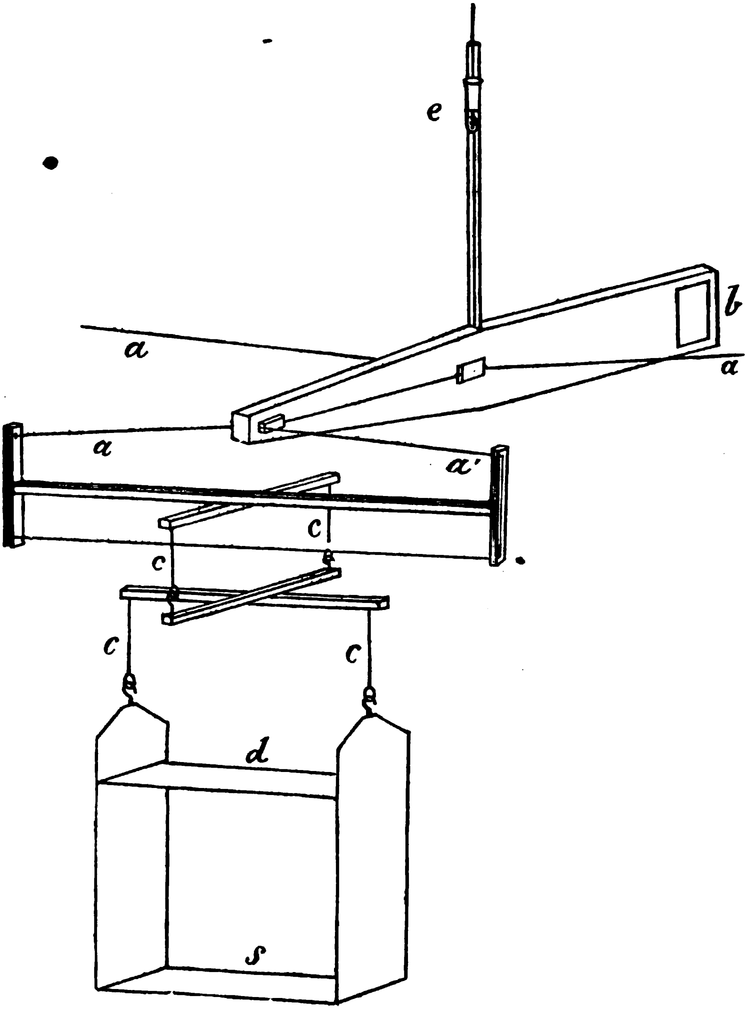
Mr. BUXTON said that he could add his testimony as to the amount of rough usage which a Pantascopic Camera would bear without being rendered unfit for work. The camera he had used last summer was terribly knocked about on its way from London, and afterwards in Scotland, but the machinery always worked as smoothly as he could desire.

## Ordinary Meeting, March 20th, 1866.

E. Schunck, Ph.D., F.R.S., Vice-President, in the Chair.

Mr. John Patterson was elected an Ordinary Member of the Society.

Dr. J. P. Joule, F.R.S., exhibited a balance which he had constructed on the principle which had been introduced by Professor Thomson, and employed by him in weighings for a long time. The adjoining figure will fully explain the instrument. The beam has a leaden weight let into its extremity *b*. It is supported by a wire *a a* stretched between the sides of the box containing the balance. This wire is led round so as to form the suspender *a′ a′* of the scale. Silk threads *c c*, *c c*, hanging from the cross pieces, form a gimbal system by which the scale is supported in such a manner that any variation in the position of the weights does not alter the torsion of the suspender. A counterpoise of known weight is placed on the stage *d*. When an article is to be weighed it is placed in the lower part of the scale *s*, and then, the counterpoise being removed, weights are placed on the stage to effect the counterpoise in the new condition. The difference between the first and second counterpoises of course gives the weight required. The upper edge of the beam is furnished with an index for showing minute effects; and attached to this is a small bottle *e* for holding shot or sand, by the addition of which the stability of the beam may be decreased to any required extent. The instrument exhibited was able to weigh articles of upwards of 3,000 grains to one hundredth of a grain. Dr. Joule stated that he had also employed Professor Thomson's principle in the construction of a galvanometer for the absolute measure of electrical currents. In this instrument a flat coil is suspended between two fixed flat coils, one of which attracts while the other repels the suspended coil, to which last the current is conducted by means of the suspending copper wires. This electrical balance is sensitive to one part in two millions.



Mr. **Binney**, F.R.S., exhibited a singular mineral which Mr. Ward of Longton had found in a nodule of clay ironstone from the North Staffordshire coalfield. At first sight it looked like a fossil coral of the genus *Cyathophyllum*, but on more careful examination it appears to be a mineral mass in a semicrystalline state. The form of the mineral appears to have been spheroidal with crystals radiating from the centre. By the kindness of Dr. Crace Calvert he had ascertained the specimen to consist chiefly of carbonate of lime, carbonate of iron, and phosphate of lime, with traces of magnesia, alumina, and organic matter, and ten per cent of silica.

He also exhibited a beautiful white specimen of carbonate of strontia obtained from a vein of carbonate of lime. It occurred among the lime in radiated masses similar to those of carbonate of barytes as sometimes found in veins of sulphate of barytes. This mineral has been found in considerable abundance, but up to this time it is believed that no use has been found for it on a large scale.

Messrs. **Hull** and **Brockbank** exhibited specimens of the iron ores referred to in their paper "On the Liassic and Oolitic Iron Ores of Yorkshire and the East Midland Counties," read at the last meeting of the Society.

Professor **Roscoe** stated that he had just received a letter from Professor Bunsen, announcing the discovery of a most interesting and important fact, namely, that the well known black absorption lines of the Didymium spectrum, when examined with polarised light, vary according to the direction in which the light is allowed to pass through the crystal. This shows that the position of the black absorption lines is in some degree dependent upon the physical structure of the body through which the light passes, and is not merely determined by its chemical constitution.

A paper was read entitled "Notes on Cotton Spinning Machinery. Part II.—Roving Frames." By J. C. DYER, Esq., Vice-President.

Having in Part I. of these Notes given an account of the inventions and improvements that had been applied to the mule jenny, and for converting it into the self-acting mule, the present paper in like manner traces the origin and progress of the machines known as roving frames, which come next in the order of scientific interest, as having engaged the labours of many eminent mechanics, through a long course of years, to bring them into their present accurate form of working. The rovings are equalised by doubling and drawing before coming to the roving frame, in which they are again drawn and slightly twisted to fit them for spinning; but the twist is not enough to give them strength to be taken up by the drag of the bobbin, as is the case in throstle spinning, so that the bobbins must be driven separately from the fliers and their speed must vary according to their different diameters, so as to make the surface motion of winding on to correspond with that of the delivering rollers, and when the latter motion is changed the former must be made to agree with such change, which is called the differential motion; and to make these agree with the frequent changes required was attended with much trouble and difficulty in working, which ultimately led to the introduction of the machine called the "tube roving frame," in which the said differential motion was wholly dispensed with. In this frame the bobbins are mounted on cylinders and turned by the friction of their surfaces. Being geared with the delivering rollers, their motions were thus made to agree at all speeds. But another property of importance in the tube frame was the twisting and untwisting of the rovings and the pressing of them hard upon the surface of the bobbins, so that the surface motion could be made accurate. This tube frame to a large extent was substituted for the bobbin and fly frame for low numbers, but it could not be made applicable to rovings for fine spinning; but the pressing of the rovings hard upon the bobbins in the tube frame rendered the application of the presser to the fly frame of great importance. After many experiments this object was effected, and the application of the presser to the bobbin and fly frame was carried into effect by means of springs, or the centrifugal action of revolving weights attached to the arms of the fliers, and both of these methods were set forth in the patent for those improvements. Nevertheless several competing parties afterwards obtained patents for very trivial changes, called "inventions," for applying the said spring and centrifugal actions to press the rovings upon bobbins.

The differential motion of the bobbin and fly frame continued very imperfect until Mr. Henry Houldsworth's important invention of a self-adjusting apparatus for giving the differential motions required. This discovery of a very simple train of movements for securing the required rotations of the delivering rollers of the bobbins and of the fliers will at once be recognised as an invention of a high order in mechanical science. From the limits of this abstract the main features only of these delicate movements can be given, and for the details the paper must be consulted *in extenso*. I may here add that about sixty years ago Mr. John Kennedy employed rotating tin cans for giving twist to rovings; but as they could not be driven with sufficient speed, the old throstle spindle, enlarged to suit rovings, was adopted for the frames called the "slubber" and the "bobbin and fly," and these frames, with many improvements by the same gentleman and others, were in extensive use until partially superseded in the year 1828 by the tube frame as before mentioned; and the suggestive nature of new inventions and discoveries as leading to the production of others is strikingly shown in the successive application of those in the tube frame to the bobbin and fly, as before said.

Again, Mr. Houldsworth's beautiful differential motion, then called "Jack in the box," being governed by bands and pulleys, their chance of slippage rendered it desirable to substitute toothed wheels, on which occasion Mr. John Kennedy and Mr. Peter Ewart each discovered the three-wheel motions since adopted—those of Mr. Ewart were mitre wheels, Mr. Kennedy's, spur gearing—the latter being mostly preferred. And it is worthy of note that the three mitre wheels for giving the differential motion in the slide lathe, required for turning cones, had been long in use and publicly seen without any one having dreamed of the application of them to other objects; nor was Mr. Houldsworth at all aware of these slide lathe differentials until pointed out to him by Mr. Ewart.

The paper concludes by an earnest appeal to Mr. Houldsworth and other eminent mechanicians personally connected with the progress of modern inventions and improvements of the machinery employed in our manufacturing establishments, to record their own experience and observations upon the several branches with which they have been more especially conversant, as has been done by Dr. Fairbairn in his published works, and as the author of these Notes has aimed to do.

The following papers were read at the Photographical Section Meeting, February 8th, 1866.

"On the Supposed Photographs by Boulton and Watt." By JOSEPH SIDEBOTHAM, Esq.

About three years ago the scientific world was startled by the announcement of the discovery of sun pictures, on paper and on silver plates, said to have been produced at the close of the last century, by Matthew Boulton and James Watt; shortly afterwards, Mr. Smith, of the Patent Museum, read a paper on the subject, and exhibited the pictures in question at a meeting of the London Photographic Society, and also produced copies of many documents connected with the subject. The whole was published in the *Journal of the Photographic Society*, together with the discussions, and a large amount of correspondence appeared in the journals. No conclusion, however, appeared to be arrived at, nor any suggestions made of the process by which the pictures on paper could have been produced.

During the last winter, in company with Mr. James Nasmyth, I paid a visit to the Patent Museum. Through the kindness of Mr. Smith, we had an opportunity of examining these pictures carefully, hearing what he had to say on the subject, and seeing some of the original letters and papers. Mr. Smith also gave me a small portion from one of the torn pictures, and has since sent me another, for the purpose of careful examination. Through his kindness, also, I am enabled to exhibit to you this evening a number of the perfect pictures, in the hope that, by your seeing and examining them, some light may be thrown on the secret of their production.

It will be, perhaps, well to give you a short historical sketch of the pictures. Those who wish to refer at length to the published accounts, will find them in vol. viii. of the *Journal of the Photographic Society*.

The pictures in question consist of a number on paper—some large (so large, indeed, that it requires two sheets to form one subject), others small. They vary in shade of colour from black to dull red—many being of a sepia tone. Some are plain, others coloured. These pictures came from Boulton's old house at Soho, and many of them are evidently experiments, being marked with large figures in pencil. The plates are two silvered copperplates, also found in the old library at Soho. These we will, however, leave for the present, and proceed with the pictures on paper.

According to the evidence of letters and other documents produced by Mr. Smith, Matthew Boulton was in possession of a secret plan for producing what he called "mechanical pictures"—the inventor, or partial inventor, of the process being a person of the name of Eginton, who appeared to superintend this department of the Soho establishment. These pictures (all apparently copies of paintings) were produced rapidly, and at very low prices—from seven shillings and sixpence upwards, according to the size. They appear to have all the touches of a painting. Sometimes they were transferred to canvas and painted in oil colours, sometimes tinted on the paper itself, sometimes transferred to copper plates. Orders appear to have been given to artists for the sole purpose of having the paintings to copy or reproduce. They were sold in considerable numbers, and could, it appears, be produced of various sizes according to order. The pictures on paper were all reversed, the figures left-handed. When more than one sheet of paper was required for a subject, the picture was not joined in a straight line, but curved, so that the junction fell in the shadow, as in the leading of painted windows. Eginton was a glass painter, and perhaps took his idea from that source.

There is much interesting matter published concerning a proposed government pension to Eginton, and a letter to the Earl of Dartmouth on the subject from Matthew Boulton; also letters from Mr. M. P. W. Boulton and others; but, as they have no direct bearing on the mode of producing the pictures, I merely allude to them here.

Although it is well known that Watt, Boulton, Davy, Wedgwood, and other members of the Lunar Society experimented in photography, and tried to fix the images formed in the camera, we have the direct statement that, up to the year 1802, Wedgwood had been unsuccessful—that no amount of exposure appeared to produce an image. It is not likely, therefore, that, for years before (the mechanical production of pictures was in full operation in 1790), his intimate friend Matthew Boulton should not only have been in possession of a secret mode of fixing images of pictures but actually producing and selling large numbers of them. For this and other reasons we must, I think, decide that these pictures could not in any way have been produced in the camera; besides the great size and the perfect definition would be beyond the power of any instruments that could then be made. There is another argument, too—that if the camera could have been used, paintings would have not been the only subjects reproduced—views from nature, or, at any rate, works of art, would have been experimented upon.

On some of the pictures are seen curious small spots, each casting a shadow in the same direction—a very familiar appearance to those who have copied oil paintings in a raking light, when each raised spot of colour does actually cast its shadow. This has been considered an evidence of the pictures having been produced by the camera; but I shall further on be able to account for this appearance in another way.

Although we cannot call these pictures photographs, seeing that they have been produced by some different process from any we are acquainted with, we have the distinct evidence of Dr. Lee and Mr. Hodgson, *clear and unmistakeable*, that pictures produced by this mechanical process were pointed out by Matthew Boulton as having been produced in some way by sunlight.

Perhaps we shall do well to narrow the field of inquiry, by considering some of the suggestions that have been made, and showing how they could *not* have been produced.

We may decide that they could not be copies by hand:—1st. Because they are distinctly called "mechanical pictures," produced by a secret process. 2nd. Because in two copies of the same subject such minute lines and marks are found to exist in both—lines not visible without the microscope, quite impossible to be copied by hand. Besides, we have the testimony of those whose lives have been spent in engraving and making *facsimile* productions that the thing is impossible. They could not, I think, be impressions from metal or blocks. The peculiar surface of the paper, and the colouring matters used, would be quite unsuited to printing purposes. The great cost of engraving these large surfaces for but a limited number of copies; the different sizes in which the pictures could be made; the non-necessity, in such case, for the reversing of the pictures—are all good arguments against any system of engraving being used.

It has been said that the plate mark on some of the pictures suggests the employment of plate printing; but, on examination, as you will see, the supposed plate mark is merely an embossed line on the paper, either intended as a finish or, more probably, to mislead as to the mode of production.

The specimens for your examination consist—

1st. Of two similar pictures—one plain, the other coloured. A careful examination of these will show that spots and lines only visible with a lens exist exactly the same in each, such as no artist could possibly copy; yet, strange to say, there are considerable differences—some of them striking; but these are of precisely the same nature as you would have in an under-exposed and an over-exposed print, both from the same negative.

2nd. We have a coloured mechanical picture—subject, *The Graces Awakening Cupid.* This may be compared with an engraving of the same subject, evidently both taken from the original painting. In this you will notice the mechanical picture is reversed.

3rd. We have a red mechanical picture—*Flora Bedecking Pan.* This may also be compared with an engraving of the same subject, or rather one somewhat similar. In the engraving another figure is added; and there are other differences. In this case the original picture had probably been reproduced with additions, and the engraving taken from the later one.

4th. A large picture in two parts, from a painting by Benjamin West. You will notice, as peculiar, the mode in which the two portions of the picture are intended to be joined together; also that the two halves are not of the same tint, either in shade of colour or depth.

I will now give you the result of my examination of the pictures and the fragments given to me by Mr. Smith for the purpose of analysis. The surface of the paper appears, first, to have been prepared with gum and sugar. On that is the image impressed, consisting of finely divided particles, apparently laid on either in the form of vapour or very fine powder. Over the picture is a coating of albumen. This has been applied, most likely, by floating the picture on the surface of a vessel containing albumen. The picture has then, probably, been taken up carefully and allowed to drain for a short time, and then laid flat to dry. Small air bubbles, or particles of dust, on the surface would just produce the curious appearance of projections and shadows beforementioned—the powdery surface being slightly carried away and deposited, just as we see it. Those who have made experiments in photography—such as in the old carbon process, &c.—will at once fully understand my remarks. The albumen, in drying, has run into the hollows of the paper, as we see in the specimens. It is easy now to see how the images could be transferred to canvass, or painted upon on the paper.

How the images were formed I cannot even venture a suggestion. The process, if re-discovered, would be still valuable, even with our other and various modes of reproduction. For effect and beauty the specimens now shown are not to be despised; and, for permanency, have had the test of nearly eighty years.

Many of these pictures must be in existence in old houses and country inns; and, if more specimens were obtained, some clue to the secret might be found. I feel confident I have more than once seen specimens. Once, in particular, when on a photographic trip, in 1853, with our old member, Mr. Barton, either at Ludlow or Hereford, we saw several of them, and puzzled ourselves to make out what they were, with their striking photographic appearance. At length we decided that they must be sepia drawings.

The pictures on silver plates are two, and, on certain evidence, thought to be views of Soho House before the alteration. As this took place at the end of last century, these pictures, according to that idea, must have been taken at least sixty-six years ago. All we have to judge upon is this evidence, which is rather weak. The pictures are evidently taken in the camera, and are genuine photographs. From the evidence published in a pamphlet by Mr. M. P. W. Boulton, it appears highly probable that these pictures were taken about twenty-eight to thirty years ago by his aunt, Miss Wilkinson.

Independently, however, of this, I fear we must give them up as modern productions. Their appearance is that of daguerreotypes of the early period, made sensitive with iodine alone; and the image, as may be seen through the microscope, is composed of mercury vapour. But another piece of evidence appears to me more conclusive still, and that is the size of the plates. I have here a daguerreotype view of Rome, taken in the early days of the art—one of a number taken at that time in Rome and Paris. If we now compare the size of the plate with the Soho pictures, we shall find them identical. Such could scarcely be an accidental coincidence. These plates were not made or used for any other purpose; and it is not within the bounds of probability that the standard size of pictures, taken by Daguerre in 1835, should have been precisely the same as those used by James Watt in 1799. However, from the stamp on the corner of my plate, the peculiar form of the figure four (4) points it out as a French production; whilst the Soho plates are apparently English, rendering the possibility of their exact coincidence in size still more improbable.

"Speculations on the Process employed by Messrs. Boulton and Watt in the Production of the Pictures called by them 'Mechanical Pictures.'" By J. B. DANCER, F.R.A.S.

My remarks at present are confined to the two pictures on the table, of human figures, numbered 7 and 8. They measure 17×13$\frac{3}{8}$ inches, and are identical in size and subject. By the kindness of Mr. Sidebotham I have had an opportunity of making a minute examination of them. The only apparent difference between them is, that No. 7 is in plain ink, and in No. 8 the garments of the figures are coloured—one red and the other blue.

I am informed that these pictures are similar to those which were supplied by the firm of Boulton and Watt, and copies from originals sent to them. They were issued, it is said, with tolerable rapidity, and at a very moderate price; but the process by which they were multiplied was kept a profound secret.

At the first glance the pictures look as if they were produced by hand; but, on comparing them carefully, the close resemblance in the drawing in each is found to be so remarkable that no artist, however clever, could produce such exact duplicates without great expenditure of time. A more minute examination by means of a lens shows scratches and lines (evidently accidental) which correspond accurately in each picture, affording a convincing proof that these copies could only be produced by some mechanical or chemical agency.

It has been stated on good authority, I believe, that a darkened room or tent was used, also that the presence of sunlight was required in the process. Now this at once suggests the use of the camera obscura — an instrument which was perfectly well known at that period. A common method of exhibiting this instrument was to darken a room and fix a lens in a hole made in the window-shutter, and view the images produced on a screen of paper placed opposite to the lens. By this method a reversed and inverted picture would be seen by the spectator.

Another form of camera in use at that time consisted of an upright box with a sliding tube through the top, in which was fixed a lens, and above this a mirror placed at the angle of 45°. This form is still in use for the purpose of tracing images thrown on to paper placed at the bottom of the box. These primitive forms of the camera must be well known to all present. I name them only as affording some explanation for the use of the darkened room or tent.

The next question which suggests itself is this:—Did they employ the camera in producing images on chemically prepared surfaces? In fact, did they practice what we now name photography?

It is stated that in 1802 Wedgwood and Davy experimented with salts of silver on leather, and produced impressions of various objects. Now if we may be allowed to imagine that Mr. Boulton and his colleagues were acquainted with this effect of light on such substances, they might have reasoned on the phenomena, and prepared paper with these constituents — tannic acid, gelatine, and some salt of silver. Had they done so they might have produced a picture by sufficient exposure in the camera. One thing is certain, the pictures 7 and 8 have a plentiful coating of gelatine, albumen, or some other substance on their surface.*

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* My son William Dancer tested this substance and pronounced it to be gum.

At this stage of the inquiry I should like to ask a question of those who have inspected a large number of these pictures. Do any of them show unmistakeable marks of the originals, such as we are all familiar with in photographic copies? If this could be favourably answered, then photography of the present day would be benefited by the discovery of the means adopted in fixing these pictures. I much doubt if one of our best photographs on paper would contrast favourably with these pictures in the whites after a lapse of seventy years. But in my humble opinion photography has not been the agent employed in producing these pictures.

Mr. Sidebotham had a portion of one picture presented to him, and he kindly shared this with me. In my experiments I found that the colouring matter forming the picture could be washed off like ordinary ink, without leaving a trace of any chemical action on the paper. Now if this portion of the picture be a genuine sample, it does not exhibit the ordinary characteristics of a photograph.

On further consideration there are many reasons which would lead me to conclude that these pictures have been produced by some modification of mezzotint engraving, in which the camera had enabled the engraver to trace his outline from the original to any scale required, giving him the correct outline drawing, the shading being an after process; but the rapidity of production, and the low price charged, unfortunately excludes this process from the list of probabilities. Although mezzotint is said to be the most rapid method of engraving, I am compelled to abandon it or its modifications, and pass on to some other process.

Now if we do not believe in the photographic portion of the secret, and are not permitted to employ engraving on metal on account of the expense and labour, we find the field of speculation becoming somewhat limited.

Now comes a very important question:—Did Mr. Boulton anticipate Senefelder in some process similar to lithography, employing metallic plates or metallic alloys? I can almost imagine that such was the case. Even prepared paper surfaces, such as are named in Senefelder's patent specification, dated 1801, might have been used at Soho. When we look at the rough hand-made paper on which these impressions are placed, and see the thick coating of gum or gelatine on the surface, the question then arises—Is this prepared surface necessary for taking up the ink or whatever was used? or is it merely to smooth over the coarse wire lines in the paper? We do know that the want of smooth paper interfered with the practice of lithography for a long period. Machine-made paper was a great improvement, but now paper is made especially for the purpose.

After glancing over these processes, I am led to imagine that the following mode may in some degree serve to produce pictures similar to those before us:—Admitting that the impressions were touched up by hand, I think the camera was employed only for tracing the outline and reducing or enlarging the image to any required scale. The pictures Nos. 7 and 8 give good evidence of tracing. The shading would be an after process, having the original for a copy, the crayon employed being made of some resinous or fatty matter mixed with the colour—this production then to be used as a transfer on the prepared surface.

In such pictures intended for painting, the ink would be light in colour—in others red or black, as the taste of the artist dictated. In this manner, as Senefelder states in his specification, the various styles of etching, stroke engraving, drawings in black and red chalk and aquatinta, &c., &c., could be imitated, and at small cost compared with any other known process.

I shall now leave this very interesting inquiry in your hands. As an excuse for the brief and imperfect handling of the subject, I may state that it is only two days since I first saw the pictures, and I have not had sufficient time to work up the subject in a proper manner; but if my speculations serve to provoke a discussion amongst the members of the Section, its purpose will have been answered.

### Ordinary Meeting, April 3rd, 1866.

R. ANGUS SMITH, Ph.D., F.R.S., &c., President, in the Chair.

Messrs. Wm. Brockbank and G. C. Lowe were appointed Auditors of the Treasurer's accounts.

Mr. BINNEY, F.R.S., said that he had observed the humming bird hawkmoth (*Macroglossa Stellatarum*) during the past summer in far greater abundance than he ever remembered having seen it before. In the month of August, he saw upwards of a hundred of them in a garden near Grimsby, were they appeared to prefer the common lavender flower for food to any other in the place. Again in the first week of October, he observed upwards of twenty in a garden at Douglas, in the Isle of Man. Here they preferred to feed on heliotrope before other flowers. It was very interesting to watch these moths hovering over the flowers, and whilst on the wing extracting their food. They appeared very wary and shy after any attempt being made to capture them, but if you merely observed without making any attempt to molest them they would continue their feeding in confidence, and you could watch them at your leisure. So a great deal of the shyness and caution for which the little creature has got the credit of, is probably more due to the persevering efforts of its enemies to capture it than any natural fear of man.

A paper was read "On a Logical Abacus," by W. S. JEVONS, Esq., M.A.

The author believed that this was the first attempt, or at all events, the first successful attempt, to reduce the processes of logical inference to a mechanical form. The purpose of this contrivance is to show tbe simple truth, and the perfect generality of a new system of pure Qualitative Logic closely analagous to, and suggested by, the mathematical system of logic of the late Professor Boole, but strongly distinguished from the latter by the rejection of all considerations of quantity.

This logical abacus leads naturally to the construction of a simple machine which shall be capable of giving with absolute certainty all possible logical conclusions from any sets of propositions or premises *read off* upon the keys of the instrument. The possibility of such a contrivance is practically ascertained; when completed it will furnish a more signal proof of the truth of the system of logic embodied in it. Still the more rudimentary contrivance called the *abacus* will remain the most convenient for explaining the nature and working of formal inference, and may be usefully employed in the lecture room, for exhibiting the complete analysis of arguments and logical conditions, and the exposure of fallacies.

The abacus consists of—

1. An inclined black board, furnished with four ledges, 3ft. long, placed 9in. apart.

2. Series of flat slips of wood, the smallest set four in number, and other sets, 8, 16, and 32 in number, marked with combinations of letters, as follows:—

First Set.

| | | | |
|---|---|---|---|
| A | A | *a* | *a* |
| B | *b* | B | *b* |

Second Set.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| A | A | A | A | *a* | *a* | *a* | *a* |
| B | B | *b* | *b* | B | B | *b* | *b* |
| C | *c* | C | *c* | C | *c* | C | *c* |

The third and fourth sets exhibit the corresponding combinations of the letters A, B, C, D, *a*, *b*, *c*, *d*, and A, B, C, D, E, *a*, *b*, *c*, *d*, *e*.

The slips are furnished with little pins, so that when placed upon the ledges of the board, those marked by any given letter may be readily picked out by means of a straight-edged ruler, and removed to another ledge.

The use of the abacus will be best shown by an example. Take the syllogism in *Barbara* :—

Man is mortal.
Socrates is man.
Therefore Socrates is mortal.

Let

A = Socrates.
B = Man.
C = Mortal.

The corresponding small italic letters then indicate the negatives.

*a* = not-Socrates,
*b* = not-Man,
*c* = not-Mortal,

and the premises may be stated as

A is B,
B is C.

Now take the second set of slips containing all the possible combinations of A, B, C, *a*, *b*, *c*, and ascertain which of the combinations are possible under the conditions of the premises.

Select all the slips marked A, and as all these ought to be B's, select again those which are not B, or *b*, and reject them. Unite the remainder, and selecting the B's, reject those which are not C or *c*. There will now remain only four slips or combinations:

| | | | |
|---|---|---|---|
| A | *a* | *a* | *a* |
| B | B | *b* | *b* |
| C | C | C | *c* |

If we require the description of Socrates, or A, we take the only combination containing A, and observe that it is joined with C, hence the Aristotelian conclusion *Socrates is mortal.* We may also get any other possible conclusion. For instance the class of things *not-Man* or *b* is seen from the two last combinations to be always *a* or *not-Socrates*, but either *mortal* or *not-mortal* as the case may be.

Precisely the same obvious system of analysis is applicable to arguments however complicated. As an example take the premises treated in Boole's Laws of Thought, p. 125.

(1.) Similar figures consist of all whose corresponding angles are equal, and whose corresponding sides are proportional.

(2.) Triangles whose corresponding angles are equal have their corresponding sides proportional, and *vice versa.*

Let

A = similar.
B = triangle.
C = having corresponding angles equal.
D = having corresponding sides proportional.

The premises may then be expressed in Qualitative Logic,* as follows:—

$$A = CD.$$
$$BC = BD.$$

Take the set of 16 slips; out of the A's reject those which are not CD; out of the CD's reject those which are not A; out of the BC's reject those which are not BD; and out of the BD's reject those which are not BC. There will remain only six slips, as follows:—

| | | | | | |
|---|---|---|---|---|---|
| A | A | *a* | *a* | *a* | *a* |
| B | *b* | B | *b* | *b* | *b* |
| C | C | *c* | C | *c* | *c* |
| D | D | *d* | *d* | D | *d* |

From these we may at once read off all the conclusions laboriously deduced by Boole in his obscure processes. We at once see, for instance, that the class *a*, or "dissimilar figures, consist of all triangles (B) which have not their corresponding angles equal (*c*) and sides proportional (*d*), and of all figures not being triangles (*b*) which have either their angles equal (C) and sides not proportional (*d*), or their corresponding sides proportional (D) and angles not equal, or neither their corresponding angles equal nor corresponding sides proportional." (Boole, p. 126.)

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* *See* Pure Logic, or the Quality of Logic, apart from Quantity, by W. Stanley Jevons, M.A., London (Stanford), 1864.

The selections as made upon the *abacus* are of course subject to mistake, but only one easy step is required to a logical machine, in which the selections shall be made mechanically and faultlessly by the mere reading down of the premises upon a set of keys, or handles, representing the several positive and negative terms, the copula, conjunctions, and stops of a proposition.

Mr. Jevons stated his opinion distinctly that these contrivances possessed a theoretical rather than a practical importance. Like the analogous Calculating Machine of Babbage or Scheutz, the logical machine would hardly find practical employment for the present at least. But its value consisted in showing the true nature of logic as a system of analysis of the possible combinations of things, in short as the highest and simplest form of the doctrine of combinations. Not only would the deductive, and especially the inductive processes of logic be thus presented in a new and clearer light, but the relation of logic, the qualitative doctrine of combinations, to mathematics the quantitative doctrine of combinations, would be defined, and the abstract sciences thus brought into harmony and due subordination.

In the description of his balance given in the last No. of the Proceedings, Dr. JOULE omitted to mention a fixed support against which the scale rests when the counterbalance is removed. By this means the wires are kept constantly in the same state of tension, and are thus preserved from the derangement which might otherwise ensue.

## MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

### March 26th, 1866.

A. G. Latham, Esq., President of the Sections, in the Chair.

The following objects were exhibited:—

Eight mounted specimens of hair of Australian animals for the cabinet; one of them, a species of Phascogale, very remarkable.—Mr. Latham.

A large collection of rare beetles from Ceylon, recently presented to the Natural History Society by —— Braybrooke, Esq.—Mr. Latham.

Many specimens of remarkable foraminifera from Dogs Bay.—Mr. Linton.

A sample of the Guano lately imported from Malden Island in the Pacific, for distribution among the members.—Mr. Latham.

Dr. Alcock showed mounted specimens of Embryonic shells of Mollusca, including fifty species collected by him from Dogs Bay sand, and named by J. Gwyn Jeffreys, Esq.—He said he had in a former communication described the peculiar characters of Anomia in the young state, and shells of this kind are abundant in the sand. Pectens are also common, and six different forms which he had sorted out are referred by Mr. Jeffreys to the following species:—P. varius, opercularis, tigrinus, testæ, similis, and maximus. Lima subauriculata and L. Loscombii are both rather scarce. Modiolaria discors in the young state is very common. Arca tetragona, abundant; Kellia suborbicularis, common; Cardium echinatum, rare; Cardium fasciatum, very common; several species of Venus, frequent; and Saxicava rugosa, very abundant. Patella vulgata, Helcion pellucidum, and Tectura virginea with spiral caps are all common, the last being very abundant. Another limpet-like shell without spiral cap, referred to in a former paper, is identified by Mr. Jeffreys as Ancylus fluviatilis. Emarginula fissura is rare, and of Fissurella Græca, one specimen only has been found. Cæcum glabrum with spiral attached is common, and separate spirals are abundant; only one specimen of Aclis unica has been met with; Eulimella nitidissima is common; Homatogyra rota, rare; Cerithiopsis adversa, common. A few of the embryonic and partly grown shells of Aplysia depilans have been found, but they are very scarce. In addition to the named specimens, a considerable number of other species not yet identified were exhibited.

### List of Embryonic Shells of Mollusca

Anomia ephippium.
Pecten varius.
„ opercularis.
„ tigrinus.
„ testæ.
„ similis.
„ maximus.
Lima subauriculata.
„ Loscombii.
Mytilus phaseolinus.
Modiolaria discors.
Arca tetragona.
Montacuta bidentata.
„ ferruginosa.
Kellia suborbicularis.
Sphærium corneum.
Cardium echinatum.
„ fasciatum.
Venus lincta.
Venus.
Tellina tenuis.
Scrobicularia alba.
Lyonsia Norvegica.
Thracia papyracea.
Saxicava rugosa.

Patella vulgata.
Helcion pellucidum.
Tectura virginea.
Emarginula fissura.
Fissurella Græca.
Homatogyra rota.
Cæcum glabrum.
Cerithium reticulatum.
Cerithiopsis adversa.
Scalaria communis.
Aclis unica.
Eulima distorta.
Chemnitzia elegantissima.
Odostomia unidentata.
Eulimella nitidissima.
„ acicula.
Velutina lævigata.
Lamellaria perspicua.
Purpura lapillus?
Mangelia linearis.
Cylichna cylindracea.
„ truncata.
Aplysia depilans.
Spirialis Flemingii.
Ancylus fluviatilis.

## PHYSICAL AND MATHEMATICAL SECTION.

March 29th, 1866.

ROBERT WORTHINGTON, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. BAXENDELL, F.R.A.S., communicated the following:—

### RESULTS OF RAIN GAUGE AND ANEMOMETER OBSERVATIONS MADE DURING THE YEAR 1865,

At St. Martin's Parsonage, Castleton Moor, by the Rev. J. CHADWICK BATES, M.A., F.R.A.S.

TABLE I.

| | 5-INCH GAUGES. | | | 8-INCH GAUGES. | | | Total Movement of Wind. |
|---|---|---|---|---|---|---|---|
| | 20 feet elevation. | 5 feet elevation. | 1 foot elevation. | 20 feet elevation. | 5 feet elevation. | 1 foot elevation. | |
| January ......... | 3·177 | 3·492 | 3·874 | 3·292 | 3·631 | 4·050 | 7156 |
| February ......... | 2·344 | 2·493 | 2·934 | 2·572 | 2·775 | 3·033 | 5875 |
| March ............ | 1·055 | 1·161 | 1·359 | 1·111 | 1·246 | 1·396 | 7275 |
| April ............... | 1·337 | 1·377 | 1·444 | 1·377 | 1·410 | 1·474 | 7074 |
| May ............... | 2·297 | 2·622 | 2·863 | 2·278 | 2·647 | 2·784 | 6934 |
| June ............... | 0·656 | 0·668 | 0·659 | 0·670 | 0·666 | 0·669 | 5213 |
| July ............... | 3·224 | 3·407 | 3·546 | 3·260 | 3·347 | 3·412 | 5751 |
| August ............ | 4·645 | 4·892 | 5·099 | 4·733 | 4·862 | 4·941 | 5591 |
| September ......... | 0·548 | 0·614 | 0·657 | 0·580 | 0·628 | 0·652 | 4561 |
| October ............ | 5·777 | 5·010 | 6·251 | 5·858 | 6·030 | 6·230 | 6621 |
| November ......... | 3·303 | 3·508 | 3·829 | 3·384 | 3·608 | 3·774 | 6097 |
| December ......... | 1·092 | 1·234 | 1·318 | 1·158 | 1·207 | 1·299 | 6121 |
| | 29·455 | 31·478 | 33·830 | 30·273 | 32·057 | 33·714 | 74269 |

TABLE II.

| | No. of Days of Rain. | Total Movement of the Wind. | Mean Daily Movement of Wind on Days of Rain. | No. of Fair Days. | Total Movement of the Wind. | Mean Daily Movement of Wind on Fair Days. |
|---|---|---|---|---|---|---|
| January | 13 | 4394 | 338 | 18 | 2762 | 153 |
| February | 15 | 3622 | 241 | 13 | 2253 | 173 |
| March | 11 | 2873 | 261 | 20 | 4402 | 220 |
| April | 8 | 2103 | 263 | 22 | 4971 | 226 |
| May | 18 | 4501 | 250 | 13 | 2433 | 187 |
| June | 4 | 1155 | 288 | 26 | 4058 | 156 |
| July | 14 | 2822 | 201 | 17 | 2929 | 172 |
| August | 16 | 3284 | 205 | 15 | 2307 | 153 |
| September | 2 | 530 | 265 | 28 | 4021 | 143 |
| October | 18 | 3847 | 213 | 13 | 2774 | 213 |
| November | 14 | 3958 | 282 | 16 | 2139 | 133 |
| December | 8 | 2709 | 338 | 23 | 3418 | 148 |
| | 141 | 35798 | 253·8 | 224 | 38461 | 171·7 |

From Table I. it will be seen that the total movement of the wind was greatest in the month of March, and least in September. In the year 1864 it was also greatest in March, but least in August. The total amounts in the quarterly periods of the two years were:—

| | 1865. | 1864. |
|---|---|---|
| Winter | 19152 miles. | 19174 miles. |
| Spring | 21283 „ | 20940 „ |
| Summer | 16555 „ | 18111 „ |
| Autumn | 17269 „ | 20088 „ |

Table II. shows that the mean daily movement of the wind on days of rain in 1865 was 253·8 miles; and on days without rain, 171·7. The numbers in 1864 were 251·6, and 179·4 respectively.

The mean results for the quarterly periods, and their ratios, were:—

| | Mean Daily Movement of the Wind on Rainy Days. | Mean Daily Movement of the Wind on Fair Days. | Ratios. |
|---|---|---|---|
| Winter | 298 | 156 | 0·52 |
| Spring | 256 | 214 | 0·83 |
| Summer | 213 | 160 | 0·75 |
| Autumn | 245 | 156 | 0·63 |

The ratios in 1864 were, Winter, 0·68; Spring, 0·82; Summer, 0·72; Autumn, 0·60. It appears therefore from the observations of the two years that the ratio of the mean velocity of the wind on days when no rain falls, as compared with that on rainy days, varies least in the spring, and most in the winter quarter.

Mr. W. L. DICKINSON read a Paper containing the results of calculations relative to the Eclipse of the Sun, and to two Occultations of the star Aldebaran by the Moon, visible here this year. The calculations have been made for the Observatory of Robert Worthington, Esq., F.R.A.S., Crumpsall, near Manchester, Lat. 53° 30′ 50″·0 N., Long. $0^{h}\,8^{m}\,56^{s}{\cdot}16$ W. The Elements used in the computations have been obtained from the Nautical Almanac.

The Partial Eclipse of the Sun, October 8, 1866, is partly visible at the Observatory, and

| | h. | m. | s. | |
|---|---|---|---|---|
| Begins.............. | 4 | 19 | 39 | Mean Time at Greenwich. |
| Greatest Phase ....... | 5 | 21 | 35 | |

At Crumpsall the Sun will set at 5h. 27m.

Magnitude of the Eclipse (Sun's diameter = 1) 0·480.

Angle, from North Pole, of first contact, 43° .............. } towards the West for *direct* image.
Angle, from Vertex, of first contact, 76° ............... }

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The Occultations of the star *a* Tauri (Aldebaran) by the Moon.

| | Disappearance. | | | | |
|---|---|---|---|---|---|
| | Sidereal Time at Observatory. | Mean Time at Observatory. | Mean Time at Greenwich. | Angle from North Point. | Angle from Vertex. |
| 1866. | h. m. s. | h. m. s. | h. m. s. | ° | ° |
| September 28th... | 3 41 36 | 15 10 56 | 15 19 52 | 84 | 75 |
| November 22nd... | 1 53 44 | 9 47 6 | 9 56 2 | 73 | 44 |

| | Reappearance. | | | | |
|---|---|---|---|---|---|
| | Sidereal Time at Observatory. | Mean Time at Observatory. | Mean Time at Greenwich. | Angle from North Point. | Angle from Vertex. |
| 1866. | h. m. s. | h. m. s. | h. m. s. | ° | ° |
| September 28th... | 4 51 41 | 16 20 49 | 16 29 45 | 295 | 303 |
| November 22nd... | 2 51 55 | 10 45 8 | 10 54 4 | 310 | 290 |

The Angles are reckoned towards the right hand round the circumference of the Moon's image as seen in an inverting telescope.

Mr. Brothers, F.R.A.S., stated that when a solution of nitrate of silver, which, from long use, has become contaminated with organic matter from collodion plates, is neutralized, or made alkaline with carbonate of soda, exposed to strong day-light in an evaporating dish, and heat applied, a scum is quickly formed on its surface. This scum, when broken by a puff of air exhibits, in a remarkable manner, all the appearances of solar spots as seen in good telescopes. So long as the current of air is continued the scum remains open at the spot, but immediately closes *partially* on the cessation of the cause, and streams of the film stretch across the opening, illustrating the *bridges* over the sun spots. At the same time a secondary scum commences to form at the edges of the opening, and may be called the *penumbra*, and in time closes the opening. The centre is occupied by the *umbra* formed by the carbonate of silver (white when first formed) which has in the course of the experiments turned *black*. It was found that when a current of the solution was forced upwards these effects could not be produced, and the film was not affected when a small body was dropped into it. When the solution has been exposed to the heat for a few hours the film becomes too thick to exhibit the experiment, and when cold the appearances described could not be produced. This experiment was referred to merely as illustrating the *appearances* of sun spots, and in no way as explaining their cause. At the same time it suggested the idea that if the luminous photosphere of the sun is formed of bodies, named by various observers, "willow leaves" (Nasmyth), "rice grains" (Stone), "bits of straw" (Dawes), and of the existence of distinct bodies of some kind on the visible surface of the sun there can now be very little doubt, they may be floating in a fluid sufficiently dense to sustain them, but at the same time easily thrust aside by some disturbing cause below the surface. The existence of an external cause at the surface of the sun being improbable, may not the cause of the sun spots arise from a current or force, such as the "*red flames*," which are supposed to be connected with the formation of spots? This force acting on the "willow leaves" would raise them from the level at which they may be supposed to float, they would slide under and over each other, and thus leave an opening; and, upon the gradual cessation of the disturbing cause, the tendency of the "willow leaves" would be to gradually assume their former positions and close up the spots in a way similar to the closing of the film in the simple experiment referred to.

Mr. BROTHERS also stated that, while observing the moon with his five-inch achromatic telescope at about eight o'clock on the evening of March 25th, he observed a small *dark* body cross the disc diagonally, from left to right, a little below the spot Copernicus. The motion was very rapid and similar to the passage of a luminous meteor across the field of view. He conceived it might be a meteoric body passing through space at a distance considerably beyond the limits of the earth's atmosphere.

## Annual Meeting, April 17th, 1866.

R. Angus Smith, Ph. D., F.R.S., &c., President, in the Chair.

The following report of the Council was read by one of the Secretaries: —

The Council have again the satisfaction to report that although the balance of the Treasurer's account has been reduced from £360. 4s. 3d. on the 1st of April, 1865, to £269. 4s. 3d. on the 1st of April, 1866, the finances of the Society are nevertheless in a healthy state. Extra expenditure has been incurred during the past year for printing and publishing vol. 2, series 3, of the Society's Memoirs; binding books in the library; printing a catalogue of the Society's library; and purchasing new books, and also volumes to complete imperfect sets.

The number of ordinary members on the roll of the Society on the 1st of April, 1865, was 187. Five new members have been elected during the year; and the losses have been by deaths, six; resignations, one; and defaulters, two. The number of members on the roll on the 1st of April, 1866, was therefore 183. The deceased members are—Rev. Thos. Buckley, M.A., Mr. James Joseph Dean, Mr. John Parry, Sir Benjamin Heywood, Bart., F.R.S., Mr. John Whalley, and Mr. Richard Hampson.

Sir Benjamin Heywood, Bart., F.R.S., was one of the oldest members of the Society, having been elected on the 27th of January, 1815. He held the office of Treasurer for many years, and was one of the Trustees of the Society's property. He took an active interest in local literary and scientific institutions, and was one of the founders of the Manchester Royal, and Mechanics', Institutions. He held the presidency of the latter institution from 1824 to 1841. In 1831–32 he represented the county in parliament, and whilst in London became an intimate associate of the prominent literary and scientific men of the time.

Mr. John Parry had been a member of the Society for 32 years, having been elected on the 26th of April, 1833. He was a constant attendant at the ordinary and sectional meetings of the Society, and took an active part in the formation of the Microscopical, Natural History, and Photographical Sections. He was the inventor of coloured signal lamps for railways, the first lamps of this kind having been made by the late Mr. Ford, of Cateaton-street, under his directions, and used on the Manchester and Leeds Railway. He had been forty-three years in the confidential employment of Messrs. Lockett and Co., the well known engravers to calico printers, and was one of the first to employ photography in connection with that business, in which he also introduced the electrotype process in 1839. His mechanical skill and ingenuity were displayed in the construction of a large solar microscope on a principle similar to that of the one exhibited at the Adelaide Gallery, London, about 1839 or 1840; in making a panoramic camera, camera bellows, apparatus for waxing paper, and for drying albumen plates; and in cutting and polishing sections of fossils for microscopic slides. He was an enthusiastic photographer, and had been very successful in producing enlarged photographs of microscopic objects. In the summer of last year he visited the coast of Galway, where he succeeded in finding a rich deposit of Foraminifera containing many new species, descriptions and lists of which have been given by Dr. Alcock in the Society's Proceedings.

The several Sections of the Society have continued to exhibit their usual activity, and have furnished many important communications to the Proceedings, and to the Memoirs of the Society. The admission of Sectional Associates has again been found to work satisfactorily, and the Council have therefore resolved that the system shall remain in force during another year.

The following papers and communications have been read at the ordinary and sectional meetings of the Society during the past session:—

*October 3rd*, 1865.—"On the Internal Heat of the Earth as a Source of Motive Power," by Mr. George Greaves, M.R.C.S.

"Note on the Eclipse of the Sun, October 19th, 1865," by Mr. W. L. Dickinson.

*October 5th*, 1865.—"On Photographs of the Eclipse of the Moon, October 4th, 1865," by Mr. A. Brothers, F.R.A.S.

"On a Camera for outdoor work without a Tent," by Dr. J. P. Joule, F.R.S., &c.

*October 12th*, 1865.—"Rainfall at Eccles for 1864," by Mr. Thomas Mackereth, F.R.A.S.

*October 16th*, 1865.—"Notes on Atlantic Soundings," by Mr. Joseph Sidebotham.

"Notes on Acherontia atropos," by Mr. Joseph Sidebotham.

"Questions regarding the Life-History of the Foraminifera, suggested by Examinations of their Dead Shells," by Mr. Thomas Alcock, M.D.

*October 17th*, 1865.—"Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes, Part I.," by Mr. J. C. Dyer, V.P.

*October 31st*, 1865.—"On Cooling and Ventilating the Workings of deep Coal Mines," by Sir J. F. W. Herschel, Bart., M.A., D.C.L., F.R.S., &c.

"On Coresolvents," by the Hon. Chief Justice Cockle, M.A., &c., President of the Queensland Philosophical Society. Communicated by the Rev. Robert Harley, F.R.S., &c.

*November 9th*, 1865.—"Note on the Variable Star, S Delphini," by Mr. J. Baxendell, F.R.A.S.

*November* 14*th*, 1865.—"On a Predisposing Cause of the Cattle Disease," by Mr. Thomas Ainsworth.

"An Attempt to refer some Phenomena attending the Emission of Light to Mechanical Principles," by Mr. R. B. Clifton, M.A., Professor of Natural Philosophy in Owens College.

*November* 20*th*, 1865.—"On Collecting Foraminifera on the West Coast of Ireland," by Mr. John Parry.

"On the Illumination of Opaque Objects under the high powers of the Microscope," by Mr. J. B. Dancer, F.R.A.S.

*November* 28*th*, 1865.—"On the Amount of Carbonic Acid contained in the Air above the Irish Sea," by Mr. T. E. Thorpe, Assistant in the Private Laboratory, Owens College." Communicated by Professor H. E. Roscoe, F.R.S.

*December* 7*th*, 1865.—"On the November Meteors, as observed at Woodcroft, Cuckfield, Sussex, November 12–13, 1865," by Mr. George Knott, F.R.A.S. Communicated by Mr. Baxendell, F.R.A.S.

*December* 12*th*, 1865.—"Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes, Part II.," by Mr. J. C. Dyer, V.P.

*December* 14*th*, 1865.—"On the Application of Measuring Rods to Photographic Pictures," by Mr. Joseph Sidebotham.

"On Celestial Photography," by Mr. A. Brothers, F.R.A.S.

*December* 18*th*, 1865.—On late Improvements in Illuminating Opaque Objects under the higher Powers of the Microscope," by Mr. H. A. Hurst.

*December* 26*th*, 1865.—"On Fossil Wood found in Calcareous Nodules in the lower Coal Seams of Lancashire and Yorkshire," by Mr. E. W. Binney, F.R.S., &c.

*January* 4*th*, 1866.—Remarks on the Barometric Disturbances during the Months of October, November, and December, 1865," by Mr. G. V. Vernon, F.R.A.S.

"Rainfall for 1865 at Old Trafford," by Mr. G. V. Vernon, F.R.A.S.

"Note on the Variable Star, T Aquilæ," by Mr. J. Baxendell, F.R.A.S.

*January 23rd*, 1866.—"Notes on Section of Chat Moss, near Astley Station," by Mr. W. Brockbank.

*January 29th*, 1866.—"On the Humming-bird Hawk-moth," by Mr. Linton.

*February 1st*, 1866.—"Results of Rain-guage and Anemometer Observations made at Eccles, near Manchester, during the year 1865," by Mr. Thomas Mackareth, F.R.A.S.

"Note on the Variable Star, S Coronæ," by Mr. J. Baxendell, F.R.A.S.

"On the Determination of the Mean Form of the Light-curve of a Variable Star," by Mr. J. Baxendell, F.R.A.S.

*February 6th*, 1866.—"Notes on the Origin of several Mechanical Inventions, and their subsequent application to different purposes, Part III.," by Mr. J. C. Dyer, V.P.

*February 8th* 1866.—"On the Supposed Photographs by Boulton and Watt," by Mr. Joseph Sidebotham.

"Speculations on the Process employed by Messrs. Boulton and Watt in the Production of the Pictures called by them Mechanical Pictures," by Mr. J. B. Dancer, F.R.A.S.

*February 20th*, 1866.—On a New Species of *Dadoxylon*, found in the Upper Foot Coal, near Oldham," by Mr. E. W. Binney, F.R.S., F.G.S.

"On Air from off the Atlantic, and from some London Law Courts," by Dr. R. Angus Smith, F.R.S., &c., President.

*March 1st*, 1866.—"On the Variable Star, R Vulpeculæ," by Mr. G. Knott, F.R.A.S. Communicated by Mr. J. Baxendell, F.R.A.S.

"On the Fall of Rain during the Different Hours of the Day, as deduced from a Series of Observations made by the Rev. J. C. Bates, M.A., F.R.A.S., at St. Martin's Parsonage, Castleton Moor," by Mr. J. Baxendell, F.R.A.S.

*March 6th*, 1866.—"Note on the Purification of Platinum," by Mr. E. Sonstadt.

"On the Liassic and Oolitic Iron Ores of Yorkshire and the East Midland Counties," by Messrs. E. Hull, F.G.S., and William Brockbank.

*March 8th*, 1866.—"On the Pantascopic Camera." by Mr. J. R. Johnson. Communicated by Mr. E. C. Buxton.

*March 20th*, 1866.—"On a New Balance," by Dr. J. P. Joule, F.R.S., &c.

"Notes on Cotton Spinning Machinery, Part II., Roving Frames," by Mr. J. C. Dyer, V.P.

*March 26th*, 1866.—"On Embryonic Shells of Mollusca, collected from Dogs Bay Sand," by Dr. Thomas Alcock.

*March 29th*, 1866.—"Results of Rain-gauge and Anemometer Observations made during the year 1865, at St. Martin's Parsonage, Castleton Moor," by the Rev. J. C. Bates, M.A., F.R.A.S. Communicated by Mr. J. Baxendell, F.R.A.S.

"On the Eclipse of the Sun, October 8th, 1866; and on the Occultations of Aldebaran by the Moon, September 28th, and November 22nd, 1866," by Mr. W. L. Dickinson.

"On a Method of Imitating the Appearances of Sun Spots," by Mr. A. Brothers, F.R.A.S.

*April 3rd*, 1866.—"On the Humming-bird Hawk-moth," by Mr. E. W. Binney, F.R.S., &c.

"On a Logical Abacus," by Mr. W. S. Jevons, M.A.

The Council desire to express their sense of the value of the services rendered to the Society by the Honorary Librarian, Thomas Windsor, Esq., M.R.C.S., in arranging the Society's library, and making out and printing a catalogue of the books; and they regret that he is unable to continue his services. At his request Mr. Charles Bailey has, for some months past, ably discharged the duties of Librarian.

The Committee appointed to discuss the question of the medal have agreed upon certain resolutions concerning the mode of award of the Society's medal; but no award has yet been made.

The Librarian reports that since the last annual meeting a catalogue of the books in the library has been printed, and that arrangements are in progress for printing a supplementary catalogue of the books received since that time. Vol. 2, series 3, of the Society's Memoirs, and vols. 3 and 4 of the Proceedings, have been bound, and are now being forwarded to the Societies and Institutions with which this Society exchanges publications. Many volumes have been bound during the year, and several purchases of books to complete sets have been made. The general condition of the books is satisfactory.

On the motion of Mr. TRAPP, seconded by Mr. B. ST. J. B. JOULE, the Annual Report was unanimously adopted.

The following gentlemen were elected officers of the Society for the ensuing year:—

### President.

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.

### Vice-Presidents.

ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S., &c.
JAMES PRESCOTT JOULE, LL.D., F.R.S., &c.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
JOSEPH CHESBOROUGH DYER.

### Secretaries.

HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S., F.C.S.
JOSEPH BAXENDELL, F.R.A.S.

### Treasurer.

ROBERT WORTHINGTON, F.R.A.S.

### Librarian.

CHARLES BAILEY.

### Other Members of the Council.

REV. WILLIAM GASKELL, M.A.
PETER SPENCE, F.C.S., M.S.A.
GEORGE VENABLES VERNON, F.R.A.S., M.B.M.S.
JOSEPH SIDEBOTHAM.
JOHN BENJAMIN DANCER, F.R.A.S.
MURRAY GLADSTONE, F.R.A.S.

## ROBERT WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

### FROM 31st MARCH, 1865, TO 31st MARCH, 1866.

**Dr.**

| Date | Particulars | No. | £ s. d. | £ s. d. | £ s. d. |
|---|---|---|---|---|---|
| 1865. April 1. | —To Balance in the Bank of Heywood Brothers and Co. | | | 335 0 3 | |
| " " | ,, Balance in Treasurer's hands | | | 25 4 0 | 360 4 3 |
| 1866. Mar 31. | —To *Members' Contributions:* | | | | |
| | Arrears, 1864-5 | 4 | | 8 8 0 | |
| | ,, Half Subscription, 1865 | 1 | | 1 1 0 | |
| | ,, Admission Fee, 1865 | 1 | | 2 2 0 | |
| | Members on the Roll, April 1, 1865, 187, at 42s. | | £392 14 0 | | |
| | Deduct Compounders | 3 | | | |
| | ,, Dead | 3 | | | |
| | ,, In Arrear, March 31, 1866 | 22 | 58 16 0 | | |
| | | 28 | | 333 18 0 | |
| | Four Members elected in 1865, April—Dec. | | £8 8 0 | | |
| | Deduct in Arrear | 2 | 4 4 0 | | |
| | | | | 4 4 0 | |
| | One Member elected in 1866, half-subscription | | 1 1 0 | | |
| | Deduct in Arrear | | 1 1 0 | | |
| | Two Admission Fees received, 1865-6 | | | 4 4 0 | |
| | | | | | 353 17 0 |
| | Ten Associates of Microscopical Section, at 10s. 0d. | | | 5 0 0 | |
| | Ten Associates of Photographical Section, at 10s. 6d. | | | 5 5 0 | |
| | One Associate of Physical and Mathematical Section, at 10s. 6d. | | | 0 10 6 | |
| | | | | | 10 15 6 |
| ,, ,, | To *Sale of Publications:* | | | | |
| | Memoirs—Ser. 3, Vol. 1 | | | 1 1 0 | |
| | ,, Ser. 3, Vol. 2 | | | 5 15 0 | |
| | ,, Ser. 2, Vol. XI. | | | 0 4 0 | |
| | ,, Ser. 2, Vol. XII. | | | 0 5 0 | |
| | Proceedings | | | 1 2 6 | |
| | Memoir of Dalton | | | 0 5 3 | |
| | | | | | 8 12 9 |
| ,, ,, | To *Sundry Income:* | | | | |
| | Sec. Contrib. Microscopical | | £4 4 0 | | |
| | ,, ,, Photographical | | 2 2 0 | | |
| | ,, ,, Physical and Mathematical | | 2 2 0 | | |
| | | | | 8 8 0 | |
| ,, ,, | Interest allowed by Bankers | | | 11 12 0 | |
| | | | | | 20 0 0 |
| | | | | | £753 9 6 |

| Particulars | £ s. d. |
|---|---|
| Composition Fund | £72 10 0 |
| General Balance | 196 14 3 |
| | £269 4 3 |

**Cr.**

| Particulars | £ s. d. | £ s. d. | £ s. d. |
|---|---|---|---|
| March 31, 1866. | | | |
| By *Charges on Property:* | | | |
| Chief Rent | | 12 13 7 | |
| Fire Insurance | | 5 12 6 | |
| Property Tax | | 2 16 8 | |
| | | | 21 2 9 |
| ,, *House Expenditure:* | | | |
| Water, Gas, Candles, and Coal | | 23 10 2 | |
| Cleaning and Petty Expenses | | 2 2 10 | |
| Repairs and Alterations | | 15 15 9 | |
| Tea and Coffee at meetings | | 17 6 0 | |
| | | | 58 14 9 |
| ,, *Administrative Charges:* | | | |
| Wages—Keeper of Rooms | £42 12 0 | | |
| ,, —Assistant ditto | 1 10 0 | | |
| | | 44 2 0 | |
| Attendance on Sections | | 8 8 0 | |
| Postage and Parcels | | 16 18 9 | |
| Stationery, Printing Circulars, &c., and Receipt Stamps | | 9 10 0 | |
| | | | 78 18 9 |
| ,, *Publishing:* | | | |
| Memoirs—Printing, Engraving, &c. | | 103 7 0 | |
| Printing Proceedings | | 43 14 6 | |
| Editor of Memoirs and Proceedings | | 50 0 0 | |
| | | | 197 1 6 |
| ,, *Library:* | | | |
| Periodicals, Binding, &c. | | 71 4 11 | |
| Executor of E. F. Ekman, for Books, &c. | | 35 12 1 | |
| Printing Catalogue of Library | | 17 10 0 | |
| Subscription to Ray Society | | 1 1 0 | |
| ,, Palæontographical Society | | 1 1 0 | |
| | | | 126 9 0 |
| ,, *Sundry Expenditure:* | | | |
| Solicitors for Preparing Bond from Keeper of Rooms | | | 1 18 6 |
| Total Disbursements | | | 484 5 3 |
| Balance in Bank of Heywood Brothers and Co. | | 267 2 3 | |
| ,, Treasurer's hands | | 2 2 0 | |
| | | | 269 4 3 |
| | | | £753 9 6 |

2nd April, 1866.

ROBERT WORTHINGTON, TREASURER.

Examined and found correct, 10th April, 1866.

GEO. CLIFF LOWE, } AUDITORS.
WM. BROOKBANK, }

A paper was read "On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes, Part I," by James Nasmyth, C.E., Corresponding Member of the Society.

In this, the first part of his paper, the author describes in considerable detail the methods and processes by which he produces speculum metal of the best quality, and casts, anneals, and rough-grinds a speculum of ten inches in diameter, his descriptions being illustrated by drawings of the apparatus he employs. In the second part of the paper he will describe the processes of fine-grinding, polishing, and figuring; and will give directions for the general management and use of reflecting telescopes. His instructions are based upon the results of thirty years' experience in the art of working specula, and will, he believes, enable any zealous amateur to make for himself, at a moderate cost, a really good and useful reflector.

## PHOTOGRAPHICAL SECTION.

April 12th, 1866.

Dr. J. P. Joule, F.R.S., &c., Vice-President of the Section, in the Chair.

A paper was read entitled "Note on the First Use of Hyposulphite of Soda in Photography," by A. Brothers, F.R.A.S.

During an investigation into the early history of photography, I met with the statement that Daguerre used hyposulphite of soda in his process for fixing the pictures, and also that in Mr. Talbot's patent the use of that substance was included. I was under the impression that Sir John Herschel had pointed out that hyposulphite of soda would fix the photographic image, but was unable to ascertain where or when the discovery was first published. In order to determine this point I wrote to Sir John Herschel, requesting him to inform me whether the discovery was his, and the date when it was published. To these questions I received the following reply: —

Colingwood, October 29, 1864.

Sir,

I think I may very fairly claim the discovery of the hyposulphites as fixing agents, as I believe I was the first to call the attention of chemists to that class of salts and their peculiar habitudes, especially in relation to the insoluble salts of silver.

In my paper "On the Hyposulphurous Acid and its Compounds," which bears date Jan. 8, 1819, and which appeared in Brewster and Jamieson's *Edinb. Phil. Journal,* 1819, occur these words: —

"One of the most singular characters of the hyposulphites is the property their solutions possess of dissolving muriate of silver and retaining it in considerable quantities in permanent solution." (page 11.)

"*Hyposulphite of potash.*—It dissolves muriate of silver even when very dilute, with great readiness." (p. 19.)

"*Hyposulphite of soda.* . . . . . . Muriate of silver newly precipitated dissolves in this salt when in a somewhat concentrated solution in large quantity and almost as readily as sugar in water." (p. 19.)

"*Hyposulphite of strontia.* . . . . . Like the rest of the hyposulphites it readily dissolves muriate of silver, and alcohol precipitates it as a sweet syrup." (p. 21.)

'*Hyposulphite of silver.*—Muriate of silver newly precipitated is soluble in all liquid hyposulphites, and, as before observed, in that of soda with great ease and in large quantities. This solution is not accomplished without mutual decomposition, as its intense sweetness proves—a sweetness surpassing that of honey, and diffusing itself over the whole mouth and fauces, without any disagreeable or metallic flavour." (p. 27.)

In a second paper on the same subject, which appeared in the same jonrnal, vol. 1, p. 396 *et seq.*, it is shown (*inter alia*) that the affinity of this acid for silver is such that oxide of silver readily decomposes hyposulphite of soda and likewise the soda in a caustic state, "the only instance, I believe, yet known of the direct displacement of a fixed alkali *viâ humidâ* by a metallic oxide." (p. 397.)

"*Hyposulphite of ammonia and silver.*—Its sweetness is unmixed with any other flavour, and so intense as to cause pain in the throat . . . . . one grain communicates a perceptible sweetness to 30,000 grs. of water." (p. 399.)

In a third communication, dated November, 1819—"The habitudes of this acid with the oxide of mercury are not less singular than its relation to that of silver."—"The red oxide is readily dissolved by . . . . , hyposulphite of soda, while the alkali is set at liberty in a caustic state," &c. &c.

The very remarkable facts above described I have reason to believe attracted a good deal of attention at the time, and thenceforward the ready solubility of silver salts, usually regarded as insoluble, by the hyposulphites was familiar to every chemist. It would not therefore be surprising if Daguerre tried it to *fix* his plates (*i.e.* to wash off the iodide coating); but I have been informed, though I cannot cite a printed authority for it, that at first he fixed with ammonia, or with a strong solution of common salt.

For my own part the use of the hyposulphites was to myself the readiest and most obvious means of procedure, and presented itself at once. My earliest experiments were made in January, 1839, and in my notebook I find:—

"**Exp. 1012.—1839, Jan. 29. Experiments tried within the** last few days, since hearing of Daguerre's *secret*, and also that Fox Talbot has got something of the same kind." . . . . . . [Here follow some trials of the relative sensitiveness of the nitrate, carbonate, acetate, and muriate of silver. I should observe that at that time I did not even know what kind of pictures Daguerre had produced. This process was not revealed till August, 1839.]

"Exp. 1013.—Daguerre's process—attempt to imitate. Requisites—1st very susceptible paper—2nd very perfect camera—3rd means of arresting further action. Tried hyposulphite of soda to arrest the action of light by washing away all the chloride of silver or other silvering salt—succeeds perfectly. Papers half acted on, half guarded from the light by covering with pasteboard, were withdrawn from sunshine, sponged over with hyposulphite, then washed in pure water, dried, and again exposed. The darkened half remained dark, the white half white, after any exposure, as if they had been painted with sepia."

"Jan. 30, 1839.—Formed image of telescope with the aplanatic lens . . . . and placed in focus paper with carbonate of silver. An image was formed in white on a sepia colored ground . . . . which bore washing with hyposulphite of soda, and was then no longer alterable by light. Thus Daguerre's problem is so far solved," &c. &c.

"Exp. 1014.—Jan. 30. Tried transfer of print and copper plate engraved letters," &c. &c.

The publication of Daguerre's process (according to Dr. Monckhoven, for I cannot refer at present to the original document) took place on the 19th August, 1839. My early experiments printed in the notices of the proceedings of the Royal Society of March 14, 1839, in which occurs this passage in the abstract of a paper read to the Society :—

"Confining his attention in the present notice to the employment of chloride of silver, the author enquires into the method by which the blackened traces can be preserved, which may be effected, he observes, by the application of any liquid capable of dissolving and washing off the unchanged chloride, but leaving the reduced oxide of silver untouched. These conditions are best fulfilled by the liquid hyposulphites."

"Twenty-three specimens of photographs made by Sir J. Herschel accompany his paper—one a sketch of his telescope at Slough, fixed from its image in a lens."

This is the image above mentioned as having been taken on Jan. 30, 1839—and was, I believe, the first picture ever fixed from an optical image ever taken in this country—at least I have heard of none earlier.

At the time of making these experiments, as already mentioned, I had no knowledge of M. Daguerre's process further than the mention of the existence of *a process* (a secret one) in a note from Admiral (then Captain) Beaufort some time about Jan. 23, 1839. Of course I used *paper*, not silver—and it was not a *suggestion*, but a regular and uniform *practice* to use the hyposulphite—I never used anything else.

I am, Sir, your obt. servt.,
J. F. W. HERSCHEL.

In reference to the subject of *fixing* the photographic image I find the following passage in a paper read before the Royal Society on January 31, 1839, by Mr. Talbot. After referring to the improvements of Wedgwood and Davy in 1802, and the difficulties they found in making the paper sufficiently sensible to receive the impression in a camera obscura, and their inability to *fix* the pictures, the author states that "his experiments were begun without his being aware of this prior attempt; and that in the course of them he discovered methods of overcoming the two difficulties above related. With respect to the latter he says that he has found it possible by a subsequent process so to fix the images or shadows formed by the solar rays that they become insensible to light . . . . . . and states that he has exposed some of his pictures to the sunshine for the space of an hour without injury."

In the abstract of the paper given in the Proceedings the method adopted for fixing the image is not stated; but in a paper read before the same Society on February 21 of the same year, it is stated that the prints were fixed in a weak solution of *iodide of potassium*. Ammonia had been tried, but not very successfully, but the method preferred was a strong solution of common salt. It will be seen, therefore, that up to the date of the publication of Sir John Herschel's paper hyposulphite of soda had not been used in photography excepting by himself.

## PHYSICAL AND MATHEMATICAL SECTION.

April 26th, 1866.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

A paper entitled "Results of a Comparison of the Magnitudes of the Bedford Catalogue with those of the Mensuræ Micrometricæ and the Bonner Sternverzeichniss," by George Knott, Esq., F.R.A.S., was communicated by Mr. Baxendell.

The question of the relation between the magnitude scales of different observers is one of considerable interest and importance, and has engaged the attention of several able astronomers. It is not my intention in this paper to go into the subject generally, but simply to submit the results of a comparison of the magnitudes of the late Admiral Smyth's *Bedford Catalogue* with those assigned to the same stars by the late Prof. Struve in the *Mensuræ Micrometricæ,* and by Dr. Argelander in the *Bonner Sternverzeichniss.* I may remark that I was induced to undertake the comparison by the perusal of an interesting paper by Mr. Pogson, the present Director of the Royal Observatory at Madras, in vol. xvii. of the Monthly Notices of the Royal Astronomical Society, in which he has given a short table of corresponding magnitudes of Struve, Bond, Herschel, and Smyth, as referred to what he terms the ***standard scale***, or Argelander's extended to the lower magnitudes by the adopted light-ratio of 2·512. The comparison was facilitated by the simplicity of the notation in the Bedford Catalogue (whole and half magnitudes only being employed), and in the annexed table will be found the corresponding magnitudes of the three observers, together with the number of comparisons on which each result depends.

TABLE I.

| Mag. Sm. | Mag. Σ. | No. of Obs. | Mag. A. | No. of Obs. |
|---|---|---|---|---|
| 1 | 1·3 | 5 | 1·3 | 7 |
| 1·5 | 1·5 | 1 | 1·4 | 3 |
| 2 | 2·0 | 3 | 2·1 | 13 |
| 2·5 | 2·6 | 2 | 2·3 | 12 |
| 3 | 2·9 | 12 | 3·0 | 36 |
| 3·5 | 3·3 | 14 | 3·4 | 22 |
| 4 | 3·8 | 27 | 3·9 | 28 |
| 4·5 | 4·3 | 19 | 4·4 | 16 |
| 5 | 4·8 | 40 | 4·9 | 35 |
| 5·5 | 5·1 | 41 | 5·3 | 33 |
| 6 | 5·8 | 87 | 5·9 | 59 |
| 6·5 | 6·2 | 45 | 6·3 | 27 |
| 7 | 6·5 | 81 | 6·9 | 37 |
| 7·5 | 6·9 | 79 | 7·3 | 23 |
| 8 | 7·4 | 101 | 8·1 | 47 |
| 8·5 | 7·8 | 85 | 8·5 | 20 |
| 9 | 8·4 | 75 | 9·0 | 34 |
| 9·5 | 8·8 | 21 | 9·2 | 6 |
| 10 | 9·4 | 25 | 9·4 | 6 |
| 10·5 | 9·7 | 5 | | |
| 11 | 10·0 | 24 | | |
| 12 | 10·2 | 13 | | |
| 13 | 10·7 | 20 | | |
| 14 | 10·3 | 6 | | |
| 15 | 11·0 | 8 | | |
| 16 | 10·9 | 3 | | |

Treating the numbers in the preceding table in accordance with Sir John Herschel's formula (Mem. R.A.S., vol. iii., p. 179),

$$M = \left\{ \frac{m_1 n_1}{S_1} + \frac{m\,n}{S} + \frac{m^1 n^1}{S^1} \right\} \cdot \frac{S}{n_1 + n + n^1}$$

where $S_1$ $S$ $S^1$ represent any three consecutive numbers in column 1, $m_1$ $m$ $m^1$ the corresponding numbers in columns 2 or 4, and $n_1$ $n$ $n^1$ the corresponding number of observations in each case, we obtain the following table of equalised results.

TABLE II.

| Mag. Sm. | Mag. Σ. | No. of Obs. | Mag. A. | No. of Obs. |
|---|---|---|---|---|
| 1 | 1·30 | 5 | 1·30 | 7 |
| 1·5 | 1·76 | 9 | 1·67 | 23 |
| 2 | 2·03 | 6 | 1·96 | 28 |
| 2·5 | 2·45 | 17 | 2·49 | 61 |
| 3 | 2·89 | 28 | 2·94 | 70 |
| 3·5 | 3·33 | 53 | 3·44 | 86 |
| 4 | 3·79 | 60 | 3·98 | 66 |
| 4·5 | 4·29 | 86 | 4·40 | 79 |
| 5 | 4·73 | 100 | 4·87 | 84 |
| 5·5 | 5·25 | 168 | 5·38 | 127 |
| 6 | 5·72 | 173 | 5·85 | 119 |
| 6·5 | 6·27 | 213 | 6·37 | 123 |
| 7 | 6·52 | 205 | 6·84 | 87 |
| 7·5 | 6·94 | 261 | 7·48 | 107 |
| 8 | 7·39 | 265 | 8·00 | 90 |
| 8·5 | 7·86 | 261 | 8·55 | 101 |
| 9 | 8·33 | 181 | 8·97 | 60 |
| 9·5 | 8·86 | 121 | 9·39 | 46 |
| 10 | 9·33 | 51 | 9·40 | 6 |
| 10·5 | 9·74 | 54 | | |
| 11 | 10·00 | 42 | | |
| 12 | 10·39 | 57 | | |
| 13 | 10·66 | 39 | | |
| 14 | 10·85 | 34 | | |
| 15 | 10·88 | 17 | | |
| 16 | 10·90 | 3 | | |

An examination of the tables suggests the following remarks. The scale of the *Bedford Catalogue*—(it should be borne in mind that the mag. of the principal star in each case was adopted from Piazzi)—and that of the *Bonner Sternverzeichniss* may be regarded as practically identical down to the 9th magnitude.* Assuming with Mr. Pogson that the 13th mag. of Argelander's scale corresponds with the 16th mag. of that of Admiral Smyth, we may tabulate the mags. below the 9th thus:—

| Mag. Sm. | Mag. A. |
|---|---|
| 10 | 9·6 |
| 11 | 10·1 |
| 12 | 10·7 |
| 13 | 11·3 |
| 14 | 11·9 |
| 15 | 12·4 |
| 16 | 13·0 |

I have not been able fully to verify these results; but the examination of eight stars marked by Sm. as of the 13th mag. on three different occasions gave 11·2 mag. as the corresponding mean mag. on Argelander's scale, presenting a satisfactory accordance with the tabulated magnitude.

The coincidence between the scale of the *Bedford Catalogue* and that of the *Mensuræ Micrometricæ* ceases practically with the naked eye magnitudes. At the 6th mag. the scales diverge, and the 11th mag. of the one corresponds with the 10th mag. of the other. It will be observed that the five magnitudes of Admiral Smyth's scale below the 11th are represented by only one magnitude in the scale of Professor Struve.

In conclusion I may just notice that by a slight oversight Mr. Pogson has, in the table to which I have already referred, assumed the 12th mag. of Struve's scale as the limit of vision for the Poulkowa refractor of 14·93 inches aperture; that mag. was however assigned by Prof. Struve as the limit for the Dorpat telescope of 9·6 inches aperture, the calculated limit for the larger telescope, as given in the Poulkowa Catalogue of 1843, being 12·89 mag.

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* Mr. Pogson has assumed the 8th mag. as the point of divergence of the two scales.

Mr. Baxendell drew attention to Sir John Herschel's "scarlet, almost blood-coloured star," immediately following *α* Crateris, and stated that, from observations made during the spring of last year, and again during the present month, he had found that this remarkable object belongs to the list of variable stars. Its present brightness is fully three-fourths of a magnitude less than that observed last spring; but this probably does not represent the extreme range of variation to which it is subject. In conformity with Prof. Argelander's system of nomenclature this new variable will be designated R Crateris. Its mean place for 1865·0 is R.A. $10^{h.}$ $53^{m.}$ $55{\cdot}4^{s.}$; Dec. 17° 36′ 3″ S.

## PHOTOGRAPHICAL SECTION.

### Annual Meeting, May 15th, 1866.

J. P. JOULE, LL.D., F.R.S., &c., in the Chair.

A note, by Mr. J. W. DANCER, on the use of Lager Beer in Photography, was read by the Secretary.

The Annual Report and Treasurer's Account were submitted to the meeting and unanimously passed; and the following Officers were elected for the ensuing year:—

**President.**
THE RIGHT REVEREND THE LORD BISHOP OF MANCHESTER.

**Vice-Presidents.**
J. P. JOULE, LL.D., F.R.S., &c.
H. E. ROSCOE, PH.D., F.R.S., &c.
JOSEPH BAXENDELL, F.R.A.S.

**Secretary.**
ALFRED BROTHERS, F.R.A.S.

**Treasurer.**
THOMAS H. NEVILL.

**Of the Council.**
JOHN B. DANCER, F.R.A.S.
E. C. BUXTON.
JOHN ROGERSON.
JOSEPH SIDEBOTHAM.
SAMUEL COTTAM.
LESLIE J. MONTEFIORE.

## MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

### Annual Meeting, June 4th, 1866.

A. G. LATHAM, Esq., President of the Section, in the Chair.

The Annual Report of the Council was read and unanimously adopted.

The following Gentlemen were elected Officers of the Section for the ensuing year:—

**President.**
ARTHUR G. LATHAM.

**Vice-Presidents.**
JOSEPH SIDEBOTHAM.
JOHN B. DANCER, F.R.A.S.
WILLIAM HENRY HEYS.

**Treasurer.**
THOMAS H. NEVILL.

**Secretaries.**
HENRY ALEXANDER HURST.
THOMAS ALCOCK, M.D.

**Of the Council.**
JOSEPH BAXENDELL, F.R.A.S.
PROF. W. C. WILLIAMSON, F.R.S., &c.
JOHN WATSON.
THOMAS COWARD.
R. D. DARBISHIRE, F.G.S.
ROBERT WORTHINGTON, F.R.A.S.
JOHN LEIGH.
SAMUEL COTTAM.

The PRESIDENT exhibited two beetles, male and female, nearly allied to Rhynchophorus Palmarum; also specimens of the very curious case formed by the larva from vegetable fibres, in which the change to the perfect insect takes place. He said that "both the larva and the insect itself are considered great delicacies by the Indians of South America, where it is found. The gizzard of the beetle, shown with the microscope and illustrated by a drawing, is divided into eight oval sections each surrounded by a strong rib strengthened by a central longitudinal rib, which is connected with the margin by powerful muscles, and from the marginal rib projects a fringe of scimitar-shaped cilia, the whole forming a most beautiful and powerful apparatus for the preparation of the food for digestion. A large number of parasites exist on the insect, and these are found congregated in groups of four or five with their tails entwined." Specimens and a drawing were shown.

# PROCEEDINGS

OF THE

# LITERARY AND PHILOSOPHICAL SOCIETY

OF

# MANCHESTER.

VOL. VI.

Session 1866–7.

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Sm

1867.

## NOTE.

THE object which the Society have in view in publishing their Proceedings, is to give an immediate and succinct account of the scientific and other business transacted at their meetings, to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

# INDEX.

Alcock Thomas, M.D.—On Echinus lividus, p. 25. On Polymorphina tubulosa, p. 85. On the Hair of the Musk-deer, p. 188.

Babington T. H.—On the Origination of the System of Issuing Storm Warnings, p. 108.

Bailey Charles.—Notes on Varieties of Sarothamnus Scoparius, Koch., and Stachys Betonica, Benth., from the Lizard, Cornwall, p. 48.

Ballot Buys, Dr.—On Storm Warnings, p. 83.

Baxendell J., F.R.A.S., Hon. Sec.—Observation of the Eclipse of the Sun, Oct. 8, 1866, p. 13. Observations of the Meteoric Shower of Nov. 13–14, 1866, p. 31. Observations of the New Variable Star, T Coronæ, p. 35. On the Recent Suspension, by the Board of Trade, of Cautionary Storm Warnings, p. 41. Observation of the Occultation of Aldebaran, Jan. 16., 1867, p. 98. Elements of the Variable Star R Persci, p. 99. On Dr. Buys Ballot's Weather Signal, p. 111. On Storm Warnings, pp. 112, 178. Observations of the Eclipse of the Sun, March 6, 1867, p. 125.

Binney E. W., F.R.S., F.G.S., V.P.—On Some Specimens from a Bore Hole at Birkdale, p. 17. On two Fossil Specimens of a species of Calamites from the Carboniferous Strata of Great Britain, p. 46. On two Remarkable Fossil Insects from the Lower Coal Measures near Huddersfield, p. 59. On a Section of the Drift and the Underlying Triassic and Coal Measures at Ardwick, p. 119.

Brothers A., F.R.A.S. — Catalogue of Binary Stars, with Introductory Remarks, p. 11. Observations of the Occultations of Aldebaran, Nov. 22, 1866, p. 67; and Jan. 16, 1867, p. 99. Observations of the Eclipse of the Sun, March 6, 1867, p. 126. Of Photographs of the Eclipse of the Sun, March 6, 1867, p. 141. Note on Photography in 1787, p. 163.

Carrington Dr. B.—On two Hepaticæ New to Britain, p. 186.

Coote J.—Description of a Frame for Printing Photographic Transparencies, p. 57. On "Braun's" Photographic Copies of Drawings by the Old Masters, p. 77.

Coward Thomas.—On Calluna vulgaris, from Newfoundland, p. 181.

Crompton Samuel, M.D.—On the Portraits of Sir Isaac Newton, p. 1.

Dancer J. B., F.R.A.S.—Remarks on "Braun's" Copies of Drawings of the Old Masters, p. 77. On the Reversed Action of Light in Photography, p. 117. Observations of the Eclipse of the Sun, March 6, 1867, p. 127. On the Microscopical Examination of Coal Ash or Dust from the Flue of a Furnace, p. 143.

Dickinson W. L.—On the Occultation of Aldebaran by the Moon, Jan. 16, and On the Eclipse of the Sun, March 5, 1867, pp. 47, 107.

Dyer J. C., V.P.—Notes on the Smallest and Simplest Forms of Matter and its Relations to Mechanical Forces, p. 152.

Gatty J. A.—Process for Printing Photographs in Various Colours, p. 7.

Hart Peter.—On a piece of Lead Pipe Coated Internally with Oxide of Manganese, p. 55.

Heelis Thomas, F.R.A.S.—On Cautionary Storm Signals, p. 45. On some Recent Observations on the Specific Gravity of Sea Water, p. 55. On a Hydraulic Driving Clock, p. 67.

Herschel Sir J. F. W., Bart., M.A., D.C.L., F.R.S., &c.—On Barometric Waves, p. 91.

Hind J. R., F.R.A.S., Superintendent of the Nautical Almanac.—Variable Star Ephemeris for 1867, p. 154.

Hull Edward, B.A., F.G.S.—On a Section of the Drift Deposits in the Banks of the Ribble, near Balderston Hall, p. 136.

Hurst H. A.—On the Plants springing up Spontaneously on the Fresh turning up of pasture land at Knutsford, Cheshire, p. 21. On the Structure of the Spines of Echini, p. 27.

Jack Prof. W., M.A.—On the Galvanometer, p. 147. Further Remarks on the Galvanometer, pp. 158, 177.

Johnson W. B.—On the Materials now Used in the Building of Iron Ships, p. 180

Joule J. P., LL.D., F.R.S., V.P.—On Dr. Buys Ballot's Weather Signal, p. 111. On an Apparatus for Determining the Horizontal Magnetic Intensity in Absolute Measure, p. 129. Note on the Tangent Galvanometer, p. 135. Additional Note on the Galvanometer, p. 151. Observations on the Alteration of the Freezing Point in Thermometers, p. 161.

**Knott George**, F.R.A.S.—Results of a Comparison of the Magnitudes of the Bedford Catalogue with those of Sir John Herschel, p. 13. Note on the Combined Magnitude of two Stars in Close Proximity, p. 15. On the Light-curve of R Vulpeculæ and its Maxima and Minima for 1866, p. 101.

**Latham Arthur G.**—On Foraminifera from Dogs Bay, Roundstone, and from Berwick Bay, pp. 85, 191.

**Linton James.**—On a Sample of Sand from Dogs Bay, Connemara, skimmed from the Surface of the Sea, by Mr. Glover in July, 1866, p. 184.

**Lynde James G.**, F.G.S., F.R.M.S.—Some Further Observations on the Cause of Rotation in the Cells of Vallisneria, p. 192.

**Mackereth T.**, F.R.A.S., F.M.S.—Results of Rain Gauge, Anemometer, and Ozone Observations, made at Eccles, near Manchester, during the year 1866, p. 94. Observations of the Eclipse of the Sun, March 6, 1867, p. 126.

**Nasmyth James**, C.E.—On Casting, Grinding, and Polishing Specula for Reflecting Telescopes, Part II., p. 82; Part III., p. 113; Part IV., p. 162.

**Ransome A.**, M.B., M.A. Cantab.—On some of the Conditions of Molecular Action, p. 121.

**Roscoe Prof. H. E.**, F.R.S., Hon. Sec.—On the Isomorphism of Thallium-perchlorate with Potassium and Ammonium-perchlorates, p. 9.

**Schönfeld Dr. E.**—Results of Observations of Variable Stars, made at the Mannheim Observatory, during the year 1866, p. 72.

**Sidebotham J.**—Notes on Wood-Feeding Coleoptera, p. 52. On Mr. Nasmyth's Suggestion of the Use of Chilled Cast Iron for Projectiles, p. 81. On the Reversed Action of Light in Photography, p. 114. On Coal Ash from Furnace Flues, p. 183.

**Smith R. Angus**, F.R.S., V.P.—On the Absorption of Gases by Charcoal, p. 195.

**Spence Peter**, F.S.A., F.C.S.—On the Manufacture of Sulphide of Ammonium, p. 18.

**Thomson Sir William**, M.A., D.C.L., F.R.S.—On a New Form of the Dynamic Method for Measuring the Magnetic Dip, p. 157.

**Thorpe T. E.**—On the amount of Carbonic Acid contained in Sea Air, p. 59. On the amount of Carbonic Acid contained in the Atmosphere of Tropical Brazils during the Rainy Season, p. 65.

VERNON G. V., F.R.A.S., F.M.S.—On the Mean Weekly Temperature at Old Trafford, Manchester, for the 17 years 1850–66, p. 68. On the Rainfall for 1866, at Old Trafford, Manchester, p. 70.

VIZE Rev. J. E.—Description of the Hairs of a Foreign Lepidopterous Caterpillar, p. 181. On Testacella, p. 192.

WARDLEY GEORGE.—On Streaks on Collodion Plates, p. 141.

WATSON J.—Further Remarks on the Plumules or Battledore Scales of some of the Lepidoptera, with Illustrations by Mr. Sidebotham, p. 183.

WILDE H.—On Sieman's and Wheatstone's Magneto-electric Machines, p. 103.

*Meetings of the Physical and Mathematical Section.*—Ordinary, pp. 13, 67, 68, 94.

*Meetings of the Microscopical and Natural History Sections.*—Annual, p. 191. Ordinary, pp. 21, 85, 181, 182, 183, 192.

*Meetings of the Photographical Section.*—Ordinary, pp. 7, 24, 57, 77, 114, 141, 163.

*Report of the Council.*—April 30, 1867, p. 165.

## ERRATA.

Page 134, line 22, for $\theta$ and $\theta_1$ in the equation, read *tan* $\theta$ and *tan* $\theta_1$.

Page 136, line 9, for *generally* read *supposed to be.*

# PROCEEDINGS
OF
# THE LITERARY AND PHILOSOPHICAL SOCIETY.

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## Ordinary Meeting, October 2nd, 1866.

**EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.**

Among the donations announced was one from Mr. Cottam, of a framed Memorial, presented by the Society to Dr. Dalton in 1844, on the occasion of his communicating to them the 50th annual result of his meteorological observations. In a letter accompanying the memorial, dated the 5th September, 1866, Mr. Cottam says: "At the Doctor's death it remained in the hands of our old friend, Peter Clare, at whose sale it was purchased by my late father, and has since hung up in my office as a fine specimen of caligraphy. I think the Society's rooms will be the most suitable place for it, and that the centenary anniversary of the Doctor's birth is a suitable occasion for its presentation."

On the motion of Mr. BINNEY, seconded by Mr. GASKELL, the thanks of the Society were unanimously voted to Mr. Cottam for his interesting donation.

A paper was read "On the Portraits of Sir Isaac Newton; and particularly on one of him by Kneller, painted about the time of the publication of the *Principia*, and representing him as he was in the prime of life," by SAMUEL CROMPTON, M.D.

The writer pointed out that all Newton's great discoveries were made prior to 1688, and directed attention to the following dates: That he was born in 1642, died in 1727, and that he discovered Fluxions in 1665-6, the Unequal Refrangibility of Light in 1666, the Reflecting Telescope in 1668, and published the *Principia* in 1687; and that with that publication, which Whewell has designated "The Great Charter of Modern Science," his career, as an original discoverer, appears to have terminated. He quoted passages from Mr. Edleston's Correspondence of Newton and Cotes, and from Sir David Brewster's larger Life of Newton, to show that his appointment to the Mint, in a great measure, stopped further researches. "Henceforward," says Mr. Edleston, "his official duties rendered it impossible for him to work continuously at his former pursuits; and his studies in mathematics and natural philosophy were by snatches, and in the intervals of business." "His duties at the Mint," says Sir David, "allowed him but little time for the performance of so laborious a task as the publication of a second edition of the *Principia*, and when his consent was at last obtained to put the work to the press, they greatly interrupted its progress?" Again, in one of his letters to Flamstead, he says that he did not like to be known to be attending to anything but the King's business, meaning the duties of the Mint. Therefore, after the publication of the *Principia*, he may be practically regarded as a retired philosopher.

The writer then attempted to show that the current portraits of Newton do not give us any correct representation of Newton, the philosopher. He exhibited various engravings of Newton. He pointed out that Vertue's print, after Vanderbank, is inscribed "Isaacus Newtonus Eq. Aur. æt. 83. J. Vanderbank pinxit 1725, Geo. Vertue sculpsit 1726;" and, therefore, that the date of that celebrated picture, in possession of the Royal Society, represents him in extreme old age. This picture, therefore, he contended, was not a portrait of the great Newton, but of Newton the Master of the Mint, who assayed Wood's halfpence; and that it could not, for one moment, be accepted as a representation of

Newton in the plenitude of his intellectual power, of whom Halley sang in 1687:—

Newtonum clausi reserantem scrinia veri,
Newtonum Musis charum, cui pectore puro
Phœbus adest, totoque incessit numine mentem,
Nec fas est propius mortali attingere divos.

Nor was it the portrait of him as he was when he published the second edition of his *Principia*, but a representation of him as a grand and venerable ruin covered with the lichens of time.

But this picture of Vanderbank's, the writer greatly preferred to the Houbraken and other prints published about 1710 or 1712; and he contended that the portrait published, as the best extant one, by Sir David Brewster, as a frontispiece to his larger Life of Newton, by no means gives a desirable representation of Newton the philosopher. It was rather an affected representation of Newton the dandy, and of Newton the prosperous man of the world, with a carriage and horses, and with three male and three female servants. The writer looked upon these prints with pity; and could not, for one moment, allow that any one of them represented that Isaac Newton, the yeoman's son, while at work in the wells of truth, and wresting from nature secrets hidden from the foundation of the world. In order to form some idea of the immortal Newton, as distinguished from Queen Anne's Newton, he asked us to consider attentively the letter written by Newton's former assistant, Humphrey Newton, to Mr. Conduit, in 1727, descriptive of Isaac's manner of life and appearance between 1684 and the beginning of 1689.

"In the last year of King Charles II., Sir Isaac was pleased, through the mediation of Mr. Walker, then schoolmaster at Grantham, to send for me up to Cambridge, of whom I had the opportunity, as well as honour, to wait of for about five years. In such time he wrote his *Principia Mathematica*, which stupendous work, by his order, I copied out before it went to the press. . . . . . His carriage, then, was very meek, sedate, and humble, never seemingly angry, of profound thought, his countenance mild, pleasant and comely. I cannot say I ever saw him laugh but once which was at that passage which Dr. Stukeley mentioned in his letter to your honour*, which put me in mind of the Ephesian philosopher who laughed only once in his life time, to see an ass eating thistles when plenty of grass was by. He always kept close to his studies, very rarely went a visiting, and had as few visitors. . . . . . I never knew him to take any recreation or pastime either in riding out to take the air, walking, bowling, or any other exercise whatever, thinking all hours lost that was not spent in his studies, to which he kept so close that he seldom left his chamber except at term time, when he read in the schools as being Lucasian professor, where so few went to hear him, and fewer that understood him, that oft times he did in a manner, for want of hearers, read to the walls. . . . . . So intent, so serious upon his studies, that he ate very sparingly, nay, ofttimes he has forgot to eat at all, so that, going into his chamber, I have found his mess untouched of which, when I have reminded him, he would reply,—"Have I?" and then making to the table, would eat a bit or two standing for I cannot say I ever saw him sit at table by himself . . . . He very rarely went to bed before two or three of the clock, sometimes not till five or six, lying about four or five hours especially at spring and fall of the leaf, at which times he used to employ about six weeks in his laboratory, the fire scarcely going out either night or day, he sitting up one night and I another till he had finished his chemical experiments, in the performance of which he was most accurate, strict, exact. What his aim might be, I was not able to penetrate into, but his pains, his diligence at these set times made me think he aimed at something beyond the reach of human art and industry. I cannot say I ever saw him drink either wine, ale, or beer, excepting at meals, and then but very sparingly. He very rarely went to dine in the hall, except on some public days, and then if he was not minded, would go very carelessly, with shoes down at heels, stockings untied, surplice on, and his head scarcely combed. . . . . . . When he has sometimes taken a turn or two (in his garden) he has made a sudden stand, turned himself about, run up y$^e$ stairs like another Archimedes, fall to write on his desk standing without giving himself the leisure to draw a chair to sit down upon. . . . . . His brick furnaces, pro re nata, he made and altered himself without troubling a bricklayer. . . . . . . He very seldom sat by the fire. . . . . . . He never slept in the day time that I perceived: I believe he grudged y$^e$ short time he spent in eating and sleeping. . . . . His bed maker in a morning has sometimes found both dinner and supper scarcely tasted, which the old woman has very pleasantly and mumpingly gone away with. . . . . . . . His thoughts were his books; though he had a large study seldom consulted with them. When he was about 30 years years of age his grey hairs was very comely, and his smiling countenance made him so much the more graceful."

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* When Sir Isaac once laughed 'twas upon occasion of asking a friend to whom he had lent Euclid to read, what progress he had made in that author, and how he liked him? He answered by desiring to know what use and benefit in life that study would be to him, upon which Sir Isaac was very merry.

Surely, Dr. Crompton contended, none of the engraved portraits of Newton represents the great Newton of those years. The conceited, double-chinned Kneller's of 1710-20, give no idea of him. Dr. C. then exhibited an admirable engraving by Mr. Oldham Barlow,* of the recently recovered portrait of Newton, painted by Kneller in 1689, and asked whether it did not fulfil every required condition? Eyes capable of penetrating into the heart of things—a brow that could measure the universe—a face "full of Godlike reason" and in its intensity and force indicating a very beagle of truth—yet what simplicity! and how contemplative! The identical white locks spoken of by Humphrey Newton, to be in a few years shorn to make way for the wig of the Houbraken print!

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* Of Victoria Road, Kensington, London.

Dr. C. then gave the history of the portrait from whence this engraving has been taken, and said he identified it at the Art Treasures' Exhibition, as Newton as he was when he wrote the *Principia*; and pointed out at that time that it was the only known portrait of Newton worthy of notice—that he obtained, in conjunction with Dr. Joule, Mr. Fairbairn, Dr. Schunck, Mr. Binney, and Dr. Angus Smith, leave from its noble owner to have it photographed; and he exhibited a print then taken—that Mr. Barlow happening to be at his house, saw an impression of it, and expressed a wish to engrave it—that Dr. Crompton obtained Lord Portsmouth's permission for the use of the original picture. Dr. C. then read letters from Lord Portsmouth, proving that the portrait came into his lordship's possession by the marriage of his ancester Lord Lymington, to the grand-niece of Newton, Miss Conduit, and that it had been in a neglected condition and recently cleaned and lined. Dr. C. added that Sir David Brewster told him that he went to Hurstbourne Park to examine Newton's MSS. and portraits, but that he did not see it, yet it was there. The picture, Dr. C. added, is in Kneller's best manner, is painted with great care, is signed in the left corner "Isaacus Newtonus," and in the right, "Godfrey Kneller fec. 1689." Dr. C. exhibited an engraving prefixed to Dr. Edleston's Correspondence of Newton and Cotes, which he said was from an India ink drawing in Magdalen College, Cambridge, undoubtedly closely allied to the Kneller picture, but greatly inferior; being, in Dr. Crompton's opinion, a bad copy by an inferior hand, but, no doubt, made for Mr. Pepys, in whose collection it was.

Dr. C. showed that Newton was in London during the greater part of 1689, as Member of the Convention Parliament, when it is certain that this portrait was painted; and he added, that he had not been able to discover any other portrait of him painted earlier than, or about, this interesting period of his life, nor any other, till several years later (probably twenty), when success had greatly changed his countenance.

## PHOTOGRAPHICAL SECTION.

October 4th, 1866.

Dr. J. P. Joule, F.R.S., &c., Vice-President of the Section, in the Chair.

A paper was read "On a Process for Printing Photographs in Various Colours," by J. A. Gatty, Esq.

My process is based upon the property possessed by ferricyanide of potassium, of forming clear solutions with certain metallic salts, producing insoluble compounds when the mixture is brought into contact with a deoxidising agent, the rays of the sun acting as such, a perfect precipitation takes place upon paper or other material prepared with the above-named solution. In producing the specimens sent herewith, I applied to the paper a concentrated solution, formed of equal parts of ferricyanide of potassium and nitrate of lead, having found the latter to answer very well, not only as a means of forming a precipitate, but also for assisting in the production of numerous colours. After drying the paper, it was exposed to the sun for about half an hour, and then washed in water in order to dissolve all the unaffected ferricyanide of potassium and nitrate of lead. I have noticed that the sun acts much quicker when there is a little moisture present. I have, therefore, placed a damp cloth between two or three thicknesses of paper behind the prepared paper.

After washing, the Photographic image remains behind as a pale greenish precipitate, easily transformed into various colours, as the following experiments will show:—

No. 1. (Blue.) Has been steeped in a weak solution of nitrate of iron for about ten minutes, and then washed in water.

No. 2. (Green.) Same as No. 1, but steeped in a weak solution of bi-chromate of potash after the nitrate of iron.

No. 3. (Reddish Brown.) Has been steeped in a solution of nitrate of copper, and then washed.

No. 4. (Brown.) Has been developed by steeping it in a mixture of weak solution of nitrate of iron and nitrate of copper.

No. 5. (Dark Brown.) Has also been treated with nitrate of iron and nitrate of copper, but containing a larger proportion of the former.

These few experiments will show that a very large number of shades may be obtained by using different salts and mixtures thereof in developing the Photograph. A further series of colours may be obtained by destroying the blue with caustic soda, which, after washing, will leave behind oxides of iron and lead, which may be dyed with vegetable colouring matters.

All the above experiments have been made about four years ago, which goes to prove that the colours are permanent. I hope shortly to be able to resume my experiments, and work the process out more perfectly.

Mr. DANCER remarked that he had some prints (which were shown to the meeting) produced by Mr. Mercer in 1856, which appeared to be done by a process similar to the one just described, prussiate of potash being one of the substances used, but he was not acquainted with the details of the process.

It was considered a useful process for certain purposes, and deserved more attention than it had hitherto received.

## Ordinary Meeting, October 16th, 1866.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

"On the Isomorphism of Thallium-perchlorate with the Potassium and Ammonium-perchlorates," by Prof. H. E. Roscoe, F.R.S., &c.

The recently ascertained isomorphism of the Thallium- and Ammonium-sulphates* renders it highly probable that the perchlorates of these metals also exhibit isomorphous relations.

Thallium-perchlorate equals the Alkaline-perchlorates in stability. It can be readily prepared either by dissolving metallic Thallium in aqueous Perchloric Acid, or by the double decomposition of Thallium-sulphate and Barium-perchlorate. From solution the anhydrous Salt is easily deposited in colourless rhombic crystals, which are transparent, bright, well-defined, and non-deliquescent. The specific gravity of the crystals is 4·844 at 15°·5 C.; they dissolve in 10 times their weight of water at 15°, and in about $\frac{6}{5}$ths of their weight of water at 100°; the salt is also slightly soluble in alcohol. Thallium-perchlorate does not lose weight when heated to 200° C., and the temperature may even be raised to within a few degrees of the boiling point of mercury without producing the slightest decomposition of the salt. On the further application of heat, a black mass is formed, and the salt finally volatilizes as Thallium-chloride.

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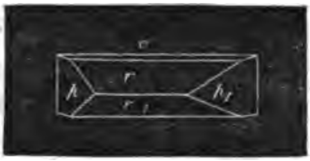
* Lang, Phil. Mag., xxv. 248.

The crystalline form of Thallium-perchlorate is that of a right rhombic prism ($pp_1$), in which the faces of the rhombic octohedron ($rr_1$) and the basal faces of the prism ($c$) generally appear; the crystals being lengthened, as is the case with the Alkaline-perchlorates sometimes in the direction of the prismatic, and sometimes in the direction of the octohedral faces. A careful measurement of the crystals gave the mean value of 102° 50′ for the angle $pp_1$; and that of 102° 6′ for the angle $rr_1$ on $c$. Hence the relation of the axes is 0·7978 : 1 : 0·6449.



The angles observed by Mitscherlich,* in the case of Potassium- and Ammonium-perchlorates agree exactly with the above; for the first of these salts $pp_1$=103° 58′, $rr_1$ on $c$= 101° 19′; and for the second $pp_1$=103° 11′, and $rr_1$ on $c$= 102° 4′, giving the relation of the axes to be (1) 0·7817 : 1 : 0·6408 and (2) 0·7926 : 1 : 0·6410. The following analysis, made in my laboratory by Mr. T. E. Thorpe, shows that the formula of the salt is $Tl\,Cl\,O_4$.

(1) Determination of Thallium. The crystals were well dried in vacuo over Sulphuric Acid, and precipitated with Platinum-tetra-chloride, the passage of the finely divided precipitate through the filter being avoided by evaporating to

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* Pogg. Ann. xxv. (1832), p. 301.

dryness on the water bath, and taking up with absolute alcohol.

(a) Salt prepared by direct solution of the metal in Perchloric Acid; 0·1831 salt yielded 0·2476 Chloroplatinate.

(b) Salt prepared by double decomposition; 0·4502 salt yielded 0·6060 Chloroplatinate.

(2) Determination of Perchloric Acid. A solution of Potassium-acetate was added to the Thallium-perchlorate, and the whole evaporated to dryness on a water-bath, the acetates of Thallium and Potassium washed out with absolute alcohol, and the insoluble Potassium-perchlorate collected on a weighed filter; 0·4570 Thallium-perchlorate yielded 0·2100 Potassium-perchlorate. Hence:

| | Calculated. | Found. | | |
|---|---|---|---|---|
| | | I. | II. | III. |
| Tl......204·0 | 67·21 | 67·38 (a) | 67·18(b) | |
| Cl...... 35·5 | 21.09 | | | 32·96 |
| $O_4$...... 64·0 | 11·70 | | | |
| 303·5 | 100·00 | | | |

"Catalogue of Binary Stars, with introductory Remarks," by A. Brothers, F.R.A.S.

The Author said that the purpose for which he had prepared a list of the stars known to be Binary at the time of the publication of Smyth's "*Cycle*," was to bring down the information as to the positions and distances of those stars to the present time, and for this object he had obtained the necessary information from some of the most eminent observers. He had found much inconvenience from having to examine a number of catalogues for the information he required for his own observations, and in the list submitted he had brought into one view all that the amateur would require in prosecuting his researches on the Binary Stars.

Mr. **Brothers** said that he thought it unadvisable to include in the present list any other objects than those which had been observed by Smyth, and that he intended to prepare another list, which would include most of the stars which had shown a sufficiently large change of angle of position since 1842, to entitle them to a place with the well known Binary systems.

The Author's Catalogue will shortly be published by the Society.

## PHYSICAL AND MATHEMATICAL SECTION.

October 11th, 1866.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

"Observation of the Eclipse of the Sun, October 8th, 1866, at Mr. Worthington's Observatory, Crumpsall," by JOSEPH BAXENDELL, F.R.A.S.

The first contact was observed at 4h. 20m. 6·0s. Greenwich mean time. Owing to the low altitude of the Sun, its limb was very tremulous, but the error arising from this cause was estimated not to exceed one second. The telescope used was the equatorially-mounted achromatic, of 5 inches aperture and 70 inches focal length, with a positive eyepiece magnifying 68 times. The position of the observatory is, latitude 53° 30′ 50·0″ N., longitude 8′ 56·16″ W.

"Results of a Comparison of the Magnitudes of the Bedford Catalogue with those of Sir John Herschel," by GEORGE KNOTT, F.R.A.S. Communicated by Mr. Baxendell.

Some few months ago I had the honour of submitting to the Physical and Mathematical Section of the Manchester Literary and Philosophical Society the results of a comparison of the Star Magnitudes of the *Bedford Catalogue* with those of the *Mensuræ Micrometricæ* and the *Bonner Sternverzeichniss*. In the following paper, which may be regarded as supplemental to that just referred to, I propose giving the results of a similar comparison of the magnitude scale of the *Bedford Catalogue* with that of Sir John Herschel.

The Stars for comparison are taken from Sir John Herschel's six Catalogues of Stars observed with the 20 foot reflector, and his two Catalogues of Double Stars measured with the 7 foot equatorial, which are printed in the earlier volumes of the Memoirs of the Royal Astronomical Society. The method of procedure was the same as that adopted in the former investigation. The magnitude assigned by Sir John Herschel to each star common to the two observers was tabulated under the magnitude assigned to it by Admiral Smyth, and the mean taken. The results thus obtained were equalized by Sir John Herschel's formula (Mem. R.A.S., vol. iii, p. 179) and the corrected and uncorrected results, together with the number of comparisons on which each depends, are given in the following Table.

TABLE.

| Mag. Sm. | Mag. H. | No. of Obs. | Mag. H. Corrected | No. of Obs. |
|---|---|---|---|---|
| 1 | 1·05 | 6 | 1·05 | 6 |
| 1·5 | | | 1·62 | 8 |
| 2 | 2·20 | 5 | 2·25 | 2 |
| 2·5 | 2·7 | 3 | 2·59 | 15 |
| 3 | 3·0 | 10 | 3·04 | 23 |
| 3·5 | 3·6 | 10 | 3·67 | 45 |
| 4 | 4·3 | 25 | 4·28 | 53 |
| 4·5 | 4·9 | 18 | 4·87 | 75 |
| 5 | 5·4 | 32 | 5·28 | 88 |
| 5·5 | 5·6 | 38 | 5·85 | 135 |
| 6 | 6·5 | 65 | 6·40 | 132 |
| 6·5 | 7·0 | 29 | 6·98 | 135 |
| 7 | 7·4 | 41 | 7·36 | 112 |
| 7·5 | 7·7 | 42 | 7·76 | 136 |
| 8 | 8·1 | 53 | 8·18 | 125 |
| 8·5 | 8·8 | 30 | 8·76 | 120 |
| 9 | 9·5 | 37 | 9·47 | 85 |
| 9·5 | 10·2 | 18 | 10·05 | 79 |
| 10 | 10·5 | 24 | 10·44 | 60 |
| 11 | 11·1 | 18 | 11·25 | 56 |
| 12 | 11·9 | 14 | 11·72 | 58 |
| 13 | 12·3 | 26 | 12·48 | 55 |
| 14 | 13·2 | 15 | 13·27 | 53 |
| 15 | 14·4 | 12 | 14·46 | 40 |
| 16 | 15·9 | 13 | 15·90 | 13 |

If we combine these results with those contained in my former paper, we obtain an indirect comparison of Sir John Herschel's scale with that of Professor Struve. The results thus obtained present but a moderate accordance with those of Sir John Herschel's direct comparison, as given in Vol. iii. of the Memoirs of the Royal Astronomical Society. This is perhaps owing, apart from the indirectness of the method, to the fact that I have not confined myself to the list of stars employed by Sir J. Herschel, and that my comparison was made with the magnitudes of the *Mensuræ Micrometricæ*, and not with those of the *Dorpat Catalogue.*

"Note on the Combined Magnitude of two Stars in close proximity," by GEORGE KNOTT, F.R.A.S. Communicated by Mr. BAXENDELL.

It is sometimes convenient to know the magnitude resulting from the combined effect of two stars in such close proximity as to appear *as one star*, either to the naked eye, or in a telescope of insufficient power to separate them.

If $m$ and $m+\Delta m$ be the magnitudes of two stars so circumstanced, and $\rho$ the *decreasing* 'light-ratio,' then if $\mu$ be the combined magnitude required, it may easily be shown that*:—

$$\mu = m + \frac{\log.\,(1+\rho^{\Delta m})}{\log.\,\rho}$$

It thus appears that if we tabulate the values of the function $\frac{\log.\,(1+\rho^{\Delta m})}{\log.\,\rho}$, (the sign of which it will be observed is essentially negative,) for certain definite values of $\Delta m$, we can obtain, by inspection, the value of the correction to be applied to the numerical magnitude of the greater of two stars closely juxtaposed, in order to obtain the magnitude resulting from their combination.

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* I have adopted the symbols of the formula as kindly communicated to me by Lieut. Tupman, R.M.A., F.R.A.S., as being more symmetrical than those of my own independent investigation.

The following Table has been calculated on the assumption that $\rho = 0{\cdot}398 = \frac{1}{2{\cdot}512}$, the reciprocal of Mr. Pogson's value for the 'light-ratio.'

TABLE.

| $\Delta m.$ | $\frac{\log.(1+\rho^{\Delta m.})}{\log.\ \rho.}$ |
|---|---|
| 0·0 | —0·75 |
| 0·5 | 0·53 |
| 1·0 | 0·36 |
| 1·5 | 0·24 |
| 2·0 | 0·16 |
| 2·5 | 0·10 |
| 3·0 | 0·07 |
| 3·5 | 0·04 |
| 4·0 | —0·03 |

"Catalogue of Binary Stars, with Introductory Remarks," by A. BROTHERS, F.R.A.S.

[This paper was afterwards read at the Ordinary Meeting of the Society, held October 16th, 1866. See page 11.]

### *Erratum.*

Proceedings Lit. & Phil. Society, Vol. 5, p. 127, lines 2 and 3 from bottom, for July 17·0 and December 2·6, read July 9·5 and November 24·1.

## Ordinary Meeting, October 30th, 1866.

R. Angus Smith, Ph.D., F.R.S., &c., Vice-President, in the Chair.

Professor R. B. Clifton, M.A., F.R.A.S., late of Owens College, but now of Oxford, was unanimously elected an Honorary Member of the Society.

Mr. E. W. Binney, F.R.S., F.G.S., exhibited some specimens which Mr. Samuel Ogden had procured from a bore hole now being made near the new hotel in Birkdale Park, Southport, for the purpose of obtaining a supply of water. At 65 yards deep from the surface beds of red and light coloured marls, with crystals of sulphate of lime and white granular gypsum, were met with. At 67 yards similar beds, with greater quantities of sulphate of lime and gypsum in them. At 69 yards like beds, without sulphate of lime and gypsum. All the deposits contained common salt, which was very evident on the application of the tongue. He said that red and variegated marls containing gypsum similar to those exhibited were met with in the upper permian beds of Lancashire and the keuper marls of the trias in Cheshire, but in the latter alone had salt been yet found, and he was therefore inclined to consider the specimens triassic, and the strata not very likely to yield an abundant supply of fresh water. The thickness of these marls had never been thoroughly proved, but at Fleetwood they had been penetrated 409 feet, at Poulton Breck 537 feet, without reaching the underlying marls and waterstones. At Middlesborough in the east of England, and Duncrue near Belfast, these beds were proved to be about 1200 feet in thickness. Rock salt and not fresh water has generally been found in them. If the sandstone beds of the waterstones or the soft red sandstone of the bunter, both of them water bearing strata, were reached, he doubted about the water being fresh if found at all.

Mr. W. W. PLATT, of the firm of Mather and Platt, said he was of opinion that the occurrence of salt in the marls was no reason why there should not be fresh water below in beds of sandstone, and if he were the parties engaged in the undertaking he should not hesitate in proceeding further, and he thought with a fair chance of success, for a good supply of fresh-water.

"On the Manufacture of Sulphide of Ammonium," by PETER SPENCE, F.S.A., F.C.S.

Hydro-sulphuret of ammonia, or sulphide of ammonium, is at present chiefly used in the laboratory, where it is a very useful reagent in metallic and other analyses, and it has often been thought it might be more extensively used if it could be made cheaply and of good quality. At present it is made by directly charging liquor ammonia with sulphuretted hydrogen gas, but this is a very expensive mode, and, from the fact that most of it thus obtained is not fully charged, it would seem not to be an easy mode of making it. Having occasion some time ago to require it largely in the manufacture of sulpho-cyanide of ammonium, I invented for myself a mode of making it very economically and fully charged with sulphur.

In communicating this plan to the Society I do it in the hope that it may be useful in the laboratories of those who produce some of their own reagents. The Society must not, however, give me too much credit for generosity, for, had I found a market for the article making it worth my while to make it largely, I should have held it secured by patent, but as it is not, I make the gift for what it is worth. I have by this plan made the article to some extent for a London house, and have sent them some 30 to 40 carboys; but the demand was only fitful, and although they were highly satisfied with the quality and would have given me my own price, it was not worth my while to continue the process.

The mode of manufacture is merely to mix a salt of ammonia, say sulphate or chloride, with double its weight of soda waste or gas lime (either being suitable), and blowing into the mixture a jet of steam and passing the vapours through condensing apparatus; the distillate is pure sulphide of ammonium. It may be as well to mix the two bodies in a portion of water; and even in that case it is needful to be careful with the condensing pipes, as sulphide of ammonium being much more volatile than water, it comes over so strong at first as to condense in a solid form. I had nearly a serious accident with it, which I name to induce caution. I was passing the still when at work, and saw something was wrong. I called my manager, who, with the man at work, examined the pipes, and found them closed; on opening them with an iron rod there was a flow of liquor and a strong blow of gas; seeing and feeling this, I called to the man to leave it instantly, but he hesitated. My manager then seized him by the arm and pulled him away, and he instantly fell down and became rigid as a log of wood; his breast was now rubbed forcibly to bring on breathing, which had quite stopped, and a doctor who was sent for poured cold water on his head. He now went into most violent convulsions, which lasted over an hour and a half, when he got gradually round and was able to work next day. I ascribed the saving of his life to the rubbing of his breast, as complete asphyxia had taken place with the rigidity of death. Sulphuretted hydrogen is most powerful in its effects on animal life, but when combined with ammonia it seems even more active;

one peculiarity of it in both forms, however, seems to be that if you escape with life it leaves no bad effects afterwards. I have myself oftener than once suffered from it, but only temporarily.

With this caution to those who attempt making sulphide ammonium, and stating that otherwise there is no difficulty in the process, I conclude this communication.

## MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

October 8th, 1866.

A. G. Latham, Esq., President of the Section, in the Chair.

Mr. Hurst read a paper "On the plants springing up spontaneously on the fresh turning up of pasture land at Knutsford, Cheshire."

He said that grass seeds were sown over a portion, but that this merely accelerated what appeared to be the certain effect of time in this district, viz., the almost total extinction of other plants by the grasses. His observations only extended through July and August, during which time he observed the following species:—

*Ranunculaceæ.*

Ranunculus bulbosus
 auricomus

*Papaveraceæ.*

Papaver dubium, Lamottii
 argemone

*Fumariaceæ.*

Fumaria confusa, E. B. capreolata
 officinalis

*Cruciferæ.*

Capsella Bursa pastoris
Brassica Rapa, E. B.
Arabis Thaliana

*Violaceæ.*

Viola arvensis
 eu-tricolor

*Caryophillaceæ.*

Silene arvensis, E. B.
Spergula arvensis, E. B. var. sativa
Stellaria media

*Geraniaceæ.*

Geranium pusillum

*Leguminosæ.*

Trifolium repens
 pratense
 procumbens
 minus
Vicia hirsuta

*Rosaceæ.*

Rubus
Cratægus

*Rubiaceæ.*

Sherardia Arvensis

*Compositæ.*
Achillea millefolium
Senecio vulgaris
Sonchus oleraceus
Crepis virens
Leontodon autumnalis, var. eu genuinis
Taraxacum officinalis

*Borragineæ.*
Myosotis arvensis

*Primulaceæ.*
Anagallis arvensis

*Scrophularineæ.*
Digitalis purpurea
Veronica serpyllifolia

*Labiatæ.*
Prunella vulgaris
Galeopsis versicolor
Lamium purpureum

*Plantagineæ.*
Plantago lanceolata
major

*Chenopodiaceæ.*
Chenopodium album

*Polygonaceæ.*
Rumex acetosa
acetosella
crispus
Polygonum convolvulus
aviculare
persicaria
lapathifolium

*Euphorbiaceæ.*
Euphorbia helioscopia

*Amentaceæ.*
Quercus pedunculata

*Juncaceæ.*
Juncus bufonius

*Gramineæ.*
Anthoxanthemum odoratum
Holcus mollis
lanatus
Lolium perenne
Bromus arvensis
Poa trivialis
procumbens
Dactylus glomeratus
Avena sativa

With regard to the foregoing list, he drew attention to the total absence of Umbelliferæ. Also the Polygonum Convolvulus was the plant which at one time covered more ground than any other species, appearing to smother most of the plants around it. Yet in untouched adjacent fields not an individual could be found. Classed by De Candolle as one of those plants cultivated by man against his will in western Europe, and only indigenous in Siberia, it certainly seemed curious that a Siberian plant should be the first to cover the soil of Cheshire on its being first exposed to the air. Mr. Hurst remarked that Polygonum aviculare came after the former species in the extent of ground covered, and on flat unshaded places appeared to hold its own against P. convolvulus better than any other plant; nor does it appear to be eventually smothered by the grasses among which it may frequently be found growing. Papaver dubium came next as to conspicuousness, though perhaps not covering so great an area as other species humbler in appearance. In the adjacent town of Bowdon, under similar circumstances, it sprang up abundantly the first year, sparingly the second, and almost disappeared the third. The slightest cakeing or hardening of the surface soil being fatal to its existence.

Plantago major, or "the footsteps of the white man," as it is called by the North American Indians, also appeared, but in small numbers. The list comprises 58 species, of which the Graminaceæ form 17 per cent; Polygonaceæ, 13 per cent; Compositæ, 11 per cent; Leguminosæ, 9 per cent.

Hence there were one-half of the species comprised in four families. Papaver argemone, Myosotis arvensis, Anagallis arvensis were single specimens, but these species are found in the adjacent fields.

## PHOTOGRAPHICAL SECTION.

### November 1st, 1866.

Dr. J. P. JOULE, F.R.S., &c., Vice-President of the Section, in the Chair.

Mr. COOTE exhibited an interesting set of prints, including views of Exeter Cathedral, Torquay, &c., which he had recently taken by the collodio-albumen process. One of the plates used had been prepared twelve months, and the print from it was equal to most of the others taken on freshly prepared plates.

Mr. BROTHERS suggested that if the meteoric shower on the night of the 12th–13th instant should prove to be one of more than ordinary interest, it might be interesting to preserve a photographic record of the event. He said that a camera with a prepared collodion plate might be adjusted on some well known star near where the meteors were noticed to be most numerous and brightest, and after about ten or fifteen minutes the plate (it might be kept wet about that time) should be developed if any meteors had been observed to pass. A succession of plates could be exposed, and a note taken of the principal star to which the camera had been directed.

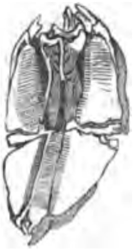
## Ordinary Meeting, November 13th, 1866.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Arthur McDougall, William Stanley Jevons, M.A., Professor of Logic, &c., Owens College; and William Jack, M.A., Professor of Natural Philosophy, Owens College, were elected Ordinary Members of the Society.

"On Echinus lividus, illustrated by specimens from Roundstone," by Thomas Alcock, M.D.

The Author described particularly the mechanism of the teeth and jaws of the animal, and showed by a dissection of the parts that the statement made both by Professor Owen and Professor Rymer Jones that the striated surfaces of the jaws are used to comminute the food is incorrect, for the whole of these surfaces is occupied by muscle, and is altogether outside the pharynx through which the food passes. These particulars may be seen in the accompanying figure taken from the dissection.



He further showed that the food contained in the alimentary canal consists of very coarse pieces of sea-weed and zoophyte, which have evidently not been subjected to the action of any triturating apparatus.

He exhibited mounted specimens of the suckers, and also of the sucker-plates cleaned in potash. He said Professor Owen quotes Professor Valentin with regard to the pedicellariæ, and states that there are three forms of them belonging to Echinus lividus, namely, gemmiform, tridactyle, and ophicephalous pedicellariæ;—these were exhibited as mounted specimens, and with them a fourth kind, quite distinct from them, and the most remarkable in form; it has long slender jaws like those of a crocodile, armed, in this species, with one very long terminal tooth and one tooth on each side not far removed from it. He remarked that in Echinus sphæra all four kinds of pedicellariæ are found and agree in their general character with those of Echinus lividus, though they are sufficiently different to be readily distinguished, and the fourth kind just mentioned has, besides the long terminal tooth, a series on each side, of six or seven recurved teeth, suggesting that the name sauricephalous would be an appropriate one for this form. Mounted specimens of the four kinds of pedicellariæ of Echinus sphæra were shown for comparison with those of Echinus lividus, together with suckers and sucker-plates, and the buccal membrane mounted entire to show the ophicephalous and gemmiform pedicellariæ complete and in their natural position.

"On the Structure of the spines of Echini," by H. A. Hurst, Esq.

Notwithstanding the general appreciation by microscopists of the spines of Echini, the author had been unable to find any satisfactory account of their structure, and he attributed this to the fact that the examination of these objects had been chiefly confined to their sections, mounted in Canada balsam, which frequently has the effect of making transparent objects too transparent. He recommended for this purpose, however, the use of Smith and Beck's semi-paraboloid lieberkhun, together with transmitted light—cutting off either means of illumination by a slight motion of the hand, or using both together; but the readiest means of ascertaining their real structure he found to be the examination of unmounted and splintered ends of broken spines by incident light, a method bringing out details and showing the connection of parts in a manner superior to any other. It was with diffidence he dissented from Dr. Carpenter's views in the last edition of his work on the microscope; but he begged to propose the following as more in accordance with the appearances of structure presented by these spines under the microscope. They are composed of two substances in outward appearance, though chemically perhaps the same, one so perfectly homogeneous and transparent when viewed by transmitted or polarised light that it cannot be distinguished from the blank field of the microscope—yet, under incident light, so dark and opaque as to appear black. This substance is frequently traversed by winding anastomising channels, which, though only containing air, seem opaque and show as solid by transmitted light, the substance they traverse itself being invisible. He had not satisfactorily made out the structure of the second substance; it resembled the pith of plants, but it was less regularly cellular, and in some spines assumed a fibrous appearance. It is opaque under transmitted, and glistening white under incident light. In the following remarks he called this opaque, and the first described transparent substance. The general structure of the spines he had examined was also twofold; in the simpler, as the Amphidotus cordatus, the centre portion is hollow; in the more complicated it is composed of the opaque substance perforated along the length of the spine by vertical solid tubes of the transparent matter, without any definite arrangement. These appear to increase only in length; hence a section at the apex of the spine shows in the centre a prolongation of the oldest portion, the thickening of the spine arising differently, as subsequently explained. The hollow centre of the Amphidotus cordatus is surrounded by a cellular fretwork of the transparent matter, while around this is a circle of solid ribs or pillars of the same, smooth on the exterior of the spine, but within beautifully hollowed out into what the heralds call an engrailed outline, the points of which connect it with the inner layer of cellular fretwork. This framework is occasionally wanting, and the engrailed points are simply connected with each other by a straight inner line of transparent matter. In the more complicated forms he was not satisfied he had ascertained the real structure, but thought it to be as follows.—The tubes of transparent matter noticed about the central opaque substance, as they approached towards the circumference of the first season's growth, gradually coalesce, and, at a certain distance from the centre, consolidate into a rib or pillar, which runs from the root to the apex of the spine, forming a longitudinally furrowed exterior, caused by the centre of each rib projecting slightly. Once this consolidation of tubes into ribs effected, the growth changes, the ribs extending eccentrically into plates radiating from the centre, and separated from each other by a mass of tissue similar to that at the centre. At the close of the second period of growth these plates thicken concentrically so as almost to touch each other, which however they do not, leaving a furrow of separation. They then continue their radiating extension till the end of another year's growth, when the concentric thickening into an outer rib again takes place, and so on in successive years. These radiating plates are horizontally perforated by circular apertures bearing a singular resemblance to rivet holes in boiler plates, and appear to have rivets passing through them of the opaque substance in a fibrous state. Altogether the structure of the spine may be compared to that of an iron tubular bridge. The thickened exteriors of these plates are highly coloured, and traversed by winding, anastomising channels containing air; in some portions of the spine, the opaque substance either grows into, or is destroyed and replaced by, the transparent substance, which then forms a solid mass, perforated by the rivet holes, now changed into winding, anastomising passages.

Forms intermediate between the extremes of complexity and simplicity were those of Echinus sphæra and lividus.

Echinus sphæra being chiefly composed of the pith-like substance, with 25 or 30 radiating glassy plates of a whitish colour; while

Echinus lividus was more solid, the pith-like substance passing into the solid glassy radiating plates through portions consisting of this glassy matter, perforated by anastomising channels.

Mr. Hurst was not able to say whether the pithy and glassy substances are distinct or not; but while the cellular matter leaves no trace after the prolonged action of vinegar, the transparent glassy substance, as well as the exterior of the spine, appears to be enveloped by a membrane, resisting the action of vinegar, which curiously converts this solid, opaque, hard, and brittle spine into a transparent, flexible body, retaining its original form.

By using direct sunlight and a semi-paraboloid condenser, the glassy matter could be distinctly seen through, even when viewed as an opaque object, and the arrangement of the cellular matter ascertained. It is this transparent substance which is tinged with the beautiful purple hue so well known to microscopists.

Mr. Hurst expressed his disappointment that the use of polarized light in these observations had led to no result, and thought its value had been over-estimated.

## Ordinary Meeting, November 27th, 1866.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

Among the donations announced was a beautiful series of preparations of the spores of Fungi on glass, from Sir J. F. W. Herschel, Bart., M.A., F.R.S., &c. &c.

On the motion of Mr. SPENCE, seconded by Dr. JOULE, the thanks of the Society were unanimously voted to Sir John Herschel for his valuable donation.

"Observations of the Meteoric Shower of November 13-14, 1866," by JOSEPH BAXENDELL, F.R.A.S.

The early part of the night of November 13th was very squally and cloudy, with showers of rain and hail and occasional flashes of lightning. At about 12h. 15m. a break occurred near the zenith, and in a few minutes the clouds had almost entirely disappeared. My observations of the meteors commenced at 12h. 16m. Greenwich mean time, and were directed principally to the determination of the time of maximum frequency, and the position of the radiant point. The observations of frequency were as follows:—

| | | | | | | Number of Meteors Observed. |
|---|---|---|---|---|---|---|
| From | $12^h$ | $16^m$ | to | $12^h$ | $32^m$ | 60 |
| | | 32 | | | 48 | 153 |
| | | 48 | | 13 | 4 | 287 |
| | 13 | 4 | | | 20 | 378 |
| | | 20 | | | 26 | 122 |
| | | 26 | | | 42 | 316 |
| | 14 | 19 | | 14 | 42 | 54 |
| | 15 | 20 | | 15 | 35 | 6 |

From 13h. 42m. to 14h. 19m., and again from 14h. 42m. to 15h. 20m., the observations were interrupted by clouds and rain, and only 73 meteors were counted during the two

intervals. At 15h. 35m. clouds came on again very suddenly, and the sky remained obscured at 16h. 5m., when I ceased to watch.

During the whole time of observation the sky was rarely entirely free from clouds for more than two or three minutes, but the errors arising from this cause are probably pretty evenly distributed through the intervals above given, and cannot therefore materially affect the final determination of the time of maximum frequency. The results of the observations are as follows: —

| | Average Number of Meteors per Minute. |
|---|---|
| At $12^h$ $24^m$ | 3·7 |
| 40 | 9·5 |
| 56 | 17·9 |
| 13 12 | 23·6 |
| 23 | 20·3 |
| 34 | 19·7 |
| 14 30½ | 2·3 |
| 15 27½ | 0·4 |

The curve formed by a projection of these numbers gives 13h. 12m. as the time of maximum frequency. The probable error of this result can hardly exceed one minute.

In order to determine the position of the radiant point, the positions of the intersecting points of the paths, continued backwards, of a great number of pairs of meteors were noted. By far the greater number of these points fell on a space bounded by lines joining the stars $\gamma$, $\zeta$, $\mu$, $\varepsilon$, and $\eta$ Leonis, and, allowing equal weights to all the observations, the mean position was found to be R. A. 9h. 58m. 12s. $=149°\ 33'$; Dec. 22° 57·5′ North. Calculating the position referred to the ecliptic, we have Long.$=143°\ 41·0'$; Lat.$=$ 9° 54·5′ North.

At the time of maximum frequency the earth was advancing in the direction of a point on the ecliptic the longitude of which was 141° 28·3′, or 2° 12·7′ less than that of the radiant. It appears, therefore, that the meteors were crossing the earth's orbit from within outwards, and that their aphelion distance is very sensibly greater than the earth's radius vector on the 13th of November.

The velocity of the earth in its orbit on the 13th of November is 18·38 miles per second, and the velocity of the November meteors when they enter the earth's atmosphere has been found to be 40 miles per second. With these data and the latitude of the radiant point as given above, 9° 54·5′ N., we find that the inclination of the orbit of the mass of meteors to the plane of the ecliptic is 17° 59′, and that their orbital velocity at the time they encounter the earth is 22·31 miles per second. The excess of this velocity over that due to their distance from the sun arises, in part at least, from the accelerating effect of the earth's attraction.

An attempt was made to estimate roughly the relative numbers of meteors of different magnitudes, and it was found that they occurred in about the following proportions:—

Out of every 100 meteors, 10 were above the 1st mag.; the brightest of these were 2 to 3 times brighter than Sirius;
15 were between the 1st and 2nd mag.
25 „ 2nd and 3rd mag.
30 „ 3rd and 4th mag.
15 „ 4th and 5th mag.
5 were below the 5th mag.

The average magnitude was 3·0.

The trains left by many of the larger meteors had a beautiful emerald green colour, others were of an ashy grey, and the remainder white. The meteors themselves were mostly white or bluish white, but many were of a fine golden colour.

In order to give some idea of the great velocity with which the meteors enter the earth's atmosphere it may be remarked that it would be sufficient to carry a body through the entire circuit of the earth in an interval of less than ten and a half minutes.

As I had the good fortune to witness the great meteoric shower which occurred on the morning of the 13th Nov., 1833, I may state that the late display was far inferior to it both in the number of meteors seen and in the brilliancy of the larger ones, and I am therefore inclined to think that a much finer display may be expected to occur in November next. At the time of the 1833 great shower I was at sea, off the west coast of central America, and although I then knew little about meteors, and the idea of a radiant point had not, so far as I am aware, ever occurred to any astronomer or meteorologist, the tendency of the great majority of the meteors to diverge from a particular region of the heavens was so strongly marked that it at once engaged my attention, and I find on referring to my notes that I fixed the central point of this region in the constellation Cancer, a few degrees east of the stars $\delta$ and $\gamma$, and not in Leo, as observed by Professor Olmstead and others, in the north-western portion of the North American continent. A great number of the meteors, however, had other radiant points, and some of the finest moved in long horizontal arcs, or in directions nearly perpendicular to that of the main stream. This fact seems to me to be strongly opposed to the cosmical theory of meteorites, except on the rather improbable supposition that the earth, on that occasion, encountered two or more groups, all, at the same time, crossing each others orbits, as well as the orbit of the earth. It may, however, be urged that such a supposition is hardly more unlikely than that which ascribes the November meteors to a ring of small bodies moving round the sun in an orbit differing little in magnitude from the earth's orbit, but the motion being retrograde, or contrary to that of the earth, and therefore inconsistent with the general analogies of the solar system, and opposed to Laplace's almost universally received nebular hypothesis.

"Observations of the new variable star, T Coronæ," by JOSEPH BAXENDELL, F.R.A.S.

While engaged on the night of the 15th of May last in observing some of the naked eye variables, my attention was suddenly arrested by a strange and rather conspicuous star about a degree distant from ε Coronæ in the south following quadrant. It was at once carefully compared with some of the neighbouring stars in order to ascertain its magnitude exactly, and a rough determination of its position was made, which led me to conclude that it was identical with Argelander's No. 2,765 of Zone + 26°, 9·5 magnitude, of the *Bonner Sternverzeichniss*, a conclusion which was fully confirmed the following night by more exact observations. On the night of the 7th of May I observed all the naked eye variables then visible, and also several telescopic ones, and among the latter R and S Coronæ, but this star, if at that time really visible, entirely escaped my notice; and as the nights between the 7th and 15th were cloudy at Manchester, I am unable to say at what date it attained its maximum brilliancy. My observations of its magnitude, colour, &c., from the date of discovery to the present time are as follows, and I may add that all determinations of magnitude after the star became invisible to the naked eye were, with only three exceptions, made at Mr. Worthington's observatory with his equatorially mounted achromatic of five inches aperture, care being taken to use always the same eyepiece, a positive, having a magnifying power of 68 times.

OBSERVATIONS OF T CORONÆ.

| Date. | Mag | Colour, &c.. |
|---|---|---|
| 1866. | | |
| May 15 | 3·7 | White, with a very slight yellow tinge, whiter than ε. |
| 16 | 4·2 | Cream coloured, but the light very bright, and star well defined, without any hazy appearance. |
| 17 | 4·9 | With naked eye. Cream-coloured, exactly similar to ε. |
| | 5·1 | With 5in. Ach. p. 68. |

| Date. | Mag | Colour, &c. |
|---|---|---|
| 1866. | | |
| May 18 | 5·3 | Cream-coloured or buff. At times I have an impression of a blue tinge, as if the yellow of the star were seen through a film of a blue tint. |
| 19 | 5·6 | Deep cream, buff, Bath brick or wash-leather colour, with a tinge of blue over it. ε the same colour, but perhaps slightly lighter and without the blue, or at least with excessively little of it. Repeatedly examined also by Mr. Dancer and Mr. Williamson, with different powers, and their estimations of colour precisely the same as my own. |
| 20 | 6·2 | Buff coloured, with a tinge of blue, and deeper than ε Coronæ, which is yellow or light buff. |
| 21 | 7·1 | Leaden or slaty blue; the yellow colour has almost entirely disappeared. |
| 22 | 7·4 | The light of the star is dull, and is of a slaty blue or dark French white colour, or nearly like Smyth's No. 4 blue. No traces of yellow or red can be clearly made out. |
| 23 | 7·5 | Dull grey or French white. Sometimes there seems to be a trace of yellow. |
| 24 | 7·7 | Dull white, with a slight tinge of yellow or orange. |
| 25 | 7·8 | Dull and slightly orange-white. A shade of blue sometimes suspected. |
| 26 | 8·0 | Dull orange white. |
| 29 | 8·4 | Dull orange yellow. |
| June 8 | 9·3 | Dull orange yellow. |
| 10 | 9·2 | Dull orange yellow. |
| 16 | 9·4 | |
| 17 | 9·5 | |
| 19 | 9·5 | |
| 25 | 9·6 | Orange yellow. |
| 26 | 9·7 | Orange. |
| July 11 | 9·7 | Dull yellow. |
| 16 | 9·7 | |
| 19 | 9·6 | |
| 20 | 9·7 | |
| 21 | 9·6 | |
| 22 | 9·5 | Dull pale orange. |
| 30 | 9·7 | Orange yellow. |
| Aug. 20 | 9·8 | |
| 27 | 9·5 | |
| 31 | 9·3 | Dull yellow. |
| Sept. 14 | 7·9 | Dull yellow; almost exactly Smyth's yellow No. 3. |
| 15 | 7·8 | Yellow. |
| 17 | 7·9 | Pretty bright yellow. |
| 22 | 7·9 | |
| 24 | 7·8 | Yellow. |
| 30 | 7·8 | |
| Oct. 1 | 7·7 | |
| 6 | 7·6 | Greyish yellow. |
| 8 | 7·6 | |
| 10 | 7·5 | Yellow. |
| 14 | 7·5 | Light yellow. |
| 19 | 7·7 | Light yellow. |
| 28 | 7·8 | Yellow. |
| Nov. 6 | 7·9 | Smyth's orange No. 4. |
| 19 | 8·3 | Dull ruddy orange. |

It will be seen that the brightness of the star diminished with great rapidity for several days after my first observation, and afterwards more gradually, and that on the 26th of June it had sunk to the 9·7 magnitude. It then remained with little change till about the 20th of August, when another rise commenced, and on the 15th of September it had attained the 7·8 magnitude. On the 10th and 14th of October it was of the 7·5 magnitude, and since the latter date its brightness has again slightly diminished.

It will also be noticed that in the recent observations no mention is made of the blue tinge which formed so striking a feature in the colour of the star for some time after its first appearance. In connection with this the following extract of a letter from Mr. Huggins, F.R.S., dated October 13th, will be interesting to the members of the Society. Referring to T Coronæ the writer says, "I observed its spectrum on September 16th, 27th, 28th, and October 8th. The bright lines are not now to be distinguished. If they exist they cannot be much, if any, brighter than the parts of the spectrum where they occur. The observation of its spectrum is now very difficult."

In the scale of magnitudes I have employed, the light-ratio is 2·512 ; but the expression in magnitudes of the brightness of a variable star gives a very imperfect idea of the nature and extent of its changes, and as any speculations respecting the causes of variability must be based upon a consideration of the variations in the intensity of the light, and not in the magnitude of the star, I have calculated the relative intensities of the light of T Coronæ for every night of observation, taking the intensity of the light of a 10th magnitude star as unity. The results are given in the following table :

INTENSITIES OF THE LIGHT OF T CORONÆ:—

| Date. | Mag. | Intensity, 10 mag. being=1·0 | Date. | Mag. | Intensity. | Date. | Mag. | Intensity. |
|---|---|---|---|---|---|---|---|---|
| 1866. | | | | | | | | |
| May 15 | 3·7 | 331·2 | June 17 | 9·5 | 1·6 | Sep. 17 | 7·9 | 6·9 |
| 16 | 4·2 | 209·0 | 19 | 9·5 | 1·6 | 22 | 7·9 | 6·9 |
| 17 | 4·9 | 160·0 | 25 | 9·6 | 1·4 | 24 | 7·8 | 7·6 |
| 18 | 5·3 | 75·8 | 26 | 9·7 | 1·3 | 30 | 7·8 | 7·6 |
| 19 | 5·6 | 57·5 | July 11 | 9·7 | 1·3 | Oct. 1 | 7·7 | 8·3 |
| 20 | 6·2 | 33·1 | 16 | 9·7 | 1·3 | 6 | 7·6 | 9·1 |
| 21 | 7·1 | 14·4 | 19 | 9·6 | 1·4 | 8 | 7·6 | 9·1 |
| 22 | 7·4 | 11·0 | 20 | 9·7 | 1·3 | 10 | 7·5 | 10·0 |
| 23 | 7·5 | 10·0 | 21 | 9·6 | 1·4 | 14 | 7·5 | 10·0 |
| 24 | 7·7 | 8·3 | 22 | 9·5 | 1·6 | 19 | 7·7 | 8·3 |
| 25 | 7·8 | 7·6 | 30 | 9·7 | 1·3 | 28 | 7·8 | 7·6 |
| 26 | 8·0 | 6·3 | Aug. 20 | 9·8 | 1·2 | Nov. 6 | 7·9 | 6·9 |
| 29 | 8·4 | 4·4 | 27 | 9·5 | 1·6 | 19 | 8·3 | 4·8 |
| June 8 | 9·3 | 1·9 | 31 | 9·3 | 1·9 | | | |
| 10 | 9·2 | 2·1 | Sep. 14 | 7·9 | 6·9 | | | |
| 16 | 9·4 | 1·7 | 15 | 7·8 | 7·6 | | | |

The outburst of T Coronæ appears to have been first observed by Mr. J. Birmingham, of Tuam, on the night of May 12, when its brightness was equal to, if not greater than that of α Coronæ, the magnitude of which is 2·6. The relative intensity of the light of the variable on that night was therefore not less than 912·1. Comparing this with the intensities in the above table it will be seen that during the first ten days of the star's appearance its light diminished with extraordinary rapidity, the intensity on the 22nd May being only 11·0 against 912·1 on the 12th when first seen by Mr. Birmingham, and 331·2 on the 15th when first seen by myself. Since the 22nd of May its changes have been comparatively slight. On the 20th of August the intensity was at a minimum, being only 1·2, or $\frac{1}{760}$th of its amount on the 12th May.

An inspection of the curve of intensities suggests strongly the idea that a force of an explosive character, such as could result only from the action of highly elastic gaseous matter, had been in operation to produce the sudden increase and subsequent rapid diminution of brightness which had taken place; but as some of the well-known periodical variables occasionally exhibit an equal, and even a greater, rapidity of change, this view cannot at present be received with much confidence; and notwithstanding the remarkable and highly interesting conclusions which Mr. Huggins and Dr. W. A. Miller have drawn from the results of their spectroscopic observations of this new variable, we are constrained to admit that the cause of variability is still involved in the deepest mystery.

The following table contains the positions and magnitudes of the stars with which I have compared T Coronæ in the course of its changes. The positions are from the "Bonner Sternverzeichniss," and the magnitudes are from my own observations. The magnitudes of some of these stars have been determined independently by Mr. Knott, F.R.A.S., and it is satisfactory to me to find that our results are almost identical.

| No. | Star. | R. A. 1855·0. | | | Dec. N. 1855·0 | | Magnitude. |
|---|---|---|---|---|---|---|---|
| 1 | β Serpentis...... | 15ʰ | 39ᵐ | 29·3ˢ | 15° | 53·1′ | 3·7 |
| 2 | γ Herculis ...... | 16 | 15 | 31·1 | 19 | 30·3 | 3·8 |
| 3 | γ Coronæ ...... | 15 | 36 | 40·0 | 26 | 45·6 | 4·2 |
| 4 | ε Coronæ ...... | | 51 | 35·7 | 27 | 18·1 | 4·3 |
| 5 | δ Coronæ ...... | | 43 | 31·0 | 26 | 31·5 | 4·8 |
| 6 | π Serpentis..... | | 56 | 3·5 | 23 | 12·3 | 4·8 |
| 7 | ι Coronæ......... | | 55 | 38·2 | 30 | 15·1 | 5·1 |
| 8 | 25 Herculis...... | 16 | 20 | 14·8 | 37 | 42·7 | 6·0 |
| 9 | Arg. 2575 + 27° | 15 | 56 | 38·2 | 27 | 1·1 | 7·5 |
| 10 | 3009 25 | | 53 | 23·8 | 25 | 51·1 | 7·6 |
| 11 | 2767 26 | | 55 | 2·8 | 26 | 34·6 | 7·7 |
| 12 | 2762 26 | | 52 | 32·8 | | 57·2 | 7·9 |
| 13 | 2754 26 | | 49 | 16·8 | | 26·3 | 8·0 |
| 14 | 3003 25 | | 52 | 39·3 | 25 | 59·5 | 8·1 |
| 15 | 2563 27 | | 52 | 30·0 | 27 | 16·8 | 8·1 |
| 16 | 2769 26 | | 55 | 28·9 | 26 | 48·0 | 8·4 |
| 17 | 2763 26 | | 52 | 54·1 | | 33·6 | 9·0 |
| 18 | 2758 26 | | 51 | 49·5 | | 9·9 | 9·2 |
| 19 | 2760 26 | | 52 | 7·3 | | 40·0 | 9·4 |
| 20 | 2761 26 | | 52 | 25·4 | | 21·2 | 9·6 |
| 21 | 2764 26 | | 53 | 22·3 | | 33·9 | 10·8 |

I may state that two of these stars, Nos. 10 and 18, have shown decided indications of slight variability, the range of variation, so far as I have yet observed them, being about four-tenths of a magnitude.

## Ordinary Meeting, December 11th, 1866.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"On the Recent Suspension, by the Board of Trade, of Cautionary Storm Warnings," by JOSEPH BAXENDELL, F.R.A.S.

On the 29th ult. a circular was issued by the Board of Trade, announcing that the "cautionary storm warnings," which have from time to time been issued by the Meteorological Department of the Board, would be suspended from the 7th day of December instant. This step, it appears, has been taken on the recommendation of the President and Council of the Royal Society, who are of opinion that "at present these warnings are founded on rules mainly empirical, and therefore should not be issued under the superintendence of the scientific body to whom the discussion of meteorological observations will be committed." No attempt appears to have been made by the Board to ascertain the opinions and wishes of those classes of the community for whose benefit the system of issuing storm warnings was originated by the late Admiral Fitzroy. No complaints against the system, as carried out by the admiral, and since his death by Mr. Babington, have been made by those most deeply interested in its operation; nor has any evidence been produced to show that it has led to loss or inconvenience. On the contrary it is very generally admitted that it has been the means of saving many valuable lives, and also an amount of property greatly exceeding in value the whole cost of the department in which it had its origin. Whether the rules on which it is founded are empirical or not, the system, as a mere commercial speculation, has been successful, and it is therefore difficult to understand why the Board of Trade should have attached so much importance to the reasons advanced by the President and Council of the Royal Society, and allowed them to outweigh other far more important considerations. If the President and Council think that the superintendence of the duties connected with the system of issuing storm warnings would be derogatory to the scientific body to whom the discussion of meteorological observations will be committed, it does not follow that the system ought to be suspended for an indefinite length of time, but rather, that its management ought to be committed to some more suitably constituted body. If, however, it is intended to be understood that the system ought to be discontinued because the rules upon which storm warnings are founded are, at present, mainly empirical, then it may be replied that, in the early stages of every science, its rules are mainly empirical; but it has never before been urged that, therefore, we ought to deprive ourselves of such advantages as may be derived from the limited amount of knowledge we may happen to possess. The rules by which the laws of magnetism are applied to the correction of compasses in iron ships are to a great extent empirical, and, in spite of the greatest care in their application, serious errors are sometimes made; but are we on that account to decline to avail ourselves of such aid as they do afford, imperfect, and to some extent uncertain, though it may be? Long after the predicted places of the moon were given in the *Nautical Almanac*, the longitudes derived from lunar distances were often very considerably in error; but no scientific man, or body of men, at that time ventured to suggest that the publication of the *Nautical Almanac* ought to be suspended until astronomers had perfected the lunar theory. The laws relating to the strength of materials were long in a very unsatisfactory state; but scientific men did not urge that, therefore, we ought to suspend building and engineering operations until these laws had been established on a strictly scientific basis. It is needless, however, to multiply illustrations to show the unsoundness of the principle on which the recommendation is based; and it requires but a moderate acquaintance with scientific methods to see that in a science which aims at prediction, its progress will be best and most surely advanced by a continuous and systematic comparison of the phenomena as they actually occur with the phenomena as predicted. It is in this way that the methods of astronomy have attained their present high degree of perfection; and it will be obvious, on a little consideration, that, if Mr. Babington's predictions or forecasts of the weather are regulary and systematically compared with the phenomena which actually occur, many points of considerable interest will doubtless often be brought under immediate notice which might otherwise long escape detection, and in this way an impetus will be given to the progress of the science which could not be applied in any other way, or by any other means.

In the circular issued by the Board of Trade it is stated that the President and Council of the Royal Society think that "in a few years the rules on which storm warnings are founded may probably be much improved by deductions from the observations in land meteorology, which will by that time have been collected and studied." Now, this allusion to land meteorology refers, no doubt, to a recommendation made in a letter addressed by the President of the Royal Society to Mr. Farrer, one of the Secretaries of the Board of Trade, on the 15th June, 1865. Mr. Farrer had informed the President "that the Admiralty were willing to undertake, and to place in the hands of their Hydrographer, all those observations which can properly be made use of in framing charts for purposes of navigation, but not those which relate to meteorology proper;" and the President, in referring to this, interprets the term "meteorology proper" to mean merely "the land meteorology of the British Islands;" but how this singular interpretation was arrived at is not stated. He then goes on to urge the desirability of establishing "a few stations—say six—distributed at nearly equal distances, in a meridional direction, from the South of England to the North of Scotland, furnished with self-recording instruments, supplied from, and duly verified at, one of the stations, regarded as a central station." This central station, he afterwards suggests, should be the Observatory of the British Association, at Kew. Now, it has long been known to meteorologists that deductions from observations made within the narrow limits of the British Islands are quite insufficient to lead to the detection and development of the laws which regulate those great movements and phenomena of the atmosphere upon which the daily changes in the weather at any given station depend. The principal meteorological elements of the British Islands have already been determined with considerable accuracy, from observations made during long series of years, at various observatories and stations, both public and private; and any minute changes in their values that may be rendered necessary by future observations with improved instruments, are not at all likely to have any sensible influence on the empirical laws upon which storm warnings have been based. The truth is, this land meteorology scheme is altogether unnecessary for the purpose of correcting these empirical laws. It is not so much in the multiplication of observations over limited areas, as in the adoption of improved methods of combining and treating observations at stations scattered some distance apart over every accessible portion of the earth's surface, that we must look for new discoveries, and for the means of establishing the rules of meteorology on a strictly scientific basis.

The cost of carrying out the land meteorology scheme would be out of all proportion to the results ever likely to be obtained from it; and it would be unjust to the commercial community to withdraw the funds which have hitherto been so usefully applied in the interests of humanity and commerce, and devote them to the establishment, by an irresponsible body, of a scheme of such doubtful utility.

It will be evident from the above remarks that the recommendation of the President and Council of the Royal Society, from whatever point of view it may be regarded, is a retrograde movement, opposed to the true interests of the science of meteorology, and likely, if acted upon, to retard its progress. It is therefore to be hoped that the Board of Trade will reconsider their decision, and not, for light and insufficient reasons, discontinue a system which, on grounds of humanity and commercial economy, has met with the general approval of the country, and is, moreover, so likely to contribute materially to the advancement of a popular and highly-important branch of science.

Mr. THOS. HEELIS, F.R.A.S., urged that the abolition of the cautionary signals would tell hardly against the coasting craft and fishermen. To large well-found sea-going vessels, well provided with instruments, the signals were not of much use, but the case was very different with the masters and owners of the coasting vessels who were unable to afford instruments. He instanced the case of the colliers which supply the large steamers at Southampton with coal brought from the south Welsh ports. These vessels have to make all their passages from Southampton to the westward in ballast, and it is of the utmost importance to them not to be caught by a gale from the southward between Plymouth or Falmouth and the Land's End. He was not aware that any complaints as to the inefficiency of the storm warnings had been received from the masters of coasters, and considering the amount of capital and number of hands employed in the trade, and the fact that they had no powerful organ to advocate their views, he thought that their interests deserved consideration, and feared that they had been overlooked. The saving of life should, especially at present when so many complaints are current about the deficiency in the supply of seamen, be a great object, even if such saving were effected by empirical means. Mr. Heelis had no confidence in the prospect of improving the meteorology of the coast by observations inland. He rather advocated the improvement of the land meteorology by observations on the coast; and he had noticed a decided improvement of late in the storm warnings. Having been engaged in a yachting cruise on the Cornish coast during the months of August and September last, he had paid a good deal of attention to the signals, and had never seen them hoisted without the forecast being speedily verified.

MR. BINNEY, F.R.S., stated that Professor H. R. Goeppert, the renowned German botanist, had lately described a fossil plant under the name of *Aphyllostachys Jugleriana*, which he classed in the group of *Calamarien*, and stated, with some degree of doubt, that it probably belonged to the lias formation of Hanover. The specimen had been in his possession fifteen years, and he says, "In spite of the lively interest which it created on nearer examination, still its relation to other plants of the former and present world remained doubtful, so that I often laid it aside for time to give me further information. Since this hope has so far not been realized I do not fail in making it public at last, with the wish that others guided by deeper insight may succeed better than I have done in its explanation." The author alluded to the resemblance of the fossil to the fructification of certain coal measure plants, particularly to the *Volkmannia sessilis* of Presl. Mr. Binney said he had in his cabinet two specimens showing organs of fructification very similar to those described by Professor Goeppert, and evidently closely allied to them. These specimens were from the carboniferous strata of Great Britain, one from the mountain limestone of North Wales, and the other from the upper coal measures at Ardwick, near Manchester, so in all probability the learned Professor's specimen came from carboniferous and not liassic strata. The fossil is the fructification of some species of *calamites*, according to the opinions of the best fossil botanists of the day.

Mr. W. L. DICKINSON communicated a paper containing the results of calculations relative to the Occultation of the star Aldebaran by the Moon, January 16, and to the Eclipse of the Sun, March 5, 1867, visible here. The calculations have been made for the observatory of Robert Worthington, Esq., F.R.A.S., Crumpsall, near Manchester, Lat. 53° 30′ 50·0″ N., Long. $0^h\ 8^m\ 56{\cdot}16^s$ W. The elements used in the computations have been obtained from the *Nautical Almanac.*

The Occultation of the star $\alpha$ Tauri (Aldebaran) by the Moon, January 16, 1867:—

| | Sidereal Time at Observatory. | Mean Time at Observatory. | Mean Time at Greenwich. | Angle from | |
|---|---|---|---|---|---|
| | | | | N.Point. | Vertex. |
| Disappearance | $2^h\ 57^m\ 1^s$ | $7^h\ 13^m\ 58^s$ | $7^h\ 22^m\ 54^s$ | 100° | 81° |
| Reappearance. | $4^h\ 10^m\ 5^s$ | $8^h\ 26^m\ 50^s$ | $8^h\ 35^m\ 46^s$ | 282° | 280° |

The Angles are reckoned towards the right hand round the circumference of the Moon's image as seen in an inverting telescope.

The Annular Eclipse of the Sun, March 5, 1867, is visible as a partial one at the observatory, and

Begins ......... $20^h\ 19^m\ 55^s$ mean time at Greenwich.
Greatest Phase $21^h\ 32^m\ 33^s$ ” ” ”
Ends ........... $22^h\ 50^m\ 24^s$ ” ” ”

Magnitude of the Eclipse (Sun's diameter=1) 0·721.

| | |
|---|---|
| Angle, from North Pole, of | first contact, 125° towards the West.<br>last contact, 84° towards the East. |
| Angle, from Vertex, of... | first contact, 93° towards the West.<br>last contact, 100° towards the East. |

for *direct* image.

"Notes on Varieties of Sarothamnus Scoparius, Koch., and Stachys Betonica, Benth., from the Lizard, Cornwall." By Charles Bailey, Esq.

The Lizard district has long been known to be singularly prolific in critical and rare British plants; and the purpose of this communication is to draw the attention of botanists to what appear to be two undescribed but well-marked forms of the plants whose names are placed at the head of this notice, and which are found in that district.

### I.—*Sarothamnus scoparius*, Koch., var.

It is only in recent years that this plant has been admitted a Cornish species, Mr. H. C. Watson, in vol. i. of his "Cybele Britannica," p. 274, giving Devon, Isle of Wight, and Kent as its most southern limit; but in the additions included in vol. iii. of the same work, Mr. Watson states (p. 404) that "the south limit extends to Cornwall, according to Mr. Gibson and Mr. Pascoe"—no details, however, being given as to the precise part of the county in which it occurs. The specimen exhibited was found growing in small patches on the cliffs of serpentine rock about Vellan Head, situate about four miles north-west of the Lizard Lights, and it differs from the normal form, here named var. *a*, in the following characters:—

Var. *a, erecta.*—Stems erect, bushy; leaves stalked, the petioles as long as, or longer than, the leaflets; leaflets elliptical-obovate, bluntish.

Var. *β, prostrata.*—Stems prostrate, spreading; leaves shortly stalked or sessile; leaflets ovate-acute, acuminate.

The Cornish form, here named *β, prostrata,* differs from the normal plant chiefly in its habit of growth, which, instead of being erect and bushy, is remarkably prostrate, the branches spreading out in fan-shaped patches, and growing flat upon the ground; the branches, particularly in the upper half, are densely clothed with short spreading hairs; the leaves have shorter stalks, with a greater tendency to suppress the two lateral leaflets, the majority of the leaves, in fact, being unifoliate; the pods are less numerous, have their dorsal and ventral sutures covered with long silky hairs, and are black rather than brown, shorter, and have fewer seeds.

The season was too far advanced for any flowers to be met with, either on Vellan Head or in the small valley running down from Jollytown—the only other locality in Cornwall where the plant was observed.

### II.—*Stachys Betonica,* Bentham var.

Of this plant three well-marked forms' have been described: *a, Betonica hirta,* Reich.; *b, B. serstina,* Host.; and *c, B. stricta,* Ait.; and in many respects the form about to be described agrees with the first of these forms. In Mr. Babbington's Manual (ed. v., p. 261) it is stated that "the English plant has the round crenate, not emarginate, lower lip of *B. hirta* (R.);" but Professor Boreau is of opinion that, while the three forms just named preserve their remarkable differences of aspect when cultivated together, the distinctive characters furnished by the divisions of the corolla are but slightly constant. *(Flore du Centre de la France,* &c., ed. iii., vol. ii., p. 530.)

Stems decumbent, numerous, radiating from the rootstock, square above, rounded below, clothed with many short hairs, which are closely appressed in the upper part and pointing downwards, those in the lower part more spreading, but still much reflexed; spikes slightly inclined, just raised above the ground, compressed-globose, the verticils many-flowered, never distant; calyx covered with straight hairs, the sepals ending in stiff points; corolla three times longer than the calyx, the exterior covered with scattered shaggy hairs, which are long and silky at the base of the tube, but becoming shorter and more scattered as they approach the lip; opening of the mouth very wide, lower lip crenate, wavy; lower leaves on long stalks, cordate at the base, oblong, regularly crenate, glandular on the under surface, with short scattered hairs, upper leaves lanceolate on short stalks.

Specimens of *B. hirta*, Reich., have not come under my notice, nor have I been able to meet with Reichenbach's diagnosis; but the form described above seems to agree very nearly with Professor Boreau's description of that plant, which is here appended for the sake of comparison: "Stem clothed with many short stiff hairs; leaves with soft long hairs, very distinctly crenate; spike short, interrupted; calyx softly hairy at the summit; lower lip of the corolla rounded crenate." *(Flore, &c., loc. cit.)* Mr. Bentham in his *Labiatarum genera et species*, p. 532, gives, amongst the synonyms of his *Stachys Betonica*, "*Betonica hirta*, Leyss., Reichb., Icon., Bot. Eur. 8, 4, t. 711," which may be identical with *B. hirta*, Reich.; but the only reference to it which I have met with is in Dr. Garke's *Flora von Nord-und-Mittel Deutschland*, where it is shortly described as "Var. *a*, *hirta*, Leyss.— Stem with short hairs, calyx rough-haired." (Ed. vi., p. 318.)

The Cornish form is very plentiful on the cliffs of "Killas" rock, lying between Caerthilian and the Lizard Lights, growing with *Genista tinctoria*, L., var. *humifusa*, Dicks., which it much resembles in habit. The same form is also met with in several other parts of South-Western Cornwall, as at Cuddan Point and the Mount's Bay district generally.

The above communication was preceded by a few remarks on the following plants of South-Western Cornwall, specimens of which were exhibited at the meeting:—

| | |
|---|---|
| Raphanus maritimus, Sm. | Cliffs under the Lizard Lights. |
| Brassica alba, L. | „ „ „ |
| Arenaria verna, L., var. β, Gerardi Willd | Rocks at Rill Head. |
| Spergularia rupestris, Lebél non Camb | Nanjissal Bay, Land's End; plentiful. |
| Tamarix Anglica, Webb | Mount's Bay. |
| Lavatera arborea, L. | Cliffs, Newlyn. |
| Trifolium subterraneum, L. | Penzance. |
| „ scabrum, L. | „ |
| Anthyllis vulneraria, L. (a very robust form) | Porthgwarra, Land's End. |
| Anthyllis vulneraria, L., var. β, Dillenii, Schult | Forming the herbage on the sandy downs above Whitsand Bay, and common elsewhere. |
| Genista pilosa, L. | Gue Graze. |
| „ tinctoria, L., var. β, humifusa, Dicks | Plentiful between Caerthilian and the Lizard Lights. |
| Illecebrum verticillatum, L. | Madron parish. |
| Herniaria glabra, L. | Common at the Lizard. |
| Valerianella olitoria, Mönch | Fields, Sennen Cove. |
| „ dentata, Koch | „ „ „ |
| Wahlenbergia hederacea | St. Paul, and generally distributed. |
| Erica vagans, L. | Gomhilly, Pradannack, and Lizard Downs. |
| „ ciliaris, L. | Edgecombe Downs, Carclew. |

| | |
|---|---|
| Erythræa pulchella, Fries | Mount's Bay. |
| „ centaurium, Pers. | A stunted broad-leaved form, from Porth Curnnow (non E. latifolia, Sm.) |
| „ littoralis, Fries | Mount's Bay. |
| Sibthorpia Europæa, L. | St. Madron's Well. |
| Linaria Elatine, Mill | Marazion. |
| Allium sibiricum, L. | Rill Head. |
| Asparagus officinalis, L. (?) | „ |
| Asplenium lanceolatum, Huds | Whitsand Bay. |

"Notes on Wood-Feeding Coleoptera," by JOSEPH SIDEBOTHAM, Esq.

The number of species of coleoptera that feed upon wood in this country is considerable, some attacking growing trees, others when cut down or partially decayed, others attack solid timber when cut up and used for buildings or furniture. The various species are not confined to one or two of the great divisions, but are to be found scattered through most of them, being found in the sections *Necrophaga Lamellicornes, Sternoxi, Malacodermata, Rhyncophora,* and *Longicornes,* &c.

As might be expected, their modes of attack on trees are as varied as their organisation, and their study is one of great interest to naturalists, besides being of the greatest importance to the owners of plantations and forests. Some species mine in the bark, others between the bark and the solid wood, others in roots and branches, and there are few, if any, of our native trees that are not liable to attacks from one or more species.

The amount of damage done to a forest when a few species get fairly established in it is very great, and too often the woodpeckers, which would assist in checking their ravages, are destroyed, because they bore holes in the trees to get at the insects.

At the present time the fine spruces in Dunham Park are being rapidly destroyed by one of the large weevils *Hylobius abietis*, and many ash trees by a small species mining in the bark, *Hylesinus fraxini*. In the valley of the Spean in Perthshire the alder trees are being destroyed by the larvæ of *Stenocdrus bifarviatum*. They begin the work, which is joined in by some smaller species, to such effect that you may see there trees, 30 inches in diameter, through which you can thrust a walking stick.

A short time ago I accompanied a friend to his fishing cottage, in the north of Lancashire. It had been closed up for a little time, and the chairs and tables had been attacked by *Anobium striatum*, and although in appearance quite perfect, when touched almost crumbled to dust.

I bring for exhibition a piece of bark mined by two species of beetle, of great interest to entomologists, *Hylesinus vittatus* and *Nemosoma elongata*. The latter species was taken in 1833 by Mr. Ingall, at Sydenham, and since then it has not been met with, except about three specimens, until the spring of this year, when my friend Dr. Power found it in Warwickshire and investigated its habits, publishing an account in the *Entomologist*. From his observations he ascertained that this species feeds on the larvæ of *Hylesinus vittatus*, carrying its galleries across so as to intercept and devour them. Having myself once found the *Hylesinus* in some old railings at Beeston, near Nottingham, when I was at the British Association meeting in August I again visited the place, and after careful search found some specimens of the rare *Nemosoma* in its mines across the tracks of *Hylesinus*. The portion of bark exhibited will show the mines, and the carded specimens will show the two species.

Stevens mentions Colwich near Nottingham, on the authority of Dr. Howitt, as a locality for *Nemosoma*; so no doubt by careful search it may be found in many other places around Nottingham. I also exhibit some bits of oak branches from Dunham mined by *Scolytus intricatus*, and a few other species of wood-boring coleoptera found in this neighbourhood, with portions of wood attacked by them.

*Scolytus destructor*, the species which has been so destructive to elm trees near London and Paris, is not common here, probably in some measure because *Ulmus campestris* is not one of our common trees.

To the entomologist the investigation of the specific differences, the habits and instincts, and the peculiar conformation of these creatures to adapt them to their mode of life, are sources of great pleasure, but he is at the same time more impressed with the enormous amount of damage they inflict than an ordinary observer, and also with the want of knowledge of those who are interested in the preservation of our woods and forests. It may happen that the removal of a tree, or even a branch, when attacked by a particular species, may save a forest, but it must be done at the proper time, so as to destroy the insects with it before they can escape to propagate their species, and it certainly ought to be part of a qualification for a forester or park keeper that he should know the life history of these wood-boring beetles and the plans that have been adopted for their destruction.

## Ordinary Meeting, December 26th, 1866.

EDWARD SCHUNCK, Ph.D., F.R.S., President, in the Chair.

Mr. PETER HART exhibited a piece of lead pipe from the Ardwick Bridge Chemical Works, coated internally with oxide of manganese. The pipe was one inch in diameter, and the thickness of the coating of oxide of manganese was about a quarter of an inch. The oxide has not been minutely examined, but it consists no doubt largely if not wholly of the hydrated peroxide, as it evolves chlorine in quantity when gently heated with hydrochloric acid. The water used in the works is collected in tunnels cut in the red sandstone, and there is no doubt that the source of the manganese is the refuse chloride from the chlorine stills situated almost immediately over one of the tunnels. A portion of this chloride, having filtered through the sandstone, has come into contact with the bicarbonate of lime in the water, chloride of calcium and carbonate of manganese have resulted, which latter body, by after contact with oxygen, has become wholly or partially converted into peroxide.

"On some recent observations on the Specific Gravity of Sea Water," by THOMAS HEELIS, F.R.A.S.

The author had in the course of several long voyages accumulated a number of observations on this subject, all of which were made at sea with the hydrometer. On coming to examine and reduce his results, he had referred to a paper by the late Professor Forchhammer of Copenhagen, published in the Philosophical Transactions. The results arrived at by the Professor differed so much from those obtained by the author that a careful examination of the Professor's paper became necessary before any progress could be made in the reduction of the author's observations, as, if the results of the Professor were correct, the observations must involve such errors that their reduction would be useless. The examination was therefore undertaken, and was extended to the results of the American observations, which are shown in a curve given among the plates to Maury's Physical Geography of the Sea (9th edition). The American observations were found to accord with those of the author and to differ from the results obtained by the Professor to an extent in some cases of one per cent of the total salinity. The apparent cause of the discrepancy seemed to be the length of time during which some of the Professor's specimens were kept before being subjected to analysis, the paper in the Philosophical Transactions showing that in one case at least this interval amounted to some twenty years. A further examination of the paper showed, as the author conceived, great errors in arrangement, and especially in the determination of the areas, all the water taken in which, was assumed to be from the same region, and all the results of which specimens were included in a common average. There was a strange deficiency in the observations for certain regions, but the boundary lines of the areas were so arranged that the Gulf Stream was included in three regions, the L'Agulhas current in two, and the Equatorial current in two at least; so that if the results had not been drawn from too few specimens, the effect of the high specific gravity of these currents would have been entirely masked by the observations of other water not the subject of current, but included in the averages as belonging to the same regions. As it was, the same result, viz. the disappearance of the effect on the average of each region of the high specific gravity of the waters of these currents was found to occur because of the small number of observations in the currents, there being only one, for instance, of the L'Agulhas current, and this one being masked in the average of the Indian Ocean region, the specific gravity of part of which, as is well known, is reduced by the fresh water of the large Asiatic rivers.

## PHOTOGRAPHICAL SECTION.

December 13th, 1866.

J. Baxendell, F.R.A.S., Vice-President of the Section, in the Chair.

**Mr. Coote** exhibited a frame for printing transparencies on glass, of which the following is a description :—

As frames for printing transparencies for the lantern and stereoscope have occupied some little attention of late, in the *British Journal of Photography*, I thought it might be useful to submit to the inspection of the meeting to-night one that I had made last winter for printing stereo-transparencies from negatives, on plates 7¼in. by 4¼in., the size I always use on account of being able to take single pictures in the same camera. The frame is the same in principle as the usual frame for printing transparencies by superposition, and without having to cut either negative or print by simply moving the prepared plate and negative to the right and left in opposite directions alternately; but it is a little longer, and 4½in. broad inside. The opening for exposure is in the centre, but is made longer than is required for a stereo print. Over this opening I place a mat made of thin brass, the desired size and shape of the picture required. This mat is sunk into the bed of the frame until it is quite level with it. A negative is then put in, and by moving the mat up or down in its recessed bed, you can get more sky or foreground at will, and the movement right and left gives you any other portion of the picture you may desire. Having settled that point, I then place on the negative a *straight* strip of soft cardboard, wide enough to reach, within about an eighth of an inch, the bottom of the opening in the mat; and then place on the prepared plate, which is kept in a horizontal position by resting on the strip of cardboard; and it also enables you to get the two sides of the picture exactly on a level with each other after reversing, to close the frame. A loose back, with a brass spring on, is put in, and a small lever bar brought down over the spring and held in its position by a small catch. It is necessary to have several strips of cardboard, varying in width, and it will be obvious to any one that they must not exceed in thickness the glass of the prepared plates, otherwise the pressure of the back is kept off the glass and the plates are not in close contact.

By use of this frame I can easily take out for the lantern portions of pictures taken the full size of the plate, say 7¼in. by 4½in., whether the subjects be horizontal or upright.

Mr. DANCER then exhibited, by means of the oxyhydrogen lantern, a very beautiful series of views, by Mr. Buxton, representing scenes in Scotland, Egypt, India, &c.; and also views in Derbyshire, Cheshire, and other places, by Mr. Coote and Mr. Brothers.

## Ordinary Meeting, January 8th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. BINNEY, F.R.S., F.G.S., exhibited two remarkable fossils discovered by Mr. Joseph Tindall, of Thomas Street, Huddersfield, in the lower coal measures near that town. One was an insect, and, according to Mr. Tindall, belonged to Dr. Dawson's genus *Xylobius* and probably to his species *Sigillariæ*. It was found in an old deep mine at Cooper Bridge, and is the first instance of a specimen of that genus having been met with in England. The other bore some resemblance to the pupa state of a coleopterous insect, not much unlike the pupa of a nut weevil or some such insect. It was found in the Cinderfield Dyke Pit at Bradley, near Huddersfield. These specimens give us evidence of the former existence of insect life during the corboniferous epoch which a few years since we should scarcely have expected; but after the discovery of a fossil spider in the German coal measures scarcely to be distinguished from a recent genus, we must expect great additions to be made to the carboniferous *fauna*, as doubtless the rich and luxuriant vegetation of that remote period would afford food and shelter for numerous insects.

I. "On the amount of Carbonic Acid contained in Sea Air," by T. E. THORPE (Dalton Scholar in the Laboratory of Owens College), communicated by Professor H. E. Roscoe, F.R.S., &c.

With the exception of the results of some determinations already communicated to the Society by the author, the only series of determinations hitherto published of the amount of carbonic acid contained in the air above the ocean are those of Lewy. At the request of the French Academy, Lewy, in 1848, collected the air of the Atlantic at different times during a voyage from Havre to Santa Marta, and determined the proportion of its three principal constituents in the eudiometric apparatus of Regnault and Reiset. The mean composition of the air of the Atlantic from Lewy's analyses is in 10,000 volumes,

| | |
|---|---|
| Carbonic Acid | 4·630 |
| Oxygen | 2102·750 |
| Nitrogen | 7892·620 |
| | 10000·000 |

These numbers differ but slightly from those usually given as representing the average composition of the atmosphere of the land, but on examining the experimental details of Lewy's analyses it appears that the air of the day was found to be considerably richer in carbonic acid and oxygen than the air of the night. The following are the means of each series:—

| | The Day (7 Expts). | The Night (4 Expts). |
|---|---|---|
| Corbonic Acid | 5·299 | 3·459 |
| Oxygen | 2105·801 | 2097·412 |

This remarkable difference appeared to become greater as the middle of the ocean was approached, where the air of the sea was less liable to be mixed with that of the land. These singular diurnal variations in the composition of the atmosphere of the sea are ascribed by Lewy to the evolution, and consequent admixture with the superincumbent air, during the day time, of dissolved gases from the heated surface-layers of the sea, such gases being considerably richer, as is well known, in oxygen and carbonic acid than ordinary air. During the night, on the other hand, the disengagement of gas is supposed to be arrested. Morren, and subsequently

Lewy, have shown that the changes in the relative amounts of the several gases held in solution in sea-water depend:—(1) upon the variations in intensity of direct and diffused solar light, producing a corresponding effect upon the vitality of sea plants and animals; and (2) upon the alterations in temperature affecting the relative amounts of the dissolved gases in accordance with the known laws of gaseous absorption. Consequently if such cases can at all influence the composition of the atmosphere of the sea it is reasonable to expect that the variations would be most perceptible in the air above the tropical oceans, in whose tepid waters infusoria exist in enormous quantities, and where the intensity of total sunlight is very great and its changes exceedingly rapid.

It would then appear from Lewy's experiments that sea-air is sensibly richer in carbonic acid than the air of the land, and that, contrary to the *a priori* statements of Vogel and Krüger sea-water does not abstract the carbonic acid from the air, but even causes a sensible increase in its comparative amount. Considering the difficulty generally experienced in accurately noting in the eudiometer contractions so minute as the absorption of the carbonic acid from a small volume of atmospheric air, it appeared desirable to test the validity of the above conclusions by a series of experiments made by one of the more convenient and accurate methods which we now possess for the estimation of atmospheric carbonic acid.

In a paper which had the honour of being read before the Society, the author communicated the results of a series of determinations of the amount of carbonic acid contained in the air over the Irish Sea. All these determinations were made by Pettenkofer's method, according to which the amount of atmospheric carbonic acid can be estimated with far greater accuracy than by any eudiometric method hitherto described. The details of the method of analysis were fully described in the paper above referred to. The experiments on the air of the Irish Sea were, by the kind permission of the Honorable Board of Trinity House, made during the month of August 1865, on board the "Bahama Bank" light vessel, situated in Lat. 54° 21′ N, and Long. 4° 11′ W, seven miles W.N.W. of Ramsey, Isle of Man, and consequently nearly equidistant from the shores of England, Scotland, and Ireland. The times of observation were 4 a.m. and 4 p.m., the hours of nearly minimum and maximum temperature, when it was assumed that any differences in the air of day and night similar to those observed by Lewy over the Atlantic Ocean would be most perceptible. It appears from the 26 experiments embodied in the Table of Results accompanying the paper, that the mean quantity of carbonic acid in the air over the Irish Sea was in the month of August 1865, 3·08 in 10,000 volumes of air—a very marked difference from the mean amount on the land, viz., 4·0 vols. in 10,000, as deduced from the most extensive observations on land-air. It was therefore concluded that the Sea in our latitudes does not act in increasing the amount of carbonic acid in the air above the ocean, as found by Lewy over the Atlantic near the equator. And it also appeared from those experiments that the differences observed in the air of night and day by Lewy over the Atlantic are not perceptible in the air above the Irish Sea.

The kindness of Messrs. Alfred Booth and Co., of Liverpool, has enabled the author to test the validity of Lewy's conclusions respecting the composition of the atmosphere above the tropical oceans by a series of determinations of the amount of carbonic acid contained in the air of the Atlantic, made by Pettenkofer's method, during a voyage to and from the Brazils. The bottles employed in collecting the air were however of increased capacity, since by thus experimenting on a larger volume of air any variations in the amount of atmospheric carbonic acid would, of course, be rendered more appreciable. From 51 experiments made during the months of February, March, and July 1866, it appears that the mean quantity of carbonic acid contained in the air of the Atlantic Ocean is 2·953 vols. in 10,000 of air: in that of the Irish Sea it was, from 26 experiments, 3·082: the mean of the 77 experiments being 3·00.

This result compared with the following numbers giving the amount of carbonic acid contained in land air, shows that, contrary to the statement of Lewy, the air over the ocean contains a much smaller proportion of carbonic acid than that of the land.

The most extensive observations on land air have given as means:—

| Observer. | Locality | No. of Expts. | Vols. in 10,000 in air. |
|---|---|---|---|
| Th. de Saussure... | Chambeisy .............. | 104 ...... | 4·15 |
| Boussingault ...... | Paris ......... ........... | 142 ...... | 3·97 |
| Verver ........... | Gröningen .............. | 90 ...... | 4·20 |
| Roscoe ........... | London and Manchester. | 161 ...... | 3·95 |
| Angus Smith ...... | Ditto ditto | 200 ...... | 4·03 |
| General mean of observations on land air .............. | | | 4·04 |
| | | Independent Expts. | |
| General mean, observations on sea air, Lewy... | | 11 ...... | 4·63 |
| Ditto ditto ditto | Thorpe | 44 ...... | 3·00 |

The quantity of carbonic acid contained in land air is subject to continual alteration from the variable circumstances of locality, temperature, fog, rain, &c.; it may thus vary from 2·5 to 8 volumes in 10,000 of air. It would appear, however, from the above experiments that the amount of carbonic acid in sea air experiences fewer and far less extensive variations: it is sensibly the same in different latitudes and is constant in the same locality throughout the year. The mean quantity of carbonic acid contained in sea air between the parallels 15° and 30° N. latitude was in the months of February and March 1866, 2·96 in 10,000 volumes of air; in July of the same year it was 3·00 volumes. From Saussure and Boussingault's observations a decided difference may be traced between the amounts of carbonic acid in the air of day and night on the land, the air of the night containing more carbonic acid than that of the day in the proportion of 100 to 92; but, on comparing the experiments made at night with those of the day, it appears from the author's observations that no such differences are to be discerned in sea air. The means of the two series are—

| | For the Day. (24 observations) | For the Night. (20 observations) |
|---|---|---|
| Carbonic acid in 10,000 vols. of air... | 3·011 ............ | 2·993 |

The non-accordance of these results with those obtained by Lewy is undoubtedly mainly due to the difference in the methods of analysis employed. Lewy's determinations were made in Regnault's eudiometer in which, as is well known, the volume of gas is maintained constant throughout the analysis, the relative changes being estimated by the variations in the pressure of that constant volume over or under that of the atmosphere, measured in millimetres of mercury; and thus, as Dr. Frankland as already pointed out, a variation in volume, which in the older method of Bunsen would appear considerable, is in that of Renault only represented by a small numerical expression, and consequently a very slight error of observation (say to the extent of one-twentieth of a millimetre) would exercise a very considerable influence on the result. It deserves also to be noted that Lewy's analyses were not made until eighteen or twenty months after the air had been collected, and although Lewy satisfied himself by experiment that air contained in glass tubes for such a length of time experiences no alteration in composition, yet Regnault has subsequently shown that the exact determination of the amount of atmospheric carbonic acid cannot be made in air thus preserved, since the glass absorbs a portion of this gas.

The main conclusions to be drawn from this paper may be briefly stated as follows:—

1. That the sea does not act in increasing the amount of atmospheric carbonic acid.
2. But that, on the contrary, the air over the sea contains a much smaller proportion of carbonic acid than the air of the land, although the influence of the sea in abstracting this gas from the atmosphere is not so great as the observations of Vogel and Krüger would indicate.
3. That the mean quantity of carbonic acid contained in the normal atmosphere of the ocean is 3·00 in 10,000 volumes of air.
4. That the proportion is constant, or nearly so, in different latitudes.
5. That this proportion is not sensibly influenced by the different seasons of the year.
6. That this proportion does not experience any perceptible diurnal variation.

II. "On the Amount of Carbonic Acid contained in the Atmosphere of Tropical Brazils during the rainy season," by T. E. THORPE.

The determinations contained in this communication of the amount of carbonic acid in the land air of the tropics were made at Pará, the principal port of entrance of the river Amazon, situate about eighty miles from the sea, on the river Grâm-Pará, in lat. 1° 27′ S., and long. 48° 28′ W. This town is built on the verge of a vast primeval forest, extending to the sea coast, and over which the trade winds of the Atlantic regularly blow during the greater part of the year. The determinations were made by Pettenkofer's method, in the manner previously described. Hydrochloric acid was, however, substituted for oxalic acid in the titration of the baryta-water, since solutions of the latter acid decompose rapidly in the tropics. From 31 experiments it appears that the mean quantity of carbonic acid contained in the air of tropical Brazils during the months of April and May 1866 was 3·28 in 10,000 volumes of air—a decided difference from the mean proportion (4·0 vols. in 10·000 of air) contained in the atmosphere of Europe, and probably due in great measure to the joint action of tropical rain and tropical vegetation in withdrawing this gas from the air. The influence of rain on the amount of atmospheric carbonic acid is well known from the experiments of Saussure and Boussingault. The annual rainfall at Pará is very heavy, amounting, according to observations kindly furnished to the author by Drs. Bruno-Cabral and José Abreu to nearly 3 metres (118 inches), of which about one-third falls during the months of March, April, and May. These determinations are consequently interesting as showing the proportion of carbonic acid in the atmosphere of the tropics in the middle of the rainy season of the year.

In conclusion, the author begs to tender his thanks to Professor Roscoe for the valuable advice and assistance he has given him during the above investigations.

## PHYSICAL AND MATHEMATICAL SECTION.

November 8th, 1866.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

Mr. BAXENDELL, F.R.A.S., read a paper entitled "Observations of the new Variable Star T Coronæ." (This paper was afterwards read at the Ordinary Meeting of the Society held November 27th, 1866. See page 35.)

Mr. BROTHERS, F.R.A.S., communicated an Appendix to his "Catalogue of Binary Stars" presented to the Section at its last meeting.

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December 6th, 1866.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

Mr. HEELIS, F.R.A.S., exhibited some drawings of a Hydraulic Clock which he had made for the purpose of driving his Equatorial, and described the nature of its action and adjustment, the principal feature being that of using the same water over and over again.

Mr. BROTHERS, F.R.A.S., communicated the following observation of the occultation of Aldebaran, which took place on the 22nd ult.:

Disappearance, $1^h\ 53^m\ 32{\cdot}5^s$ local sidereal time. The star appeared projected on the moon's limb for at least half a second, and then disappeared instantaneously.

Position of the Observatory: Lat. 53° 27′ 9″ N; Long. $0^h\ 8^m\ 54{\cdot}5^s$.

Mr. HEELIS, F.R.A.S., read a paper "On some recent results of Observations of the Specific Gravity of Sea Water." (This paper was afterwards read at the Ordinary Meeting of the Society held on December 26th. See page 55.)

Mr. VERNON, F.R.A.S., exhibited a diagram showing the excessive amount of barometric and thermometric oscillation which had occurred during the month of November, 1866.

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January 3rd, 1867.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

"On the Mean Weekly Temperature at Old Trafford, Manchester, for the seventeen years 1850—1866," by G. V. VERNON, F.R.A.S., F.M.S.

As I am not aware that there have been any carefully deduced values of the mean temperature of this neighbourhood, perhaps the data accompanying this paper may serve until such time as a more extended series can be obtained. I may state that the thermometers used have all been standard ones compared at Greenwich, and all the observations have been reduced to that standard. The thermometers are placed upon a stand 4 feet from the ground, and carefully protected from radiation and other disturbing influences.

The unavoidable omissions in my register I have been enabled to supply by the kindness of my friend Mr. John Curtis, F.M.S., whose thermometers are placed similarly to my own, and within a very short distance from my station. The mean values have generally been determined from the readings of the maximum and minimum thermometers in the shade combined with readings of a standard thermometer read once a day; these observations being all made at 8 a.m.; in reducing them Mr. Glaisher's corrections for diurnal range have invariably been applied. Whilst upon this subject I would like to suggest the great desirability of having these corrections deduced from a much larger series of observations, for although on the whole I find them to agree pretty closely, yet at times great differences exist, especially in comparing the mean temperature deduced from maximum and minimum readings with those of a standard thermometer read at certain fixed hours.

The coldest weeks during the 17 years average appear to be those ending 7th and 14th January, and the warmest that ending July 22nd.

The week ending December 29th appears to be the one in which the greatest variation of mean weekly temperature is likely to occur, and the one ending August 19th that in which the least variation occurs.

Taking the mean differences for each month, and we obtain the following figures:—

| | Mean Differ. | | Mean Differ. |
|---|---|---|---|
| January............ | 16·40° | July .............. | 13·22° |
| February ......... | 18·62 | August ............ | 8·20 |
| March ............ | 13·40 | September......... | 10·26 |
| April ............... | 12·05 | October ............ | 11·34 |
| May ............... | 14·37 | November......... | 13·62 |
| June ... ........... | 12·42 | December ......... | 20·22 |

From this table we see that the greatest amount of variation occurs in December, and the least amount in August.

October does not appear to exhibit any abnormal variation, although from the amount of barometrical oscillation doing, so it might have been expected it would do.

I should like to see a much longer period observed for determining the values of the mean weekly temperature here, but hope until such is the case the values I have given may be deemed worthy of the confidence so short a period can deserve, and I can safely say, that as the great mass of the observations were made by myself, every care has been taken in order that they might be made correctly.

## MEAN WEEKLY TEMPERATURE AT OLD TRAFFORD, MANCHESTER.

106 Feet above Sea Level.

| Week Ending | Mean Temp. 17 Years, 1850–1866. | Difference between Warmest and Coldest Week. | Week Ending | Mean Temp. 17 Years, 1850–1866. | Difference between Warmest and Coldest Week. |
|---|---|---|---|---|---|
| January 7 | 37·5° | 21·7° | July 8 | 58·3° | 10·7° |
| „ 14 | 37·5 | 15·0 | „ 15 | 60·6 | 16·9 |
| „ 21 | 38·3 | 13·9 | „ 22 | 61·0 | 12·1 |
| „ 28 | 38·7 | 15·0 | „ 29 | 59·8 | 13·2 |
| February 4 | 38·0 | 14·2 | August 5 | 58·6 | 7·7 |
| „ 11 | 39·0 | 14·3 | „ 12 | 59·6 | 10·6 |
| „ 18 | 38·5 | 22·8 | „ 19 | 57·8 | 7·2 |
| „ 25 | 37·7 | 23·2 | „ 26 | 58·0 | 9·1 |
| March 4 | 39·8 | 13·8 | September 2 | 57·8 | 6·4 |
| „ 11 | 39·5 | 14·1 | „ 9 | 57·5 | 7·6 |
| „ 18 | 40·8 | 10·8 | „ 16 | 56·1 | 13·4 |
| „ 25 | 41·0 | 12·2 | „ 23 | 54·5 | 13·8 |
| April 1 | 42·4 | 16·3 | „ 30 | 53·3 | 10·1 |
| „ 8 | 45·4 | 14·1 | October 6 | 53·2 | 7·5 |
| „ 15 | 46·5 | 8·2 | „ 13 | 49·8 | 11·2 |
| „ 22 | 48·0 | 15·1 | „ 20 | 49·0 | 12·7 |
| „ 29 | 48·0 | 10·8 | „ 27 | 47·1 | 16·6 |
| May 6 | 47·3 | 12·8 | November 3 | 44·7 | 8·7 |
| „ 13 | 49·3 | 12·9 | „ 10 | 43·1 | 14·0 |
| „ 20 | 52·8 | 18·4 | „ 17 | 40·4 | 12·6 |
| „ 27 | 54·4 | 13·4 | „ 24 | 40·6 | 16·8 |
| June 3 | 54·7 | 12·6 | December 1 | 39·5 | 16·0 |
| „ 10 | 56·8 | 11·1 | „ 8 | 40·1 | 14·3 |
| „ 17 | 57·2 | 14·1 | „ 15 | 40·7 | 20·9 |
| „ 24 | 58·1 | 10·9 | „ 22 | 39·0 | 18·6 |
| July 1 | 60·0 | 13·4 | „ 29 | 39·0 | 27·1 |

## "On the Rainfall for 1866, at Old Trafford, Manchester," by G. V. VERNON, F.R.A.S., F.M.S.

Rain Gauge 3 feet above the Ground, and 106 feet above Sea Level.

| Quarterly Periods. 1865. | Quarterly Periods. 1866. | 1866. | Fall in Inches. | Average of 73 Years. | Differences. | No. of Days' Rainfall in 1866. | Quarterly Periods. 1866. | Quarterly Periods. 1865. | Quarterly Periods for 73 Years. |
|---|---|---|---|---|---|---|---|---|---|
| Days | Days | | In. | In. | In. | | In. | In. | In. |
| 48 | 59 | Jan. | 3·252 | 2·480 | +0·772 | 22 | 8·403 | 7·143 | 7·166 |
| | | Feb. | 2·983 | 2·387 | +0·596 | 20 | | | |
| | | March. | 2·168 | 2·299 | −0·131 | 17 | | | |
| 36 | 32 | April | 0·299 | 2·001 | −1·702 | 7 | 5·814 | 5·226 | 7·258 |
| | | May | 1·540 | 2·356 | −0·816 | 9 | | | |
| | | June | 3·975 | 2·901 | +1·074 | 16 | | | |
| 36 | 67 | July | 4·309 | 3·571 | +0·738 | 13 | 16·556 | 7·502 | 10·441 |
| | | Aug. | 5·119 | 3·616 | +1·503 | 26 | | | |
| | | Sept | 7·128 | 3·254 | +3·874 | 28 | | | |
| 44 | 56 | Oct. | 2·521 | 3·816 | −0·295 | 13 | 12·396 | 8·518 | 10·548 |
| | | Nov. | 5·721 | 3·494 | +2·227 | 20 | | | |
| | | Dec. | 4·154 | 3·238 | +0·016 | 23 | | | |
| 164 | 214 | | 43·169 | 35·413 | +7·756 | 214 | 43·169 | 28·389 | 35·413 |

The rainfall for 1866 has been 7·756 inches above the average of the last 73 years, and 14·780 inches above the fall for 1865. The fall occurred upon 214 days in 1866, as compared with 164 days in 1865.

The principal excess occurred in the three months of July, August, and September, the fall in the latter month being the greatest fall for September during the entire period. In the months of April, May, and June the total fall was considerably below the average, June, however, taken alone, having an excess, but April had an unusually small fall.

With the exception of October, the rainfall was greatly in excess from June to December, and this would tend still further to bear out the supposition that October is subject to some peculiar abnormal disturbances, as pointed out by Mr. Baxendell (*Memoirs*, 3rd Series, Vol. I., page 263), and also by myself (*Proceedings*, Vol. V., page 87), in former communications.

During the later months of the year there were excessive barometric and thermometric disturbances, as shown by the amounts of oscillations.

The small amount of rainfall in October this year is very remarkable, as it is on an average the wettest month of the entire year, as will be seen on comparing the annexed figures.

The daily falls exceeding half an inch were as follows:—

| | Inches. | | Inches. |
|---|---|---|---|
| January 21 | 0·568 | September 2 | 0·500 |
| May 11 | 0·760 | „ 8 | 1·150 |
| June 18 | 1·006 | „ 11 | 0·650 |
| July 3 | 0·610 | „ 20 | 0·550 |
| July 28 | 1·985 | „ 28 | 0·770 |
| August 1 | 0·900 | October 24 | 0·523 |
| „ 16 | 0·670 | November 15 | 1·240 |
| „ 29 | 1·084 | „ 16 | 0·802 |
| September 1 | 0·750 | December 3 | 0·580 |

The great fall of November 15 and 16 was accompanied by the great flood of the Irwell, the Irk, and the Medlock. A fall of half an inch in this neighbourhood may be considered excessive, and even the above table only gives 18 days on which the rainfall reached this amount, during what may be regarded as a very wet year. Examination of the figures given by Mr. Curtis, F.M.S., in his paper printed in Vol. XV. of the 2nd Series of *Memoirs* of our Society, gives the following years in which the rainfall exceeded 40 inches since 1794:—

| | Inches. | | Inches. |
|---|---|---|---|
| 1823 | 44·767 | 1836 | 45·551 |
| 1824 | 42·941 | 1841 | 41·190 |
| 1828 | 45·267 | 1845 | 41·415 |
| 1830 | 40·861 | 1847 | 45·230 |
| 1833 | 41·677 | 1852 | 45·730 |

In 1860, which was generally considered a very wet year the rainfall was only 36·530 inches, but fell upon 235 days, or 21 more than this year (1866), the principal difference being that in 1860 rain was continually falling, though often not much in amount, whereas this year rain has fallen on a large number of days, and often very heavily.

"Results of Observations of Variable Stars made at the Mannheim Observatory during the year 1866," by Dr. E. Schönfeld, Director of the Observatory. Communicated by Joseph Baxendell, F.R.A.S.

The number preceding the name of each star in the following list is the ordinal number of the star in the Author's "Catalog von veränderlichen Sternen mit Einschluss der neuen Sterne. Mit Noten."

6. *S Cassiopeæ.*—The last maximum occurred 1866, August 23. The period will be about 573 days.
8. *R Piscium.*—The approaching maximum appears likely to occur considerably sooner than the elements require.

15. *R Persei.*—The maxima of the present year occurred on April 27 and November 20; the first, however, was observed in the evening twilight, and the time may in consequence be slightly in error. It appears, therefore, that the period is about 205·5 days, and that one period must be added to the number hitherto assumed to have happened since the date of Bessel's observation. This correction, however, does not accord with an observation made at Bonn 1853, November 30, where the star was omitted in one of the zones observed by me, although it would then be near a maximum. But it was on that evening bright moonlight.

24. *R Aurigæ.*—This star has again had another double period. A maximum took place March 4, mag. 8·8; a minimum, June 7, mag. 9·4; and then a principal maximum on August 26, mag. 7·0. The interval between my two observed principal maxima is 463 days.

30. *R Canis minoris.*—In its last minimum, 1866, March 10, this star was of the 10th magnitude. The obsertion of Oudemans, according to which it was then invisible, appears to rest upon an error.

35. *U Geminorum.*—A very short maximum of this star occurred in April, 1866. On the 15th it was of its ordinary minimum brightness, or at most, but a very little brighter than usual. April 16 and 17 the weather at Mannheim was cloudy.

| | | | | | | |
|---|---|---|---|---|---|---|
| April 18, at | $8^h$ | $18^m$ | M. T. Mannheim, | U was = | 9·5 | mag. |
| | 11 | 3 | „ | „ | 9·4 | „ |
| 19, | 8 | 29 | „ | „ | 9·7 | „ |
| | 10 | 36 | „ | „ | 9·7 | „ |
| 20, | 10 | 41 | „ | „ | 10·2 | „ |
| 21, | 8 | 58 | „ | „ | 11·1 | „ |
| 23, | 8 | 36 | „ | „ | 13·0 | „ |

These observations give the epoch of maximum = 1866, April 18·5.

39. *S Cancri.*—My Ephemeris in No. 1593 of the "Astronomische Nachrichten" is proved to be very nearly correct by a well-observed minimum of this star 1866, November 29, $13^{h}$ $3^{m}$ M. T. Mannheim = $12^{h}$ $42{\cdot}7^{m}$ M. T. Paris, at the earth's mean distance.

55. *V Virginis.*—A maximum 1866, April 13, occurred 23 days earlier than my elements indicated, and a comparison with Mr. Pogson's epoch for 1860 gives the period = 249·4 days; but Goldschmidt's earlier observations appear to show that the period must be increased to 251 days.

59. *S Bootis.*—A careful examination of my observations gave a maximum 1865 July 11·5; and a good determination of the following maximum gave the epoch, 1866, April 17. The corrected elements will therefore be—1865, July 14 + $275^{d}$ E.

63. *δ Libræ.*—Schmidt has communicated to me his observations of the past summer, which have enabled me again to improve this star's elements. At the same time I satisfied myself on the 9th of June, 1866, that $2^{h}$ $8^{m}$ after the epochs given in my Ephemeris a second minimum occurs. I believe that the value multiplied by E in my new Elements

Minimum 1866, April 12 $15^{h}$ $25^{m}$ M. T. Paris
+ 6 23 33 38·39 E

is three times the value of the real period, which would therefore be = $2^{d}$ $7^{h}$ $51^{m}$ $12{\cdot}80^{s}$, with a probable error = ± $2{\cdot}057^{s}$. This is a very interesting star.

64. *S Serpentis.*—The maximum of this year occurred on February 26, or exactly one year after the preceding maximum. It appears, therefore, that the period is yet longer than that given by Argelander; also, that

this inequality of the period begins to show itself as a periodical one.

71. *R Scorpii.*—Maximum, 1866, May 5.—The observations commenced only on the 24th April, but are not unsatisfactory. However, it is possible that this maximum (11 mag.) was by no means the proper one, but only a secondary one during the waning of the star, and that the proper maximum occurred much earlier.

72. *S Scorpii.*—Maximum observed 1866, June 17, tolerably good. Interval from previous year's maximum =355·5 days.

77. *S Ophiuchi.*—The elements are better than I expected, for they gave a maximum on the 20th June, 1866, whilst the epoch from my observations was June 24·5.

78. *S Herculis.*—Last maximum 1866, October 11, apparently good, but 11 days earlier than the Elements give.

89.—*T Sagittarii.*—The epoch of maximum for this year as concluded from the course of the light-curve is September 14 but I had no observations of this star from August 23 to September 28. I believe, however, that the result given cannot be many days in error. The period appears, therefore, to be rather less than one year.

90.—*R Sagittarii.*—Observed maximum 1866, July 13. The period is therefore 269¾ days. Dr. Auwers had already informed me that Pogson's number, 467 days, was erroneous.

91. *S Sagittarii.*—Maximum 1866, October 4·5, very good. The period is to me still unknown.

101. *R Delphini* (*S Aquilæ*).—Observed maxima 1865, August 30·5; and 1866, May 27·5. Interval 270 days. Although both determinations are good, the period, if regular, cannot be under 279 days.

104. *S (R) Delphini.*—My observations of the last maximum give for epoch 1866, October 18·5, which is apparently very good, but it is by no means adapted to a period of 283 days.

105. *T (S) Delphini.*—Last maximum 1866, July 10·5, which is in fair accordance with the Elements.

113. *T Pegasi.*—Observed maximum 1866, October 31. It appears, therefore, that the period is considerably shorter than as given in the Elements.

115. *S Aquarii.*—A well-observed maximum 1866, October 27·5, confirms the correctness of the Elements.

117.—*R Pegasi.*—Observed maximum 1866, July 9. Argelander has communicated to me the following maxima derived from his earlier observations:

1848, November 7, doubtful
1850, December 7
1851, December 20
1853, January 6
1854, January 21

These are all very well represented by the Elements 1856, February 17+379·5$^d$ E; and it appears, consequently, that Oudemans' maximum 1855, January 26 or 27, was observed under favourable circumstances, notwithstanding the star's proximity to the horizon.

119. *R Cassiopeæ.*—The last maximum again confirms the probability of the conclusion that the period is now becoming shorter. According to my observations it occurred 1866, April 10 (the previous one 1865, February 21·5), and the star was brighter than I had ever before seen it. It was nearly equal to λ Andromedæ, and, in spite of its low position near the northern horizon, was very good to see with the naked eye.

## PHOTOGRAPHICAL SECTION.

January 10th, 1867.

Dr. J. P. JOULE, F.R.S., &c., Vice-President of the Section, in the Chair.

Mr. COOTE exhibited a selection from the series of photographs by Braun, of Paris, consisting of copies of drawings by the old Masters, from the originals in the Louvre. Mr. Coote explained the method by which they were supposed to be printed, and from the appearance of some of the prints there could be no doubt that one of the processes suggested by Sir John Herschel about the year 1840, or some modification, had been employed; and it was evident that the method proposed by Mr. Gatty would also produce similar results. The prints are remarkable for their perfect resemblance to old drawings and sketches, and, as studies for artists and for use in schools of design, are quite equal to the originals. Of their permanence there cannot be the least doubt.

The following letters from Mr. J. B. DANCER, F.R.A.S., addressed to the Chairman, were then read by the Secretary:—

1.—I am too unwell to attend the meeting of the Section this evening, and have therefore put on paper a few remarks on "Braun's" copies of drawings of the old Masters.

Mr. Coote kindly showed me these pictures, and my first impression was, that they had been produced by Mr. Swan's process, as it is quite possible to replace the carbon by coloured pigments, and imitate the colour of any chalk, crayon, or even black lead pencil, in the latter case using plumbago. The act of rolling a picture of this kind would give it a lustre like a pencil drawing. There are specimens of this appearance in the collection.

Having had occasion to write to Mr. Swan, I requested him to give me such information as he possessed as to how these copies were produced. The following is an extract from his reply:—

"TO J. B. DANCER.

"Many thanks for your kind enquiry. Mr. Braun, of Dornach, is printing a number of copies of pictures by my process, in particular a series of Holbein's works; and if this series form a part of the collection to be exhibited at your society's meeting, they are no doubt by my process. Mr. Braun is however also using another process (Rousseau's) for the production of the Louvre pictures, and I think it most probable that all the red pictures, except the Holbeins, are by this process. I do not know precisely what M. Rousseau's process is, but I have heard that it is a modification of one of the processes of Sir John Herschel.

"You are quite right in thinking my process suitable for the imitation of old red chalk drawings; the shades of colour can be imitated exactly.

"Mr. Braun is now carrying out my process very energetically, and has erected a steam engine to drive the tissue making machinery. "JOSEPH W. SWAN."

I think, Mr. Chairman, you will agree with me, when looking over these copies, that they mark a most important advancement in the art of Photography. In these productions we have identity in colour as well as in form.

2.—At a meeting of the Photographical Section, October 4th, 1866, it may be remembered that I exhibited some specimens of photographs in various colours, on paper and calico, a process invented by Mr. John Mercer, F.R.S. I did not recollect sufficient of what Mr. Mercer had told me of the process to give you any definite information as to the production of the colours. A few days since I stumbled on his description; if it is thought of sufficient interest, please to communicate it to the members.

Mr. Mercer read a paper on the subject at the meeting of the British Association at Leeds, in the year 1858. He exhibited coloured photographs on paper and calico, and gave the following directions for producing them.

### DIRECTIONS.

34oz. of sulphate of iron are converted into peroxalate; this is diluted to 2 gallons, and will impregnate 200 square yards of paper. The paper being floated on the solution until fully wet in the usual way, it is then exposed, and afterwards steeped in some solution which only acts on that part where the iron has been reduced from the per- to the protoxide. Red prussiate of potash and sulphuric acid act well, making the image blue and the ground white.

Sulphocyanide of potassium and a salt of copper form another bath; the protoxide of the picture deoxodises the copper, and the sulphocyanide of the suboxide of copper is fixed in the cambric or paper. This may be converted into the red prussiate of copper.

A vast number of colours may be obtained by replacing the iron or copper by other metals, such as lead, zinc, tin, mercury, silver, gold, or manganese.

With these bases may be used various dyes, as madder, cochineal, murexide, logwood, galls or quercitron bark, besides the iodides, chromates, prussiates, or oxides of the metals themselves and mixtures of these.

He also showed how the peroxalate paper might be used as a fair actinometer, by placing a slip between the leaves of a book and pulling it out by steps every stated number of seconds. It is then easily converted into a graduated scale.

## Ordinary Meeting, January 22nd, 1867.

Dr. J. P. JOULE, F.R.S., &c., Vice-President, in the Chair.

Mr. **SIDEBOTHAM** called attention to the letters which had appeared in the "Times" newspaper during the last few days, with reference to chilled cast iron shot and shell, intended for penetrating armour plated ships, and as to whom the credit of first suggesting it rightly belonged. He said that no doubt whatever existed that at the Meeting of the British Association, in October, 1862, their member, Mr. Nasmyth, C.E., did most strenuously urge a speedy trial of chilled cast iron elongated shot and shell, as being not only much cheaper than steel, but better in every respect for the purpose of penetrating iron-clad ships; he also gave his reasons for such an opinion, and held his own during a long discussion against all comers.

Mr. Sidebotham then read letters to Mr. Nasmyth from P. Le Neve Foster, Esq., who acted as secretary to section G at the Cambridge meeting of the British Association, and T. Aston, Esq., Q.C., who took part in the discussion. The latter concludes his letter by saying:—

"There can be no doubt whatever, that any one who may claim to have been before you in teaching the public the use of chilled cast iron for projectiles intended to penetrate armour plates, must give proof of having done so prior to your vigorous advocacy of that material at the Cambridge meeting of 1862."

Mr. Sidebotham then read reports of the discussion from the "Times" and the "Athenæum;" both these are short, as such reports usually are, and do but represent a very small portion of what was said. The report in the "Times" of October 7th, 1862, is as follows:—

"Mr. Nasmyth inquired whether chilled cast iron shot had been tried, the process of chilling cast iron was a very inexpensive one; and if chilling flat ended shot had not been tried, it was very desirable it should be.

"The President (Mr. Fairbairn) said they had no practical experience in the matter; but he believed that the shot being chilled only to a certain extent they would find that the whole shot, having the same velocity when striking the object, would break, as if it had not been hardened at all.

"Mr. Nasmyth: That is a mere matter of opinion. (Hear, hear.) Have you tried any experiments on the subject? It would involve an expense of only some ten shillings. (A laugh.)

"The President: In order to gratify Mr. Nasmyth, I hope that before the next meeting of the British Association we shall have the matter proved experimentally. (Hear.)"

Mr. Sidebotham, in conclusion, said he thought from the above it would be seen that their member, Mr. Nasmyth, ought to have the credit of the suggestion of elongated chilled iron shot and shell, in place of steel, rather than Major Palliser, whose experiments and publication dated nine months later. He regretted the absence of Mr. Fairbairn from the meeting this evening, as he was the chairman of the meeting at Cambridge, and could have fully corroborated all that had been said.

"On Casting, Grinding, and Polishing Specula for Reflecting Telescopes," Part II., by JAMES NASMYTH, Esq., C.E., Corresponding Member of the Society.

In this part of his paper the author gives full details, illustrated by diagrams, of the methods which after many years' experience he has found to be best adapted for ensuring success in the fine-grinding and polishing of specula for reflecting telescopes.

## Storm Warnings.

Since the meeting, Mr. **Vernon**, F.R.A.S., has forwarded to the Editor a letter from Dr. **Buys Ballot**, of Utrecht, in which the writer calls attention to the fact that the system of issuing storm warnings was first suggested and carried out by himself at Utrecht, and not by Admiral Fitzroy in London, as is generally supposed in this country. He says, "I laid down my principles before the Dutch Academy of Sciences in October, 1857; in the *Comptes Rendus de l'Académie des Sciences de Paris* in November, 1857. I made my first proposal to the Dutch Government on the 14th of November, 1859, and the system was commenced on the 1st of June, 1860, or nine months sooner than in England. I communicated a paper on the subject to the British Association at the Newcastle meeting, and I wonder that my system has not been adopted in England, the more so because I recommended it to Mr. Francis Galton in August last. It is not only a right of priority that I claim; but I think my system is much better, though it gives no particulars as to rain or snow, but only the direction of the wind for the next twenty-four hours almost without fail, and the force or strength of the wind with great probability. *It is a fact above all doubt* that the wind that comes is nearly at right angles to the line between the places of highest and lowest barometer readings. The wind has the place of lowest height at its left hand, and is stronger in proportion as the difference of barometer readings is greater. If Portsmouth and Valentia rise much above Nairn, then I think you will have a gale from the west, and you have to attend to the oscillation of that difference of barometer readings. The only question is how many hours does that difference take place before the wind comes. Now, I propose to introduce an instrument that in all our ports will simply show the azimuth in which the greatest difference of barometer readings prevails, and the amount of that difference.

The sailor acquainted with the meaning of the signs will have to look at that instrument at the same time as the barometer. It is a new instrument that may be regarded as a differential barometer showing the state of the atmosphere in the surrounding countries.

"For many years weather signals after my method have been exhibited every morning at nine o'clock at Utrecht; the strength and direction of the wind are observed every hour and compared with the prognostics so that I could say how many times the rule has held good, and how many times it has failed. Why does not the British Government send a distinguished meteorologist to Utrecht in order to see what we have done? I do not pretend to have found the whole truth, and acknowledge that the rule may admit of exceptions in other countries; but in Holland we have had more experience of the prognostics of gales and storms than all other countries put together, for here all is reduced to measures and time; in England and France all is vague and in general expressions."

## MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 7th, 1867.

A. G. LATHAM, Esq., President of the Section,
in the Chair.

The PRESIDENT exhibited mounted specimens of Foraminifera from Dogs Bay, Roundstone, and from Berwick Bay, and he remarked on some differences in the character of the two deposits. He pointed out that in the Dogs Bay sand the prevailing forms are Truncatulina and Miliolina, while in the Berwick Bay dredging they are Dentalina and Biloculina. He also remarked on the great difference in the quality of the shell of Truncatulina from these two localities, those from Dogs Bay being delicate and hyaline, while those from Berwick Bay are opaque and very thick and strong.

The following paper, "On Polymorphina tubulosa," was read by Dr. ALCOCK.

In the course of examinations of the Dogs Bay sand, I have collected great numbers of detached branches of Polymorphina tubulosa, a form of foraminifer which is not likely ever to be found perfect in shore sand. I have, however, met with several fine specimens of it with only the tips of the branches broken away; but the most interesting examples are some which are more damaged, and show several structural features difficult, if not impossible, to be seen in perfect specimens. The main body of the shell of Polymorphina tubulosa has the form of Professor Williamson's P. communis, and appears to be identical with it, this form only, so far as I have seen, taking on the peculiar final development characteristic of P. tubulosa. It consists in the mature state of the rounded shell of P. communis more or less concealed by several covered passages commencing at the mouth and taking a direction towards the base of the shell. These passages have their arched walls developed into tubular prolongations, extending in all directions, and soon dividing irregularly into small branches, which, in one or two instances in the specimens shown, will be found to anastomose: they are either closed at their tips, as a small glass tube might be closed in the flame of a blowpipe, or they expand into little cauliflower-like excrescences, which are also apparently closed. The shell composing the parts just described is very delicate and thin compared with that forming the rounded nucleus, and its outer surface is frosted with small glassy projections of an irregularly squared figure, like imperfectly formed crystals. It is evident that this is a hastily deposited shell-covering on the sarcode developed since the last regular chamber of the shell was formed, and which, instead of collecting itself into a definite shape to produce a chamber similar to the others, had been surprised, as it were, while fully expanded by the calcifying process, which consequently gives us a petrified representation of the ordinary appearance of this external sarcode, with its pseudopodia protruded, the probable suddenness of the process being illustrated by the cauliflower excrescences which terminate many of the branches, and which have resulted from the contraction of the extremely fine terminal filaments of sarcode. It would appear that this is the final act in the life of the Polymorphina, its enfeebled vital power being insufficient to gather together the sarcode for the formation of another regular chamber, and therefore, properly speaking, the shell is fully formed and perfect before this last addition is made to it. There is evidence, however, in the specimens I have now to show, that the animal must have lived for a considerable time in a full-grown state before it thus terminated its existence, by producing a permanent likeness of its living self. These specimens have their arched coverings, with the branches proceeding from them, more or less broken away, so as to expose the floor beneath them, which consists of parts of the strong outer wall of the rounded nucleus, and which in all the cases examined presents the same peculiar appearance. It is riddled through with many large holes sometimes nearly circular but oftener oval or kidney-shaped, and so numerous as to open a very free communication between the external sarcode and that in the interior of the shell. It is not unusual to find Polymorphinas of a different type from these with a few small round holes in their outer walls, but they are scattered irregularly, are few in number, and have no evident relation either with one another or with any structural peculiarity of the animal; whereas in the present case they are invariably contained within the area of the floor of the covered passages, and are so numerous and encroach so much on each other that in some parts they leave only narrow isthmuses of the original shell-wall between them, and the larger holes have every appearance of having been formed by the union of several smaller ones. It is evident from a consideration of their character that they have been produced by the removal of shell-material previously deposited, and this gives them a physiological interest, for though it is natural to suppose that a creature which has the power of precipitating carbonate of lime on its surface would also have the power of removing portions of it by solution or absorption if required, the foraminifera are so structureless that we should hesitate to attribute to them this function without clear and positive proof.

In order to follow the successive changes in the latter part of the life of this Polymorphina, as they are illustrated in the specimens before you, the large rounded shells of P. communis should be first noticed, in which no opening is perceptible excepting the mouth, showing that at this stage the numerous large holes which are afterwards formed have no existence. The great thickness of the outer walls compared with that of the internal parts of the shell shows that the animal must have existed for a considerable time in this condition, during which the surface has been strengthened by repeated deposits of calcareous matter from its coating of external sarcode, and the smoothness and evenness of this surface shows that the coating was at that time spread uniformly over the whole of it. But broken specimens of P. tubulosa show that a change in the disposition of the external sarcode has been afterwards made, for in these it is found to have collected itself into two or three irregular bands, always commencing by one end at the mouth and extending towards the base of the shell, an arrangement clearly mapped out by the remains of its ultimately formed shell-covering, fragments of which are seen still attached to the surface of the smooth rounded nucleus.

The next event in the life of this Polymorphina is the formation of those numerous openings through the thick shell-walls, the observation of which in the specimens before you has chiefly led me to introduce them to your notice. These show, by their definite position and the evidence they give of their progressive formation, that when the external sarcode has once taken the form of bands it remains permanently in that state, and that these bands hold a fixed position on the parts of the shell where they were first placed. Among the specimens shown are some which only differ from ordinary shells of P. communis in being remarkably smooth on the surface and in having numerous large holes arranged in several rows radiating from the mouth towards the base of the shell, exactly as in undoubted specimens of P. tubulosa, but they are without the slightest trace of the external arched coverings and tubular branches. These might at first sight be set down as very much rolled and worn specimens of the ordinary P. tubulosa, but there is no evidence in the Dogs Bay sand of other kinds of foraminifera being worn to the extent which would be necessary to produce such a result, and the suggestion is uncalled for in this particular case since it is evident that at one part of the life of the animal its shell must have presented the appearance of these specimens, unless it could be admitted that the holes are formed after the production of the shell-covering on the expanded pseudopodia. But this last is clearly a single act, and its plan is evidently not such as would be adopted if the protection of sarcode were the object in view, the subdivision into many projecting branches most delicate and fragile at their points exposing it as much as possible to every injury; and therefore presenting a form and arrangement not at all likely to promote the comfort and convenience of the animal if it were to exist long in that state; and when to this we add that the pseudopodia, which are the means by which the foraminifera communicate with the external world, are sheathed by their shell-covering so as to be incapable of action, and moreover that every part of the animal becomes completely enclosed, the conclusion seems inevitable that this is not a condition in which it passes any considerable portion of its life, but that it is, as already suggested, merely the closing and final act. The holes through the thick shell however present a different history, they show by the quantity of shell-material removed and by the way in which separate holes have run together, that time has been spent in their formation, and they have also a clear and intelligible use in the economy of the animal, this being to open free communications between the internal and external sarcode. As to the process by which the shell-matter is removed it seems impossible under the circumstances to suppose it done in any other way than by absorption by the sarcode in contact with it. Among the specimens shown is one of P. tubulosa which has been completely broken open and shows that the process of absorption is not confined to the outer walls but that the inner partitions which at first formed parts of the walls of the separate chambers are also in great part removed, throwing the whole of the interior into one large irregular cavity.

The quantity of carbonate of lime deposited at once in the covering of the external sarcode and its pseudopodia is so considerable that some unusual source might naturally be looked for to supply it, and this is apparently found in the shell-material redissolved by the process just described, which must eventually lead to the sarcode being excessively charged with mineral matter and may be considered a sufficient reason for the final catastrophe; and if the view here given of the later stages of the life of Polymorphina tubulosa be correct it adds another point of interest by showing that the deposit of shell-material, in this one case at least, is more of a chemical than a vital act.

## Ordinary Meeting, February 5th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Commander Alexandre Cialdi, of Rome, and Dr. Edward Schönfeld, Director of the Mannheim Observatory, were elected Corresponding Members of the Society.

Mr. **BAXENDELL** read the following letter from Sir J. F. W. **HERSCHEL**, Bart., M.A., F.R.S., &c., dated Collingwood, February 2, 1867:—

"May I be permitted to offer a remark to the Lit. and Phil. Society on the very striking fact mentioned in Monsr. Buys Ballot's letter on storm warnings in the report of the last meeting of the Society ('Proceedings,' vol. vi. p. 83)—where, speaking of the gales of wind that accompany great barometric fluctuations he observes: '*It is a fact above all doubt* that the wind that comes is nearly at right angles to the line between the places of highest and lowest barometer readings. The wind has the point of lowest height on its left hand.'

"In my paper in 'Good Words' on the weather and weather prophets, published in January, 1864, (and written in November, 1863) the fact of this perpendicularity of direction of the wind to that of the wave's advance, in the case of the hurricanes which accompany the 'great November wave' is noticed; and in a few words added to the original notice of this peculiarity, in a reprint of that paper which has lately appeared ('Familiar Lectures on Scientific Subjects,' p. 144, note), an analogy is suggested between the fact in question and the transverse rotation of the etherial molecules in the propagation of a circularly polarized ray of light.

"The remarkable feature in question as it now appears from this statement of Monsr. Buys Ballot, is no special peculiarity of the November gales, but a general one. And as such, an origin has to be sought for it in the dynamical laws which connect the movements of the individual aerial particles (which constitutes the wind) with the direction and progress of the *advancing form* which constitutes the atmospheric wave.

"Weber, in his 'Wellenlehre,' has made us familiar with the conception of the propagation of a wave by the successive transfer from particle to particle of circular or elliptic movements. In the case of undulating liquids—to which his work chiefly refers, the plane of such circulation coincides with the *line of advance* of the wave, and thus are easily explained all the phænomena of breakers and (as I have since shown) of alternating tide currents. But there is nothing, dynamically speaking, to limit such circulating movements to that plane. It is equally conceivable that their plane may be obliquely situated: and that a wave *may* be propagated even in the extreme case of the perpendicularity of the two directions, the phœnomena of polarized light alluded to, demonstrate. I cannot help thinking then that a *general feature* of this nature being now so strikingly brought before us, there is something more than a mere resemblance in the phœnomena I have thus ventured to assimilate and that the attention of hydrodynamicians might be advantageously directed to the subject as likely to cast some additional light on the mechanism of storms.

"P.S.—Would it not be worth while to enquire whether the condition as to the wind having the barometric minimum (the trough of the wave) on its left hand is not reversed in the southern hemisphere?"

Mr. EDDOWES BOWMAN, M.A., exhibited and explained the construction and peculiarities of Hoffman's Polari-Microscope.

## PHYSICAL AND MATHEMATICAL SECTION.

January 31st, 1867.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

"Results of Raingauge, Anemometer, and Ozone Observations, made at Eccles, near Manchester, during the year 1866," by Thomas Mackereth, F.R.A.S., F.M.S.

The following results are intended as a sequence to those presented to this section last year. I regret, however, that on account of the removal of my observatory, some of the observations were interrupted during a period of three months. The observations to the end of April were made under the same circumstances as those of last year, an account of which will be found p. 107, vol. 5, of the Proceedings of this Society. The rainfall 3ft. from the ground continued to be measured under former circumstances till my removal was completed in August, when all the observations were resumed as before. The rain is measured from three different gauges. Two of the gauges stand close to each other in the same plane 3 feet from the ground, 75 feet from my house, and 21 feet from my astronomical observatory. The receivers of the rain gauges on the ground are of different shapes, one being round, 10 inches in diameter, the other 5 inches square, in both the edges are turned inward. The other gauge is now 34 feet from the ground and 4 feet above the ridge of my house—and free from every obstruction.—Up to April, at my residence, it was 32 feet from the ground. The receiver is a square one of the same size and shape as the 5-inch square one 3 feet from the ground.

First I represent the rainfall for 1866, as measured by the 10-inch gauge 3 feet above the ground, and 145 feet above the sea. Compared with the average fall for six years at Eccles:—

| Quarterly Periods. | | 1866. | Fall in Inches. | Average of 6 Years | Differences. | Quarterly Periods. | |
|---|---|---|---|---|---|---|---|
| 1865. | 1866. | | | | | 1865. | 1866. |
| Days | Days | | | | | | |
| 50 | 64 | January | 3·162 | 2·488 | +0·679 | 7·162 | 8·441 |
| | | February | 3·122 | 2·193 | +0·929 | | |
| | | March | 2·157 | 2·742 | —0·585 | | |
| 38 | 39 | April | 0·361 | 1·562 | —1·201 | 4·929 | 6·336 |
| | | May | 1·702 | 2·314 | —0·612 | | |
| | | June | 4·273 | 3·129 | +1·144 | | |
| 40 | 68 | July | 4·086 | 2·973 | +1·113 | 7·638 | 15·929 |
| | | August | 4·584 | 3·566 | +1·018 | | |
| | | September | 7·259 | 4·345 | +2·914 | | |
| 49 | 61 | October | 2·606 | 3·447 | −0·841 | 8·085 | 12·370 |
| | | November | 5·758 | 3·586 | +2·172 | | |
| | | December | 4·006 | 2·528 | +1·478 | | |
| 177 | 232 | | 43·076 | 34·868 | +8·208 | 27·809 | 43·076 |

The above table appears to add further confirmation to the remarks made on the rainfall at Old Trafford by Mr. G. V. Vernon, F.R.A.S., F.M.S. at the last meeting of this section. There is perhaps one thing worthy of notice, viz., that the average annual difference of rainfall between Old Trafford and Eccles for 6 years is about half an inch. This year the difference is not quite one tenth of an inch. The difference between the height of my former station above the sea and my present one is 27 feet. This probably may account for my having measured a trifle more rain during the last five months.

I now present the monthly amounts that fell into each gauge, and the number of miles of the horizontal movement of the air.

| 1866. | Rainfall in 10 inch round gauge, 3 feet from ground | Rainfall in 5 inch square gauge, 3 feet from ground. | Rainfall in 5 inch square gauge, 34 feet from ground. | Amount of horizontal movement of the air, in miles. |
|---|---|---|---|---|
| January ......... | 3·162 | 3·215 | 3·108 | 5,794 |
| February ...... | 3·122 | 3·079 | 3·056 | 4,664 |
| March ......... | 2·157 | 2·087 | 1·870 | 4,573 |
| April ........... | 0·361 | 0·299 | 0·307 | 5,833 |
| May ........... | 1·702 | 1·655 | ,, | ,, |
| June ........... | 4·273 | 4·284 | ,, | ,, |
| July ........... | 4·086 | 4·003 | ,, | ,, |
| August ......... | 4·584 | 4·521 | 4·060 | 5,333 |
| September ...... | 7·259 | 7·230 | 6·625 | 5,325 |
| October ......... | 2·606 | 2·585 | 2·314 | 3,759 |
| November ...... | 5·758 | 5·585 | 4·938 | 5,860 |
| December ...... | 4·006 | 3·922 | 3·012 | 5,753 |
| Totals ...... | 43·076 | 42·465 | | |
| Taking out May, June, and July, in 1st and 2nd columns—Totals are...... | 33·015 | 32·523 | 29·285 | 46,894 |

The next table shows the relative proportion of the winds for each month reduced to the four cardinal points, the number of days on which rain fell, together with the average amount received in each gauge.—

| 1866. | Relative Proportion of Winds to the Days of each Month. | | | | Days on which Rain fell. | Average Amount of Rain received in each Gauge. | | |
|---|---|---|---|---|---|---|---|---|
| | N. | E. | S. | W. | | 10 inch Round Gauge, 3 feet from Ground. | 5 inch Square Gauge, 3 feet from Ground. | 5 inch Square Gauge, 34 feet from Ground. |
| January...... | 3 | 2 | 14 | 12 | 20 | 0·158 | 0·160 | 0·155 |
| February ... | 7 | 3 | 9 | 9 | 22 | 0·142 | 0·139 | 0·138 |
| March ...... | 9 | 6 | 9 | 7 | 22 | 0·098 | 0·094 | 0·085 |
| April ......... | 8 | 10 | 7 | 5 | 9 | 0·040 | 0·033 | 0·034 |
| May ......... | 7 | 9 | 3 | 12 | 11 | 0·154 | 0·150 | ,, |
| June ......... | 4 | 8 | 9 | 9 | 19 | 0·224 | 0·225 | ,, |
| July ......... | 6 | 7 | 6 | 12 | 13 | 0·314 | 0·307 | ,, |
| August ...... | 10 | 4 | 4 | 13 | 26 | 0·175 | 0·173 | 0·156 |
| September... | 4 | 4 | 10 | 12 | 29 | 0·250 | 0·249 | 0·227 |
| October..... | 11 | 11 | 3 | 6 | 14 | 0·186 | 0·184 | 0·165 |
| November... | 7 | 2 | 5 | 16 | 22 | 0·261 | 0·253 | 0·224 |
| December ... | 6 | 3 | 8 | 14 | 25 | 0·160 | 0·156 | 0·120 |
| | 82 | 69 | 87 | 127 | 232 | 0·180 | 0·177 | 0·145 |

If the direction of the wind from the above table be compared with the monthly rainfall in the first table, it will be seen that the fall is always in excess when the west and south winds prevail over the north and east; and that if the average rainfall, and the number of days on which rain fell in each month, be compared with the general direction of the wind, it will be seen that the prevalence of the south and west winds, not only increases the amount of rainfall, but the number of wet days. The excess of rainfall on the ground over that at 34 feet from the ground appears also to be affected by the wind, for the excess on the ground increases when south and west winds prevail over the north and east. The only real exceptions being in the difference of rainfall 34 feet from the ground in March, and in the number of days in July. I present below a table of comparison illustrative of these remarks.—

| 1866. | Ratio of Winds. | Differences from the Average No. of Days of Rainfall. | Differences of Rainfall from the Average of 6 Years. | | Differences between Rainfall 3 feet from Ground, and 34 feet from Ground. |
|---|---|---|---|---|---|
| | N. and E. to S. and W. | | In 10 inch Round Gauge, 3 feet from Ground. | In 5 inch Square Gauge, 3 feet from Ground. | |
| January ...... | 5 to 26 | +3 | +0.679 | +0·732 | —·112 |
| February...... | 10 to 18 | +6 | +0·929 | +0·886 | —·028 |
| March ......... | 15 to 16 | +4 | −0·585 | +0·655 | —·217 |
| April ......... | 18 to 12 | —4 | —1·201 | —1·263 | —·008 |
| May ............ | 16 to 15 | —4 | —0·612 | —0·659 | „ |
| June ......... | 12 to 18 | +1 | +1·144 | +1·155 | „ |
| July ............ | 13 to 18 | −3 | +1·113 | +1·030 | „ |
| August......... | 14 to 17 | +6 | +1·018 | +0·955 | —·461 |
| September ... | 8 to 22 | +11 | +2·914 | +2·885 | —·605 |
| October ...... | 22 to 9 | —5 | —0·841 | —0·862 | —·271 |
| November ... | 9 to 21 | +5 | +2·172 | +1·999 | —·677 |
| December ... | 9 to 22 | +7 | +1·478 | +1·394 | —·910 |

The next table shows the average daily fall in each kind of gauge, when the velocity of the wind has ranged between the number of miles indicated in the first column.—

| Daily Movement of Wind. | 34 feet from the Ground. | 3 feet from the Ground. | |
|---|---|---|---|
| | 5 inch Square Gauge. | 5 inch Square Gauge. | 10 inch Round Gauge. |
| 0 to 50 miles | ·054 | ·050 | ·053 |
| 50 to 100 „ | ·088 | ·095 | ·098 |
| 100 to 150 „ | ·098 | ·107 | ·110 |
| 150 to 100 „ | ·191 | ·213 | ·217 |
| 200 to 250 „ | ·233 | ·250 | ·251 |
| 250 to 300 „ | ·150 | ·195 | ·202 |
| Above 300 „ | ·212 | ·228 | ·228 |

I have a few remarks respecting the large amount of ozone I now detect over the quantity I detected at my former station. My present station is only 27 feet higher than my former, and about one-third of a mile farther north. I was before about one-third of a mile from the most populous part of the village. I am now nearly two-thirds of a mile from it. I have used similar test-papers at both stations. I make my own tests, and have used those which were made at the same time at both stations. The average amount I used to detect at my former station from August to December, was = 0·79 on the scale of comparison. Last year from August to December, the amount was = 2·40 on the scale. Previous to my removal I tried a test-paper at each station. I found each comparison average a similar difference. The amount detected by Mr. Vernon, at Old Trafford, was usually a little in excess of mine for the months named; but the last year shows a very different result. The average amount detected by Mr. Vernon from August to December of last year (see the Manchester and Salford Sanitary Association's Weekly Returns), was 0·91 on the scale; showing a difference from my present station of 1·49 or nearly 1½, while the amount is 1·61 above the amount detected at my former station.

### Observations of the Occultation of Aldebaran by the Moon, January 16th, 1867.

By J. Baxendell, at Mr. Worthington's Observatory.

| | 53° 30′ 50·0″ N. | $8^{m.}$ 56·16$^{s.}$ W. |
|---|---|---|
| | Local Sid. Time. | Greenwich Mean Time. |
| Disappearance ........... | $2^{h.}$ $59^{m.}$ 6·1$^{s.}$ | $7^{h.}$ $25^{m.}$ 0·2$^{s.}$ |
| Reappearance ........... | 4 10 7·8 | 8 35 50·2 |

The disappearance and reappearance were both instantaneous, and no unusual phenomenon was observed.

The telescope used was the equatorially mounted achromatic of 70 inches focal length and 5 inches aperture with a power of 68.

By A. **Brothers**, at 110, Upper Brook Street, Manchester.

| | | |
|---|---|---|
| | 53° 27′ 56·0″ N. | $8^{m}$ $54{\cdot}38^{s}$ W. |
| Disappearance ............... | $2^{h}$ $59^{m}$ $6^{s}$ | Local Sid. Time. |
| Reappearance ............... | 4 10 13 | „ „ |

The disappearance was instantaneous. The reappearance was not so well observed, as the star must have emerged a few seconds before it was seen. The telescope is a refractor equatorially mounted, and in other respects identical with the one employed by Mr. Baxendell. Power used, 88.

"On the Elements of the Variable Star R Persei," by Joseph Baxendell, F.R.A.S.

In Professor Schönfeld's "Results of Observations of Variable Stars, &c.," read at the last meeting of the Section it is stated, "that the period of R Persei is about 205·5 days, and that one period must be added to the number hitherto assumed to have happened since the date of Bessel's observations." This value of the period is derived from the following four observed maxima compared with Bessel's single observation of the star, when at or near a maximum on the 6th of January, 1833.—

1865, March 12.
„ October 6.
1866, April 27.
„ November 20.

R Persei has been under pretty regular observation at Mr. Worthington's Observatory since the announcement of the discovery of its variability by Dr. Schönfeld in September, 1861, and a projection of all the observations has yielded the epochs of eight maxima, or double the number observed by Schönfeld, thus enabling me to determine with greater exactness the length of the mean period. These maxima are as follows:—

| | |
|---|---|
| 1861, October 16 | 8·7 mag. |
| 1862, November 21 | 8·9 „ |
| 1864, January 8 | 9·3 „ |
| „ August 20 | 8·7 „ |
| 1865, March 9 | 8·4 „ |
| „ October 1 | 8·7 „ |
| 1866, April 19 | 8·2 „ |
| „ November 14 | 9·0 „ |

Equating, and treating by the method of least squares, we have

Mean period = 206·8 days.
Mean epoch = 1864, August 10·7.

Calculating from these elements we find that a maximum ought to have occurred 1832, November 25, or 42 days earlier than Bessel's observation. As this difference considerably exceeds any that could be due to the form of the light curve at its maximum, it seems probable that the length of the period has slightly increased since the date of Bessel's observation.

The differences between the observed and the calculated times of maxima are:

| Observed—Calculated. | Observed—Calculated. |
|---|---|
| + 4·7 days | + 3·9 days |
| —7·9 „ | + 3·1 „ |
| —8·5 „ | —3·7 „ |
| + 9·7 „ | —1·5 „ |

The mean magnitude of the star at maximum appears from my observations to be 8·7. The magnitude at minimum is generally below 13.

Comparing Dr. Schönfeld's times of the last four maxima with my own, it will be seen that in every case his date is later than mine, the differences being

Schönfeld—Baxendell.
+ 3 days
+ 5 days
+ 8 days
+ 6 days

the mean difference being 5·5 days. At present I am unable to explain the cause of this difference, but hope that future observations specially directed to the point will clear up the mystery.

The next maximum of R Persei will be due 1867, June 10.

Mr. BAXENDELL read the following extract of a letter from George Knott, Esq., F.R.A.S., dated Woodcroft Observatory, January 24th, 1867, and exhibited the Light-curve to which it refers:—

"I have much pleasure in sending a copy of my light-curve of R Vulpeculæ for 1866. In forming the November maximum I have availed myself of a few observations kindly sent me by yourself. With the exception of a marked discordance at the end of the month our estimates agree very well, and show that the November maximum was an unusually bright one — brighter in fact than I have ever observed it before. But what I wish particularly to call your attention to, is the curious flattening of the ascending curve. I have noticed traces of this peculiarity in several instances in the case of this star, and cannot help regarding it as a real feature, though I should like to establish this view by further observation before laying too much stress upon it. You will notice, too, the curious flattening of the curve at the September minimum, which was a bright one."

## MAXIMA AND MINIMA OF R VULPECULÆ, 1866.

| Maxima. | | Minima. | |
|---|---|---|---|
| DATE. | MAG. | DATE. | MAG. |
| July 7·0 | 7·6. | May 2·5. | 13·2. |
| Nov. 15·5 | 7·1. | Sept. 13·5. | 12·3. |

## Ordinary Meeting, February 19th, 1867.

J. P. JOULE, LL.D., F.R.S., &c., Vice-President, in the Chair.

Mr. **SIDEBOTHAM** inquired whether any member was acquainted with the particulars of a remarkable discovery in connection with the conversion of dynamical into electrical force, by M. Siemens and Dr. Wheatstone, a brief notice of which appeared in the *Athenæum* of Feb. 16.

Mr. **WILDE** said, that he happened to be present at the meeting of the Royal Society on the 14th inst., when M. Siemens' interesting paper "On the Conversion of Dynamical into Electrical Force, without the aid of Permanent Magnetism," was read. The apparatus used in his experiments, and exhibited at the meeting, consisted of two electro-magnets, coiled with long and thin wires, and between the poles of these electro-magnets an armature, also coiled with long and thin wires, was made to revolve rapidly by means of pulleys.

When a voltaic battery was connected with the wires of the electro-magnet for a few moments, and afterwards disconnected, the residual, or *permanent*, magnetism of the electro-magnets was sufficiently strong to generate a feeble current of electricity in the wire of the revolving armature. The feeble current thus generated was then transmitted through the coils of its own exciting electro-magnet, instead of the current from the battery, and by the mutual action and reaction of the electro-magnet and the armature upon each other, the magnetism of the machine could be exalted to such a degree as to require the application of very considerable power to drive the apparatus.

That this increase of magnetism was attended by an increase of dynamic electricity in the wires surrounding the armature and electro-magnet was manifest from the briliancy of the spark at the commutator and the increased deflection of a galvanometer needle placed within the influence of the electro-magnetic circuit; but the quantity of electricity was not sufficient to ignite wires; and was comparatively small in proportion to the mechanical force expended in producing it.

The apparatus used by Dr. Wheatstone in his experiments, which was also shown in operation at the meeting, was very similar in construction, and identical in principle, with that of Siemens', though each of these experimentalists appears to have worked out his results independently.

Instead of the long and thin wire used by Siemens in his machine, Wheatstone employed coils of thicker wires, and the wire on the electro-magnet was of the same diameter, and about eight times the length of that on the armature. The coils of the electro-magnet had consequently about eight times the resistance of the armature circuit. In this arrangement, the wire surrounding the armature being the generating circuit, it follows from a well-known property of the electric circuit, that the amount of dynamic, or useful effect, which the current is capable of exerting when the resistance of the coils of the electro-magnets is in circuit, is about one-ninth of the total amount which the armature coil is capable of evolving when the electro-magnet is excited to the same degree by a separate battery or other electromotor.

This peculiarity of the machine was made evident by Dr. Wheatstone in a very striking manner by means of the following well-devised experiment:—Two branch wires were led off from the extremities of the armature coil, and when the electro-magnet was fully excited the free ends of the branch wires were bridged across by means of a fine platinum wire about nine inches in length, which wire immediately became white-hot, and as suddenly ceased to be visibly hot until contact with the branch wires had been broken for a short time and afterwards re-established.

This momentary ignition of the platinum wire appeared to Mr. Wilde to be due to the diversion of the greater portion of the current generated by the armature into the new channel or short circuit established by the platinum wire through the armature coils, before the high degree of magnetism which had been maintained in the electro-magnet by the full current from the armature had been reduced by the loss of a portion of the current so diverted.

The results obtained by Siemens and Wheatstone are undoubtedly very interesting from a scientific point of view, but, practically, the exciting of the electro-magnet by the current from the armature used in connection with it, produces results very inferior to what are obtained from similar machines, the electro-magnets of which are excited by a separate source of electricity of small power, as, in the latter case, the full force of the current generated by the armature coil is available for the performance of external work.

Mr. Wilde having himself, some time ago, made some experiments similar to those of M. Siemens and Dr. Wheatstone, came to the conclusion that in the present state of our electrical knowledge, the difficulty of utilising the current from the armature after it had passed through the coils of the electro-magnet was insuperable; as it is absolutely necessary that the coils of the electro-magnets of electro-magnetic machines, when excited by intermittent currents, should be of considerable length, in order that they may acquire and retain a charge of electricity or magnetism, as has been shown by Drs. Henry and Faraday in their experiments on the induction of a current upon itself. On the other hand, for the production of powerful electro-dynamic effects, it is necessary that the wire of the armature coil should be comparatively short and thick. Hence the incompatibility of the armature and electro-magnetic circuits, when exalted dynamic effects, outside the machine, are required.

It is not a little interesting to observe how, in the progress of physical science, the same train of reasoning is often pursued by several individuals having no communication with one another. In the present instance, it would appear that the "augmentation of the power of a magnet by induction currents produced thereby, and reacting upon the magnet itself," has also suggested itself to other experimentalists besides M. Siemens and Dr. Wheatstone. In a letter published in *The Engineer* of July 20th, 1866, p. 42, the writer (Mr. J. Murray) says:—"The description in recent numbers of *The Engineer* of Mr. Wilde's admirable improvements in the magneto-electric machine, induces me to point out, if you will permit it through your columns, a variety of the principles embodied in his machine, which is so obvious that it cannot fail to be hit upon by some inventor before long."

It is briefly this:—"Whereas Mr. Wilde, beginning with an ordinary magneto-electric machine, uses the current obtained from it to charge a powerful electro-magnet, and from this obtains a second and more powerful current, which, used in like manner, produces one still more intense, I, using only a single machine, pass the current obtained from its armatures through wires coiled round the permanent magnets, in such a direction as to intensify their magnetism, which, in its turn, reacts upon the armatures and intensifies the current.

"It is obvious that the amount of mechanical force which can thus be converted into electricity is only limited by the capacity of the iron for magnetism; just as the power of a steam engine is only limited by the capacity of the boiler for absorbing heat."

Mr. Wilde has also received a letter on the subject of his recent experiments in magnetism from Mr. Moses G. Farmer, of Salem, Mass., United States, dated November 9th, 1866, in which he says that he had obtained an increase of 31 per cent. in the power of a magneto-electric machine, by transmitting the current from the armature through coils of wire surrounding pieces of soft iron forming the prolonged extremities of the permanent magnets of the machine. Mr. Farmer, in the same letter, adds:—"I have built a small machine in which a current from the thermo battery excites the electro-magnet of your machine to start it, and after the machine is in action, a branch from the current of the magnets passes through its own electro-magnet, and this supplies the magnetism required. It is not exactly like a person standing in a basket and trying to lift himself—because the electricity proceeds from the conversion of the mechanical energy, which must be continually supplied. Neither can it in any wise be likened to the various schemes for producing perpetual motion; but depends on the principle, that the actual energy of the mechanical force, conjointly with the potential energy of the magnet, can develope a greater amount of potential energy than is originally resident in the magnet, or, in other words, it is a method of converting part of the actual energy of the prime mover into the potential energy of magnetism."

Mr. W. L. DICKINSON, having discovered an error in his calculation of the Occultation of Aldebaran (see Proceedings, December 11th, 1866, page 47), communicated the following results of the corrected computation.

Calculation for the Observatory of Robert Worthington, Esq., F.R.A.S., Crumpsall, near Manchester, Lat. 53° 30′ 50·0″ N., Long. $0^h\ 8^m\ 56{\cdot}16^s$ W.

The Occultation of Aldebaran by the Moon, January 16, 1867:—

| | Sidereal Time at Observatory. | Mean Time at Observatory. | Mean Time at Greenwich. | Angle from | |
|---|---|---|---|---|---|
| | | | | N.Point. | Vertex. |
| Disappearance... | $2^h 59^m 13^s$ | $7^h 16^m 10^s$ | $7^h 25^m 6^s$ | 100° | 82° |
| Reappearance ... | $4^h 10^m 5^s$ | $8^h 26^m 50^s$ | $8^h 35^m 46^s$ | 282° | 280° |

The angles are reckoned towards the right hand round the circumference of the Moon's image as seen in an inverting telescope.

Mr. BAXENDELL read the following letter, addressed by T. H. Babington, Esq., to G. V. Vernon, F.R.A.S., and dated Langley, 13th Feb., 1867:—

You have been kind enough to send me a copy of a letter recently received by yourself from Dr. Buys Ballot, of Utrecht, in which the writer claims for himself the origination of the system of issuing storm warnings.

He says, "I laid down my principles before the Dutch Academy of Sciences in October, 1857; in the *Comptes Rendus de l'Académie des Sciences de Paris* in November, 1857. I made my first proposal to the Dutch Government on the 14th of November, 1859, and the system was commenced on the 1st of June, 1860, or nine months sooner than in England."

It is no doubt the fact that Admiral Fitzroy did not give his first "warning" before the 5th of February, 1861, but he had been *for years* engaged in organising the service and maturing his plans.

I feel it therefore incumbent upon me, in justice to the memory of my late chief, to offer a few remarks upon that portion of Dr. B. Ballot's letter which I have quoted.

What the nature of the system of warnings which Dr. B. Ballot claims to have introduced at Utrecht in 1860 may have been I do not know,—but Holland must, at that period, have been mainly or entirely dependent upon observations made at her own stations, that is, over an area *not larger than Wales,* and forming part of a great continent.

It will be clear to you and to every meteorologist that forecasts founded upon barometrical differences over an area so limited, and geographically so situated, could not be of much practical value,—and that for purposes of forecasting, Dr. B. Ballot could, at that period, have had but little advantage over any individual and isolated observer.

But however that may be, I will, with your permission, relate as simply as possible what I know with respect to the origination of the storm-signals — observing that any knowledge I may possess on the subject is derived from the fact that I was Admiral Fitzroy's assistant from the first establishment of the Meteorological Department in January, 1855, that I was in daily personal communication with him, and I believe, saw the whole of his correspondence.

By continued and consecutive series of charts constructed on the synoptic or synchronous principle, during the winter of 1856-57, it appeared to Admiral Fitzroy that a light was thrown on the atmospheric changes over the British Isles and their vicinity which had been unattainable previously. Those charts were prepared by myself under Admiral Fitzroy's direction, with the express view (as I know from frequent conversations with him on the subject) of gaining by their intercomparison an insight into the laws of our atmosphere which might enable us to know what weather was likely to prevail during the next two or three days, and, as a corollary, when a storm was likely to occur. Those charts however were not found sufficient by themselves, and led to subsequent arrangements and correspondence.

In the Report of the Meteorological Department in March, 1857, Admiral Fitzroy wrote — "It has been desired that a great many observations should be compared throughout the British Isles (with their neighbouring coasts and seas) at certain remarkable periods, to obtain the means of delineating or mapping the atmosphere at successive times; and *thence to deduce the order of those changes of wind and weather* which affect navigation and fisheries especially."

In September, 1859—or *two months before* Dr. Buys Ballot appears to have made his first proposal on the subject to the Dutch Government—the British Association for the Advancement of Science met at Aberdeen under the presidency of the Prince Consort, and, the question being brought before them, it was then resolved by their Council that application should be made to Her Majesty's Government for an organisation and trial of a plan by which the approach of storms might be telegraphed to distant localities.

At two meetings in Buckingham Palace early the following year (1860), minutes were authorised on this subject, and correspondence ensued which resulted in establishing a telegraphic communication of meteorological facts between twenty home stations, besides foreign ones.

When the British Association met at Oxford in July, 1860, a paper was read by Admiral Fitzroy on the measures proposed for meteorological telegraphy, which obtained approval without eliciting any opposition.

Advancing gradually, the first cautionary or storm-warning signals were made early in 1861—on the 5th and 6th of February.

In further confirmation of the hitherto undisputed fact that it is to the zeal and energy of Admiral Fitzroy (and those only who had the advantage of his intimate acquaintance know how great and vexatious was the opposition that his plans encountered from certain quarters) that England, and to a great extent Europe generally, is or was indebted for her system of cautionary signals, I will merely quote the following paragraph from Sir Henry James's "Instructions for taking Meteorological Observations," *published in 1860.*

Sir Henry James observes—"It is unnecessary to point out the vast importance of being able to foretell the advent of a storm many hours before it could arrive at any of our ports, and Admiral Fitzroy, impressed with the idea that this can be done by the aid of the telegraph, has *for some years past* urged upon the Government the desirability of establishing telegraphic communications daily between our most distant ports, and especially from those in the south of Ireland."

I have no desire to detract in any way from the important services rendered to meteorology by Dr. B. Ballot. When, however, Admiral Fitzroy at length succeeded in extorting from those in authority a reluctant consent to the issue of storm warnings in England, he was distinctly told that the cautions must be given on his sole responsibility and that upon him must rest the responsibility of their failure. He did not shrink from that responsibility. The storm signals are generally admitted to have been a great public benefit. Had it been otherwise, upon Admiral Fitzroy would have fallen the *dis*credit which usually attaches to failure. It behoves his friends, therefore, to take care that his name be not deprived of any of the credit which belongs to success.

Mr. Baxendell explained the construction and mode of using the weather signal recently invented by Dr. Buys Ballot, and also stated that the Doctor had lately found that the discovery of the fact that the direction of the wind was generally at right angles to that of a line joining the areas of high and low barometer was due to Dr. Lloyd of Dublin; but he still claimed to have been the first to make practical application of this discovery.

Dr. Joule, F.R.S., suggested the desirability of adding to Dr. Ballot's instrument an arrow fixed in a direction perpendicular to the plane of the indicating board, with the view to prevent the possibility of any mistake being made as to the direction in which the wind might be expected to come.

Mr. **Baxendell**, referring to the statement made by the President of the Board of Trade in the House of Commons on the 15th instant, in reply to the questions put by Colonel Sykes respecting storm signals, remarked that a concession had certainly been made by the Government, but unfortunately it had this very objectionable feature, that while it would be far less useful than the thing asked for, it would involve a very much greater current expense. "The information which was collected daily by the scientific committee would be telegraphed as heretofore to the different ports and stations throughout the kingdom in time to allow of forecasts to be made in any place where there was a disposition to make them." But the cost of daily telegraphing all this information to the various outports and stations would obviously vastly exceed that of occasionally transmitting telegraphic notices of approaching storms; and moreover, it seems never to have occurred to the scientific committee nor to the Board of Trade that it would require scientific meteorologists at all the ports and stations to interpret the indications afforded by this crude and undigested information, men, in fact, with ability to do that which the scientific committee shrink from attempting themselves. But such men are not to be found at many of the outports, and especially at the fishing stations; and even if they were, it is not to be expected that they would voluntarily undertake onerous duties which ought to be discharged by a competent public officer appointed by the Board of Trade. The scientific committee are unwilling to issue storm warnings because they are not prepared with a system of forecasting founded on a strictly scientific basis; but this is no valid reason for allowing them to abolish the late system, which was working so well for the interests of science, commerce, and navigation, until they are prepared with something better to replace it. And it must be borne in mind that it will not in the end conduce to the interests of science, if the public find that a body of scientific men, invested with an unusual power, are either unable or unwilling to use it for the public good.

"On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes," Part III., by JAMES NASMYTH, Esq., C. E., Corresponding Member of the Society.

The author in this part of his paper describes in considerable detail the methods he employs in casting, grinding, polishing, and giving perfectly flat surfaces to the plane mirrors of reflecting telescopes.

## PHOTOGRAPHICAL SECTION.

February 20th, 1867.

Dr. J. P. JOULE, F.R.S., &c., Vice-President of the Section, in the Chair.

"On the Reversed Action of Light in Photography," by JOSEPH SIDEBOTHAM, Esq.

Those who have for many years been engaged in photographic pursuits, either as amateurs or professionals, have (especially when trying some new process) met occasionally with very curious results—failures as photographic pictures—but of the greatest value as remarkable results of the effects of light. These were probably much more frequent in the early days of photography, when we were dependent in a great measure on our own manufacture of chemicals, even to the preparation of æther. Unfortunately but few of these curiosities, as we may call them, have been preserved; in the desire to produce good pictures these have been thrown aside, and too often the causes of the strange results have not been ascertained. This is the more to be regretted as discoveries of great value may be hidden under these so called failures. On a former occasion I exhibited here a collodion positive, on glass, of a landscape, with the natural colours, red, green, and blue, very fairly represented, although much duller than in nature. This was an over exposed picture, and not noticed particularly until a day or two after it was taken, and all my subsequent attempts were unsuccessful to obtain another similar picture.

It is well known that when plates are over exposed in the camera the action of light becomes reversed; thus, a sky which ought to be black becomes transparent, and a print from this looks somewhat like a negative on paper. Still the action is by no means regular, and there are many points connected with this action which require careful investigation.

It is my intention to bring some of these before you this evening in a brief manner, and to exhibit some curious specimens in illustration.

All photographers must have noticed now and then where two lines of light and dark meet, that the light is rendered more intense just at the edge of the black or dark object; in portraits of gentlemen this is sometimes very prominent, the dark figure is thrown up as intended from the light background, but the latter is so much more intense just round the figure that it has sometimes the appearance of light streaming from behind. This effect is sometimes seen in landscapes — round a tower or even a tree projected against the sky runs a line of intense white much deeper than the sky itself. On the other hand it sometimes happens that in a landscape with mountains the edge against the sky seems to be robbed as it were of a portion of its intensity, and so when printed exhibits a dark edge.

Again, it is well known that the sky, seen through small openings in trees or small windows in a ruin, is much more intense in a negative than the great mass of the sky; also that in taking an interior view, where light streams through a window in front of the camera, the effect produced on the negative is as if the light came round, illuminating to some extent the inside, and gradually as it were eating away the tracery of the window.

In some cases where the bright sky is seen through an opening of the trees, the trees and branches around the opening are converted in the photograph into pure white tracery on a darkened sky, and the remainder of the picture quite natural; in some cases again, one half of a photograph may be correct as a positive, and the other, the distance exactly like a paper negative. I bring with me for exhibition specimens of these several effects, which appear intimately connected together, and deserving of our attention.

The question for us to consider is whether these effects are to be accounted for altogether by the received theories of the reversed action of light on iodized plates or paper when exposed too long in the camera, in other words, what used to be called solarization, or whether to some extent, at least, these are not copies of the natural effects as the camera sees them, and as we should see them, if we looked aright.

In Tyndall's work on the glaciers of the Alps is a reference to a curious phenomenon he saw, where the trees and shrubs projected against the sky showed as brilliant white, he says this phenomenon is rarely seen by guides or travellers, perhaps because they avoid looking towards the brilliant sky; he also quotes a letter from Professor Necher to Sir David Brewster, describing the same phenomenon. He says he has seen it at different times of the day, and says that the appearance of the trees projected against the sky is as if they were made most delicately of the purest silver.—At pages 180 and 181 are sketches, which, as you will see, are wonderfully like the photographs I have to show.

That some of these effects can be accounted for I am well aware, but when taken together there is much that is inexplicable, the explanation for one case will not suit another. I would, therefore, make the suggestion, that in some states of the atmosphere, these bright lines are to be found in nature, either visible, or of such actinic power as to produce the results you see; there are certain effects in nature, for instance, the streaming light of the sun from behind clouds, of which the camera takes no notice, and on the other hand, there may be effects in nature invisible to us, but which act like the invisible rays of the spectrum with great chemical intensity.

The following photographs were exhibited to illustrate the above remarks:—

No. 1. Showing white lines round figures, and chair in carte de visite portrait.

No. 2. Showing white line round church tower and yew tree.

No. 3. Showing white line faintly above the hill, also the increased depth of shade at the extreme top of it.

No. 4. Showing intense white line dividing landscape from sky, also the railings on bridge against the sky showing intense white, whilst those against the trees are black.

No. 5. Curious compound of positive and negative, the house, road, trees, &c., in foreground being positive, and correct, the sky black, and the distant trees and buildings white.

No. 6. Bridge and stream, Fountains Abbey. Showing the foliage around the opening to sky traced in pure white, the other part of photograph being natural.

No. 7. Showing the blurring of dark tracery when light streams through a window.

No. 8. Showing one of the distant hills reversed, appearing light against a dark sky, the other mountains appearing natural.

Mr. Dancer remarked that several years since he had attempted to explain the marginal appearances which some photographic negatives exhibit, by supposing that the force which governed the deposit forming the picture was more energetic at the abrupt boundary line between a light and dark object than on other parts of the exposed sensitive plate where there was less contrast, this intensity of force bearing some analogy to that which is observed on the edges of bodies influenced by magnetic, electric, or chemical action. More recent observations had induced him to alter this opinion. He thought the phenomena were caused by interference of light accompanied by diffraction fringes of colour produced by the projection of a dark body against a bright sky. The actinic effect of these fringes might possibly be increased or decreased according to the amount of chromatic correction of the camera lens for the primary and secondary spectra. Mr. Dancer stated he had frequently noticed the reversed action of light from over exposure; with the old Daguerreotype process he had obtained very perfect negative instead of positive pictures. On one occasion, in 1840, he accidentally exposed an iodized Daguerreotype plate in the camera much longer than the usual time; in this picture the natural colours of the objects were brought out in a marked degree. This picture he exhibited to many friends, but from not being fixed it faded after some time. Subsequent attempts on the same objects failed in obtaining the natural colours.

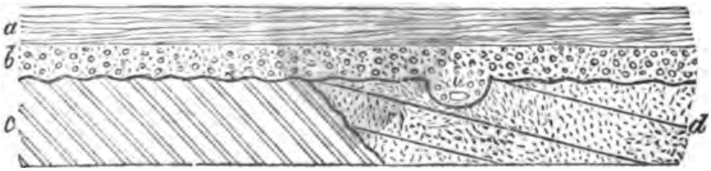
## Ordinary Meeting, March 5th, 1867.

J. P. JOULE, LL.D., F.R.S., &c., Vice-President, in the Chair.

Mr. BINNEY, F.R.S., F.G.S., said that some time since, by the kindness of one of our members (Mr. W. Mellor), he had been enabled to see an interesting section of the drift and the underlying triassic and coal measures at Ardwick. The following beds were met with in the descending order:—

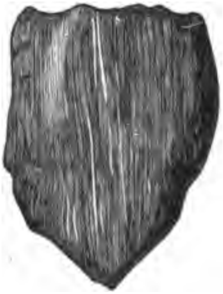
| | Ft. | In. |
|---|---|---|
| Till, or Boulder Clay, about ............ | 45 | 0 |
| Lower Gravel.............................. | 2 | 6 |
| Lower Soft Sandstone (Trias) ......... | 0 | 0 |
| Upper Coal Measures .................. | 0 | 0 |

The accompanying wood cut will show the position of the Strata:—



*a* Till or Boulder Clay.
*b* Sand and Gravel.
*c* Upper Coal Measures.
*b′* Circular hole in the Sandstone filled with Sand and Gravel.
*d* Lower Soft Sandstone (Trias).

The gravel under the Till was quite dry, and when it was removed by tunnelling in the sandstone so as to let the whole of it fall down, the under surface of the Till showed a polished appearance, and was as smooth and even as a plastered ceiling. It was composed of granitic, trappean, silurian, and carboniferous rocks, many of the latter being of Spirorbis limestone, all more or less rounded and mixed with a reddish-coloured sand. The thickness of it was 2 feet 6 inches, and it rested on an under surface of upper coal measures and lower soft sandstone, which bore conclusive evidence of erosion and long exposure to aqueous and atmospheric agencies. In the sandstone was a singular excavation, of a basin shape, about 2 feet deep and 2 feet 10 inches in diameter, and marked *b′* in the wood cut, filled with rounded pebbles and sand. One of the former was larger than the rest, and composed of Spirorbis limestone found in the neighbouring upper coal measures, and was of a pointed form, somewhat like the cutter of a planing machine, and rounded, polished, or striated on all sides but one. Its weight was sixteen pounds, and its form is shewn in the accompanying wood cut, about $\frac{1}{36}$th the size of the original:—



The surfaces of rocks under the drift is generally interesting to geologists, and is now attracting considerable attention. How the hole above described was formed it is very difficult to say. It might have been made by an eddy of water carrying the stones round, and thus excavating the rock, but in that case we should have expected to find all the pebbles very nearly of a similar shape and well rounded, which is not the fact. The form of the stone herein figured, as previously stated, reminds us of a cutter, and if it were fixed at the bottom of a mass of ice moving round in water it would very soon form a hole like that above described.

He (Mr. Binney) wished geologists to examine carefully all the surfaces of rocks *in situ* under the Till, especially those containing hollows with gravel or stones in them, which might come under their observation, and not only record the forms of the holes, but also the shape of the stones found in them.

"On some of the Conditions of Molecular Action," by ARTHUR RANSOME, M.B., M.A., Cantab.

The scope of this paper was restricted to the action of different substances upon one another through the intervention of purely molecular forces or affinities.

The following propositions probably include what is known of molecular force:

1. That it depends essentially upon the elementary nature of bodies.
2. That it acts without regard to *mass.*
3. That it increases inversely as the distance at some enormous ratio.
4. That its action is affected by calorific, electric, and probably by luminous vibrations, and by chemical affinity.
5. That it does not seem to differ, in kind, from the attraction of cohesion, which binds together the particles of the same substance.
6. That chemical affinity may be similar in kind, but that it differs from molecular attraction in the power of producing *combination.*

When molecular operations take place between the particles of bodies of dissimilar chemical composition, the following conditions are found to favour the production of molecular and chemical changes:

(A) That two or more of the substances submitted to molecular influences have a more or less powerful attraction for one another.

(B) That their physical condition is favourable to molecular action.

(C) That the molecular agent or "catalyte" has very low *chemical* affinities for the substances acted upon.

(D) That the molecules of the catalyte are free.

Each of these conditions were considered separately in their bearing upon different molecular actions.

(A) *Of Molecular Affinity.* All substances may possess absolutely the same amount of molecular force, but from the differences in the degree of approximation of their particles, from their varying chemical affinities, and from the various extent to which they are subject to the different affections of matter, they may not be able equally to exert their molecular power.

The following classes of actions were noticed as instances of molecular affinity, and the mode in which it acts was pointed out:—

1. *Attraction of solids for liquids,* including capillary attraction; the rise of liquids in porous substances, and upon the surfaces of solids; the formation of drops; and some of the phenomena of cementation by means of colloid substances.
2. *Mixtures of fluids.*—It was pointed out that in order that two fluids should be entirely miscible, it is necessary that the particles of each fluid should have for those of the other a greater molecular affinity than they have amongst themselves.
3. *The attractions of vapours and gases* were illustrated by the distillation and diffusion of mixed vapours and gases.
4. *The solution of solids* was supposed to be due to molecular affinity, causing disintegration and liqui-

faction before mixation; and the influence of heat upon the action was considered.

5. *In catalytic actions* the force sometimes produces decomposition, at others synthesis. When contact produces decomposition, as when binoxide of manganese facilitates the evolution of oxygen from chlorate of potash, the molecular affinity of the catalyte is probably greatest for one of the constituents of the substance acted upon, and it attracts this to such an extent as to enable it to gain its freedom. When synthesis results, as in the oxidation of alcohol by means of platinum black, the catalyte draws the particles together, or it may be able to concentrate upon itself one of the elements so exclusively that it may appear in an undiluted or liquid form.
6. The phenomena of osmose were fully investigated, and were referred to the mutual attractions of the two fluids and of the septum itself.
7. Dialysis probably differs from osmose, in consequence of the peculiar molecular properties of colloid substances.
8. *The attraction of colloids, and of heated metals for gases and vapours,* was illustrated by the recent researches of Dr. Graham and Messrs. Dumas and St. Clair Deville.

(B.) *Molecular conditions favourable to catalysis.* Since molecular attraction is independent of mass, and since its power is only exerted at very small distances, it is evident that molecular attraction can only be exerted at the surfaces of the acting body, the second stratum of molecules will scarcely, if at all, affect substances lying on the surface; hence a large extent of surface is an important aid to molecular actions. Points of matter possess still greater advantages from their power of undisturbed action. These remarks were attested by observations upon catalytes, by the power possessed by points of facilitating vaporisation and crystallisation.

(C.) *Low chemical affinities of catalytes.* When it is considered that the molecular forces exerted by a catalyte can only be employed when the substances to be acted upon are brought into very close contact with it, so close that if these substances had strong chemical affinities for one another they would combine; and when, further, it is considered that if the catalyte united chemically with other material it would soon cease to exist as such and would lose its catalytic power, it becomes obvious that catalytes must be chemically indifferent to the substances they act upon, and not only is this the case, but all the most energetic catalytes display chemical indifference not only towards those substances which they affect, but also towards ordinary chemical reagents; instances of this fact were found in the catalytic properties amongst metalloids, of charcoal, glass, and sand; amongst metals, of platinum, and others which are least affected by oxygen. The researches of Dr. Charles Henry and Mr. J. A. Ransome, and of the Rev. J. Eyre Ashby, upon the catalytic powers of metals and their oxides, were quoted, and the conclusion was drawn that it is not those oxides which have an excess of oxygen, nor yet those which most readily part with it, but it is the sesquioxides whose influence is most felt. It was also pointed out that ferments, the most remarkable substances in the organic kingdom for their catalytic power, belong universally to the class of *colloid* bodies, one of whose chief characteristics is that of "chemical indifference."

(D) The molecules of the catalyte must be free from any stain of vapour or any other substance which might partially impede its molecular action. The condensation of vapour upon solids had been noticed by Professor Magnus and Messrs. Janion and Bertrand, and the importance of cleansing catalytes by heat or other means was pointed out; also the effect of acids upon alloys and the energy of nascent substances was ascribed in part to their freedom from all taint of other matter.

Catalytic bodies were shown to fulfil all these conditions, but it was pointed out that many forms of so-called catalysis were complex, not simple processes.

The application of these facts to physiology was remarked —the presence in the living body of substances whose chemical reactions are continually modified by variations in their molecular condition, and by which energy was stored up until it is required, and by which, probably, fermentative changes are produced.

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## Observations of the Eclipse of the Sun, March 6th, 1867.

By Mr. BAXENDELL, at Mr. Worthington's Observatory. 53° 30′ 50·0″ N. $8^m\ 56{\cdot}16^s$ W.

First contact ... ........ $8^h\ 20^m\ 16{\cdot}5^s$ a.m. G.M.T.
Last contact ........... $10^h\ 50^m\ 21{\cdot}9^s$ „ „

About a minute before the time of first contact two or three small clouds passed over the sun, and its limb, which had been tolerably steady and well defined, became excessively tremulous, and continued so till after the contact had taken place. The time given may therefore be uncertain to the extent of *two* or even *three* seconds. At the end of the eclipse the limb of the sun was very steady, and the time of last contact was observed very satisfactorily. Only one small *black* spot and two or three minute ones of a penumbral character were visible on the sun's disc; but a fine group of faculæ near the following limb formed a very interesting object for examination during the early part of the progress of the eclipse.

By Mr. Brothers, F.R.A.S., at Upper Brook Street. 53° 27′ 56·0″ N. $8^m\ 54{\cdot}38^s$ W.

First contact ........... $8^h\ 20^m\ 15{\cdot}4^s$ a.m. G.M.T.
Last contact ........... $10^h\ 50^m\ 20{\cdot}7^s$ „ „

By Mr. Mackereth, F.R.A.S., at Eccles. 53° 29′ 12″ N. $9^m\ 21{\cdot}5^s$ W.

First contact ........... $8^h\ 20^m\ 15^s$ a.m. G.M.T.
Last contact ........... $10^h\ 50^m\ 10^s$ „ „

## Ordinary Meeting, March 19th, 1867.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

"Observations of the Eclipse of the Sun, March 6th, 1867," by J. B. Dancer, F.R.A.S.

Observatory, Old Manor House, Ardwick, Lat. 53° 28′ 23″ N., Lon. 8m 53·93s W.

First contact, not timed.

| | | | | |
|---|---|---|---|---|
| Contact with elongated black spot...... | $8^h$ | $43^m$ | $36^s$ | G. M. Time. |
| Last contact of moon's limb ............ | 10 | 50 | 17·5 | „ |

Just before the first contact my attention was unfortunately taken from the telescope, and when I got to the instrument again the eclipse had commenced.

The weather was very favourable and the atmosphere remarkably steady; clouds occasionally passed over the sun but did not obscure the disc for any length of time.

The telescope used was 10 feet 6 inches focal length and 7¼ inches diameter. The full aperture of the object glass was employed, with a power of 50, and using a diagonal glass sun prism numerous groups of faculæ were visible, but no maculæ excepting the dark spot named above. The behaviour of this spot interested me, and as it may have been noticed by other observers I will describe it. I have already stated that the atmosphere was very steady, indeed I have seldom seen the edge of the sun so free from all disturbance. When the moon's edge approached the dark spot it suddenly became tremulous and lighter in colour before it disappeared behind the body of the moon; there was no visible cloud passing at the time and the sun's edge was sharply defined and perfectly steady.

During the progress of the eclipse the serrated edge of the moon was beautifully distinct, powers from 50 to 320 were used to look at the prominences on the moon's edge. I had several telescopes directed to the sun; some of these had 4½ inches and 5½ inches diameter object glasses. The value of aperture in observing the edge of the moon and the faculæ on the sun's disc was very manifest when using the 7¼ inch diameter glass.

The temperature in the sun during the eclipse was taken at intervals with a very delicate black bulb mercurial thermometer. The record shows that the increase of temperature due to the increasing altitude of the sun was greater than that of the opposite effect caused by the obscuration of the disc until nearly the time of the greatest phase. The effect of passing clouds is shown by the fluctuations of the mercury during the progress of the eclipse.

Temperature 3 feet 6 inches above ground. Black bulb in sun.

| | | h | m |
|---|---|---|---|
| 39° | | 8 | 20 |
| 39·25 | | 8 | 34 |
| 39·5 | | 8 | 48 |
| 39·75 | | 8 | 58 |
| 39·1 | | 9 | 11 |
| 38·75 | Greatest Phase | 9 | 32 |
| 40· | | 9 | 58 |
| 40·2 | | 10 | 3 |
| 40·5 | | 10 | 7 |
| 40·1 | | 10 | 19 |
| 42·75 | | 10 | 28 |
| 44·25 | | 10 | 40 |
| 43·4 | End | 10 | 50 |

Temperature in shade 4 feet 6 inches above ground.

| | |
|---|---|
| Time of greatest obscuration | 32° |
| At the end of eclipse | 34·5 |

Temperature on ground.

| | |
|---|---|
| Greatest obscuration | 29° |
| End of eclipse | 31·5 |

### "On an Apparatus for determining the Horizontal Magnetic Intensity in Absolute Measure," by J. P. JOULE, LL.D., F.R.S., &c., V.P.

Many years ago Weber described a small portable apparatus for ascertaining the horizontal component of the intensity of the earth's magnetism. In it a magnetic bar, about four inches long, was vibrated under the influence of the earth's magnetism, and afterwards employed to deflect a compass needle. The moment of the deflecting bar was determined from its dimensions and weight, the compass needle was supported on a point in the ordinary manner, and no allowance was made for the influence of the earth in increasing the strength of the magnet while vibrating in the magnetic meridian. Notwithstanding these defects, the results obtained were of considerable accuracy, and justified the employment of the portable apparatus by travellers. I have endeavoured to improve Weber's apparatus, so as to give it an accuracy comparable to that of the instruments employed in stationary observatories, without increasing its expense or diminishing its portability. I will, with these preliminary observations, at once commence by describing the various parts of the apparatus I have constructed for my own use.

I employ two deflecting bars, for reasons which will afterwards appear. It is important that the length of the bars should be decreased as far as possible. By so doing, there is greater probability of obtaining perfect homogeneity and accuracy of dimension, whilst they may be more readily and thoroughly magnetized. I have, after much trial and consideration, fixed upon two inches as the most convenient length, and on one quarter of an inch as the section of the bar. The bars are first filed, then thoroughly hardened, and afterwards ground to an exact figure, the operations being finished by rubbing the bars together with a little cutting powder between them. The bars may easily be made so true that when dry and perfectly polished, they will adhere strongly together. When finished, they must be measured by a standard rule under a microscope—the means of observations in many places being taken for the respective dimensions. Their weight must also be accurately determined.

The moment of a homogeneous parallelopiped in vibration is found by multiplying the square of the diagonal of the upper surface by the weight of the bar, and dividing by 12. If it be thought desirable to test the accuracy of the moment thus deduced, the best plan is to fasten small cubes of lead to the ends of the bar, by an elastic band. These cubes must be carefully wrought and measured, and their moment may be found by subtracting the moment of a bar of lead two inches long from that of a bar two inches long, plus the length of the two pieces. When the moment of the pieces of lead is determined, that of the steel bar may be found by vibrating it first by itself, and then with the lead cubes attached. Another plan is to hang pieces of lead from the ends of a filament of silk thrown over the bar, but in this case great care is requisite that the filaments have very little force of torsion, and that the vibration is very slow. The moments of the four bars I have finished, as determined by the above means, are as follows :—

| | By Measurement. | By Fixed Lead Weights. | By Suspended Lead Weights. | Average. |
|---|---|---|---|---|
| No. 1… … | 86·246 … | 86·173 … | 86·225 … | 86·215 |
| No. 2… … | 86·424 … | 86·330 … | 86·435 … | 86·396 |
| No. 3… … | 85·248 … | 85·070 … | 85·283 … | 85·200 |
| No. 4… … | 84·843 … | 84·773 … | 84·906 … | 84·841 |

The results show that the moments of steel bars, when carefully finished, may be very correctly deduced from their linear dimensions and weight.

It is important that the bars should be thoroughly and uniformly magnetized. This is best effected by bringing the ends of the bar into contact with the ends of two straight electro-magnets of considerable power. In this way the confused polarity, which more or less exists in all bars magnetized by stroking, is avoided—the result being, that the magnetic virtue is more permanent, and at the same time, much more effective.

The needle to be deflected should be of small dimensions, not exceeding half an inch in length. It may be conveniently made of several perfectly hard pieces of common sewing needle mounted on perforated card. A glass index should be affixed to it, the ends being furnished with very fine bits of copper wire. These show well when traversing the graduated circle.

A circle of six inches diameter will be found a convenient size. It should be divided to thirds of a degree. The deflections may be read off with sufficient accuracy by the aid of an eye-glass, a piece of looking-glass being placed on the table to ensure the correct perpendicularity of the eye. It is easy after a little practice, if read off the deflections to half a minute or less. The needle should be protected from currents of air by being placed with its circle in a shallow box covered by a glass plate, in the centre of which there is a hole for the purpose of suspension.

A filament of silk of six inches length, and strong enough to carry the needle with safety, will be found to give a quantity of torsion so small that it may be neglected, not amounting to more than a small fraction of a minute in a deflection of 45°. For suspending the deflecting bars a stronger thread is required, but by increasing its length to four or or five feet the effect of torsion in this latter case also becomes too small to require notice.

The remaining part of the apparatus gives the means of placing the deflecting bars at accurately measured distances from the needle. This has generally been done by means of a divided metal rod; but I find that a plain board of well-seasoned deal, with a circular hollow excavation in the centre for the graduated circle, answers the purpose very well. It is needful to be able to place the bars rapidly to an exact position on the board. This is best done for each distance by driving pins into the board, and cutting them off pretty close to the wood, one pin regulating the distance and two others the direction. It is only necessary to push the bar up to its defined position. The distances of the pins on the east side of the suspended needle from the corresponding ones on the west side are in my own apparatus 15, 20, 25, and 30 inches, half those distances being the mean distances of the bar from the needle. The first two distances are, however, all that are actually required.

In making the observations of deflection two bars are used, one on the east side, the other on the west of the needle. They must be carried to their respective places by wooden pincers, and on no account touched by the fingers or subjected to any violence. The deflection marked by the pointer at both ends of the needle must be observed, and then the bars must be reversed and the deflection in the other direction also observed. The bars must then be removed to the next distances and the observations repeated. Finally the bars must be made to change sides from east to west, and all the former observations repeated in the reverse order. Thus eight separate observations are obtained, of which the average gives the deflection due to any given distance.

The only further observation required for finding the earth's horizontal intensity is that of the number of vibrations of the bars under the influence of the earth's magnetism. It is clear that when a bar is vibrating about the magnetic meridian its magnetism is slightly increased by the inductive influence of the earth, and that thus the number of vibrations is increased. The use of two bars enables me to get rid of this difficulty in a very simple manner. I find the distance at which one bar must be held below the other so as to counteract the earth's magnetism; then I suspend the two bars, one beneath the other, at that interval, and vibrate both together. The result gives the number of vibrations due to the magnetism of the bars as uninfluenced by induction.

The method of suspension I have found most convenient is as follows:—A fine wire is twisted round the centre of one of the bars, forming a loop which is caught by a small hook fastened to the extremity of the suspending fibre. Then a filament of silk of the proper length, tied at the ends so as to form a long loop, is thrown along the length of the suspended bar, and the ends of the other bar are placed in the ends of the loop. There is some slight difficulty in making the bars hang quite horizontally, but this may be overcome by a little practice. The proof of accurate horizonal position is made by looking at the bar and a spirit level simultaneously from a distance with a telescope.

The bars being arranged as above in one system, and having nearly the same amount of magnetism in each, vibrate with perfect regularity. To ascertain the time occupied by the vibrations I look at the system through a telescope placed at a distance of two or three yards, a clock beating seconds being within hearing. Then I have a slate divided into ten columns, and each time that I note the end of a vibration I make a mark on the slate, counting at the same time the ticking of the clock. Every time that the end of a vibration appears to be sensibly coincident with the beat of the clock I write the number of seconds on the slate alongside the usual mark. After observing the vibrations, which ought not to exceed 3° of amplitude, for about ten minutes, materials will have been collected for obtaining a sufficiently accurate determination of the time of vibration by dividing the times between the several coincidences by the number of intervening vibrations and taking the mean of the whole.

As has been shown by Gauss and Weber, if M be the force of the magnetic bars in absolute measure, $\theta$ the angle of deflection produced at the distance R, and T the absolute horizontal intensity of the earth's magnetism, $\frac{M}{T}=\frac{R^3 \tan \theta}{2}$, but approximately only, because the deflecting power of a bar decreases in a more rapid ratio than the inverse cube of the distance, on account of its own magnetic length. Hence the necessity of a correction which may be found by observing the deflections at two distances R and $R_1$ from which we obtain $L^2=\frac{\theta R^3-\theta_1 R^3_1}{2(\theta R-\theta_1 R_1)}$, where L signifies the virtual half length of the bar. Then we obtain the closely correct value $\frac{M}{T}=\frac{R^3 \tan \theta}{2}-L^2 R \tan \theta$. Calling this quantity $r$, as in Gauss and Weber's equation, we have finally $T=\frac{1}{t}\sqrt{\frac{12C}{r}}$, where $t$ is the time of vibration in seconds, C is the moment of inertia of the bars multiplied by $\pi^2$, and 12 is the coefficient to reduce from inches to feet.

The following were the data for determining the horizontal intensity on four several occasions.

| | Jan. 29th, 1867. | Feb. 5th, 1867. | Feb. 11th, 1867. | March 16th, 1867. |
|---|---|---|---|---|
| C | 1691·80 | 1690·04 | 1690·04 | 1690·04 |
| R | 7·5 | 7·5 | 7·5 | 10 |
| $R_1$ | 10 | 10 | 10 | 12·5 |
| tan $\theta$ | ·913087 | ·97852 | ·97520 | ·40610 |
| tan $\theta_1$ | ·379294 | ·40607 | ·40480 | ·20656 |
| $t$ | 2·8407 | 2·7445 | 2·7483 | 2·7423 |
| $L^2$ | ·9679 | 1·028 | 1·0122 | ·9007 |
| $r$ | 185·976 | 198·861 | 198·303 | 199·395 |
| T | 3·6779 | 3·6795 | 3·6796 | 3·6776 |

The above determinations were obtained in a room in which there are several pieces of iron in fixed positions, and in the neighbourhood of which are some considerable masses of that metal. I have ascertained that these had an influence in increasing the intensity by about a fortieth. My current-meter, by which the horizontal intensity is immediately deduced from a comparison of its weighings with the deflections of a tangent galvanometer included in the same circuit, shows a gradual increase of the intensity since last September, conformably with the results obtained in past years at Kew.

"Note on the Tangent Galvanometer," by J. P. JOULE, LL.D., F.R.S., &c., V.P.

It is well known that a current circulating round a magnetic needle produces a deflection of the latter, the tangent of which is approximately proportional to the strength of the current. Pouillet made the approximation much more close by using a broad strip of copper bent into a circle, instead of the usual coil, and thus obtained the instrument commonly known as the tangent galvanometer. The arrangement of Pouillet ensures a close approximation, it is true, but it does not, after all, exactly fulfil the law of tangents; and in absolute measurements it has the disadvantage that the diameter of the circle of ribbon cannot be taken as the virtual diameter. It appeared to me that the best plan for a galvanometer would be to use a wire of about one-tenth of an inch diameter, surrounding a short needle, and to supply the small correction needed to the tangents of the deflections of the latter.

Calling, then, $\theta$ the angle of deflection, $l$ the magnetical length of the needle (generally about four-fifths of the actual length), and $d$ the diameter of the coil, we have

$$\left\{(2\sin\theta)^2-(\cos\theta)^2\right\}\left(\frac{l}{d}\right)^2\tan\theta,$$

or, more elegantly and simply, as suggested to me by Professor Jack,

$$\frac{1}{2}(4\tan^2\theta-1)\frac{l^2}{d^2}.\sin 2\theta,$$

for the correction to be supplied to the tangent of the angle observed. This correction is additive at great deflections, and subtractive at small ones, whilst at 26° 34′ the correction vanishes.

It seems, therefore, desirable that in exact researches a needle of small length should be employed, and the apparatus so arranged as to give a deflection somewhere about the above-indicated angle, where the correction, if needed at all, would be of trifling amount.

"On a Section of the Drift Deposits in the Banks of the Ribble, near Balderston Hall," by EDWARD HULL, B.A., F.G.S.

Since my paper on the "Drift Deposits in the neighbourhood of Manchester," &c., was published in the memoirs of the Literary and Philosophical Society of Manchester,* I have had opportunities of considerably extending my acquaintance with these quaternary beds in a northerly direction, as far as the banks of the Ribble. What I have seen in these districts has fully borne out the statement which I made in that paper, that the sand and gravel which rests on the Lower Till, or Boulder Clay, is, in turn, itself "overlain by an Upper Till, or Boulder Clay, quite as important, both in thickness and extent as that which lies below it." The succession of these drift deposits which I then ventured to submit in modification of that by Mr. Binney, F.R.S.,* was as follows:—

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* Mem. Lit. & Phil. Soc., Manchester, vol. ii., 3 series, p. 449.

Recent...1. Valley Gravel and River Terraces.
2. Upper Boulder Clay, or Till.
Drift... 3. Middle Sand and Gravel.
4. Lower Boulder Clay, or Till.

I wish now to describe a remarkable section visited by me a few months since on the Banks of the Ribble at Balderston, some distance below Ribchester. It is shown on the south bank of the river at Jackson's Bank Wood. In this district, the Ribble meanders over a wide alluvial plain, hollowed out of a larger valley of older date, which is overspread to a great depth by drift deposits, so that it is only at intervals the solid formations appear from below, even in the bed of the river itself. At the elbows of the river the banks, which are often 150 to 200 feet in height, are generally composed of quaternary deposits, and the river is ever wearing away these soft materials, which frequently come down in great landslips, and are carried downwards to the sea. My visit was made a few days after the great flood of last November, when the river rose 50 feet above its bed—overflowing the alluvial plain, and bringing down vast masses of its banks.

At Jackson's Bank Wood the river makes a fine sweep, and the section, which is 200 feet in depth, is very clearly laid open throughout a horizontal distance of several hundred yards. It is represented in cross section in the figure below.

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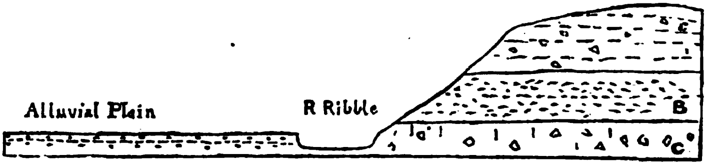
* "On the Drift Deposits of Manchester and its Neighbourhood." Mem. Lit. & Phil. Soc., vol. viii., 2 series.

*Section of Drift Deposits in the Banks of the Ribble at Balderston.*



| | | |
|---|---|---|
| c | *Upper Till.* | Reddish Till, from 70 to 80 feet in thickness. |
| B | *Middle Sand.* | Yellow and white soft sand, with beds of gravel, from 85 to 90 feet. |
| C | *Lower Till.* | Dark blue stiff clay, forming the bottom of the bank, about 25 to 30 feet in thickness. |

The line of demarcation between the several members of the drift in this section is remarkably clear, and shows the great importance of the Upper Till, which forms the top of the bank for a depth of about 85 or 90 feet, and spreads over the country for miles. If there were no other section than this it would be sufficient to establish its place in the series of Drift Deposits.

In this section, and in some others I have noticed, there appears to be a difference between the Upper and Lower beds of Till. The Upper seemed to partake of a laminated or bedded structure, while the lower (as far as it was possible to judge at some distance, as it was inaccessible) seemed to want any appearance of stratification, and to correspond rather to the description which Mr. Geikie gives of the Lower Till of Scotland. This he considers to have been formed by land ice, and in Scotland is generally destitute of any true stratification. In Lancashire, however, I have several times observed the Lower Till to be truly bedded; and I doubt much whether we have anything which exactly corresponds to the Lower Till of Scotland, at least until we approach the hills of the Borders and of the Lake districts.

At Blackburn the Upper Till descends into, and forms, the valley on which the town is built. It is in some places several yards in thickness resting on the Middle Sand, which at Pleasanton and Hoghton produces an undulating country, sometimes assuming the form of sand-dunes.

I shall conclude this notice with one remark. There are few things more deceptive than steep sections in drift deposits on the banks of a river or brook; and unless the observer is aware of the fact, he is liable to error. In cases where Till forms the upper part of the bank and rests upon beds of sand, the latter is sometimes completely hidden from view by a thick wash of the Till, which then seems to form the whole cliff. This is sometimes the case with the banks of the Ribble, and arises from two causes: The first is—that the sand is easily undermined by the stream, and giving way, the Red Till comes slipping down from above, and being more plastic, or cohesive, forms a covering for the whole surface. The second cause is—the rain, which softens the clay, and when a heavy fall happens, it is carried down in a semi-fluid state and spread over the underlying beds. It is, therefore, by no means common to find so fine a section, showing the three members of the drift series in their respective order and thickness, as that described at Balderston, which is my apology for bringing it before the notice of the Society.

I may state that the height of the cliff is taken from the contour lines on the Ordnance 6-inch map, and the thickness of the different members, by estimating by the eye their relative proportions from a point on a level with the centre of the cliff.

## PHOTOGRAPHICAL SECTION.

March 12th, 1867.

SAMUEL COTTAM, Esq., in the Chair.

Mr. **WARDLEY** said that he had observed recently in the Photographic Journals that complaints were made as to the appearance of streaks on collodion plates in the direction of the dip. He had never until lately seen anything of the kind, but he had been using some collodion which produced the markings, and he noticed that the streaks could be seen on the plates when taken out of the bath, and before exposure. The marks were not such as would be removed by re-dipping the plate and moving it from side to side in the solution; and he thought the cause would be found to arise from the collodion being over iodized. He had used collodion containing about one-eighth less iodizing solution and found that the streaks had disappeared.

Mr. **BROTHERS**, F.R.A.S., exhibited a number of photographs taken during the Eclipse of the Sun on the 6th inst. He said that he had anticipated some difficulty in photographing the sun from the excess of light causing over exposure, as the image was taken at the focus of the telescope without enlargement through an eyepiece. To overcome that difficulty the aperture of the telescope was reduced first to two inches, and then to half an inch; and collodion and developing solution which had been mixed upwards of twelve months, and an old silver bath were used. The exposure was as instantaneous as could be made by hand.

In explanation of the four prints being on one plate, Mr. Brothers stated that the slide he used for astronomical photography is arranged so that four negatives of the sun or moon can be taken on the same plate, an advantage which he had found of much importance in this kind of work. The time occupied in taking the four negatives on plate No. 3 was thirty seconds, No. 4 twenty-five seconds; and the last set was taken in *twenty seconds*—an average of *five seconds* for exposing and changing the position of the plate.

Two of the prints, 3 and 4 of the series, were mounted, in reversed order, for the stereoscope, and they exhibit very good stereoscopic effects although taken from the same station, the difference in the position of the moon in an interval of about seven minutes being sufficient for the purpose. The negatives on No. 3 were taken at $9^{h}\ 32^{m}$ to $9^{h}\ 32^{m}\ 30^{s}$, and those on No. 4 at $9^{h}\ 39^{m}$ to $9^{h}\ 39^{m}\ 25^{s}$

Mr. BROTHERS also exhibited a photograph on canvass printed without the aid of bitumen of Judea or gelatine, or of any other substance likely to change by time. The print is so completely stained into the canvass as prepared in the ordinary way for artists, that it cannot chip or rub off. Another advantage is that hyposulphite of soda is not required to fix the image.

## Ordinary Meeting, April 2nd, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Sir Henry Holland, Bart., M.D., F.R.S., &c., was elected an Honorary Member of the Society.

Messrs. G. C. Lowe and W. L. Dickinson were appointed Auditors of the Treasurer's accounts.

"On the Microscopical Examination of Coal Ash or Dust from the Flue of a Furnace, illustrated by the Microscope," by J. B. DANCER, F.R.A.S.

When coal is burnt in a furnace to which atmospheric air has free access, a portion is converted into gaseous and volatile matter; and the incombustible substance which remains is the ash. The amount of ash in coals from different localities is very variable; it is said to range from 1 to 35 per cent. The ash or dust which is the subject of this paper was collected from the flue of my steam boiler furnace, in which common engine coal is used as fuel. This coal leaves a considerable amount of incombustible matter. A specimen of the dust is now before you; it is of a reddish brown colour, and free from soot or carbonaceous particles.* When this dust is examined under the microscope with a power of 40 or 50 diameters, it is found to consist of ferruginous matter and crystallised substances, some particles transparent, others white and red. It contains also a number of curious looking objects, which vary considerably in size and colour; the majority of these bodies are spherical, and when separated from the irregularly shaped particles forming the bulk of the dust they become interesting objects for the microscope. I shall confine my remarks more especially to these globular bodies. Some of these are as perfect in form as the most carefully turned billiard balls, and have a brilliant polish. The various colours which these globules exhibit give additional interest to their examination. Some are transparent crystal spheres, others are opaque white, many are yellow and brown, and variegated like polished agates or carnelian of different shades. The most abundant of the highly polished balls are black; there are others which look like rusty cannon balls—some of these have an aperture in them like a bomb shell, and many are perforated in all directions. To obtain these objects the dust should be washed in a bowl and all the lightest particles allowed to float away; the remainder consists of fragmentary crystalline and ferruginous substances; mixed with these are the polished balls described, which under the microscope, by a brilliant reflected light, look like little gems. To separate the spherical bodies from the irregular ones it is only necessary to sprinkle some of this material on an inclined glass plate, and by gentle vibration the balls roll down, and can thus be collected. Having satisfied ourselves with the examination under the microscope, it is natural that we should desire to know more about these novel objects. What is their elementary constitution? Why are they spherical? How do they get into the flue? I have not attempted a chemical analysis of these minute bodies, many of which are less than the 100th part of an inch in diameter. I can only therefore offer an opinion as to their probable constitution, judging from what is known of the chemical analysis of coal ash, and from the appearance they present under the microscope. Referring to the chemical analysis of coal ash, we find that it sometimes contains silica, magnesia, alumina, sesqui-oxide of iron, lime, soda, potash, sulphate of calcium, anhydrous sulphuric acid, anhydrous phosphoric acid, sulphur, and sometimes traces of copper and lead. The vegetable origin of coal is now generally admitted, and doubtless some of the substances I have just named have been taken up by the coal plants, whilst other portions may have collected in the locality where the coal was formed. As this is not immediately connected with our present inquiry, I proceed to speculate as to the constitution of these globular bodies. The transparent spheres I imagine to be silicates of soda or potash; the opaque white are most likely silicate of soda or potash combined with lime and alumina; the yellow and brown are silicates coloured by iron in different proportions. The black globes are not all alike in composition; some of these are silicates coloured by carbon, others are iron balls coated externally with a silicate. Many of these rusty cannon balls are probably ferrous oxide formed by the action of heat on the iron pyrites in the coal. There are also balls of black magnetic oxide: the perforated shells are probably ferrous sulphides, The globular form of these bodies suggests that they have been thrown off in scintillations, such as are seen during the combustion of iron in oxygen gas, and whilst in a fluid state they assume a spheroidal form. They are carried by the draught into the flue, and being of greater specific gravity than the carbonaceous matter forming the smoke, they fall before the current of air has reached the chimney. Some of the dust has been a considerable time in the flue, exposed to the intensely heated circulating flame; the reducing action of this would probably convert some of the oxide into metallic iron. Many of these balls have the appearance of reduced oxides. The flue dust contains a larger amount of ferruginous matter than can be accounted for by the analysis of coal ash. I think the surplus may be regarded as representing the wear and tear of the iron work about the furnace, such as fire bars, boiler plates, &c. The brick work and cement about the boiler and flues may also supply some of the silica, alumina, and iron for these balls, numbers of which are merely thin shells. The movements of these objects, caused by the approach of a magnet under the stage of the microscope, are somewhat amusing, and it is at times startling to see the crystalline objects, both spherical and irregular, exhibit magnetic attraction: probably they contain particles of iron imbedded in them; if they do not, may we not imagine that there is some magnetic compound in which the crystalline matter predominates? When we consider the accidental condition under which this matter has combined, it is just possible that some new molecular arrangement or combination of elements may have taken place. It is very probable that many of these polished balls are much more complex in their elementary constitution than I have stated. They are in fact a kind of glass, and many of them merely bulbs. Pelouze states that glass is probably an indefinite mixture of definite silicates. Glass, containing small quantities of ferrous oxides and sodic sulphates, when exposed to sunlight becomes yellow, and possibly some of these balls may have changed in colour since they came from the flue. Hydrochloric and nitric acid exert very little action on the ferruginous globes: this may be due in some measure to the high temperature at which the oxides have been formed; in other cases they are no doubt protected by an external coating of some silicate. It would require much time and patience to collect a sufficient number of each kind of these minute objects for a chemical analysis; but the spectroscope might probably assist in revealing their constitution. When time permits I hope to resume the subject.

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* My attention was drawn to this subject by Mr. Johnson, of Wigan, in November, 1866.

"On the Galvanometer," by **William Jack**, M.A., Professor of Natural Philosophy at Owens College.

A current of intensity $i$ passes through a circular wire (radius $r$) fixed in a vertical plane passing through the magnetic meridian. A needle of length $l$ (between the poles) is suspended in the usual way, on a fine point, situated on the axis of the plane of the coil, at a distance $e$ from its centre, and can rotate round the point in a horizontal plane. Let E represent the earth's magnetic intensity, $\alpha$ the angle of deviation of the needle, and $\rho = \sqrt{e^2+r^2}$, the distance of the needle point from the circumference of the coil. Then

$$\mathrm{E}\tan\alpha = \frac{2\pi i r^2}{\rho^3}\left(1 - 3\frac{l^2(4e^2-r^2)(1-5\sin^2\alpha)}{4\rho^4} + \frac{45l^4}{64\rho^8}(8e^4-12e^2r^2+r^4)\right.$$
$$\left.(1-14\sin^2\alpha+21\sin^4\alpha)\right)$$

(Wiedemann, *Galvanismus*, vol. 2, p. 165.)

Accordingly,

$$i = \tan\alpha\frac{\rho^3\mathrm{E}}{2\pi r^2}\left\{1 - 3\frac{l^2(4e^2-r^2)(1-5\sin^2\alpha)}{4\rho^4} + \frac{45l^4}{64\rho^8}(8e^4-12e^2r^2+r^4)\right.$$
$$\left.(1-14\sin^2\alpha+21\sin^4\alpha)\right\}^{-1} = \tan\alpha\frac{\rho^3\mathrm{E}}{2\pi r^2}\left\{1 + 3\frac{l^2(4e^2-r^2)(1-5\sin^2\alpha)}{4\rho^4}\right.$$
$$-\frac{45l^4}{64\rho^8}(8e^4-12e^2r^2+r^4)(1-14\sin^2\alpha+21\sin^4\alpha)$$
$$\left.+\frac{36l^4}{64\rho^8}(16e^4-8e^2r^2+r^4)(1-10\sin^2\alpha+25\sin^4\alpha)\right\}$$

$+$ higher powers in $\frac{l^2}{\rho^4}$ which may be disregarded when $l$ is small compared with $\rho$.

As a first approximation, therefore, we have

$$i = \frac{\rho^3\mathrm{E}}{2\pi r^2}\tan\alpha,$$

which shows that to this approximation the intensity varies as the tangent of deflection.

The first term to be applied in correction is

$$\frac{\rho^3\mathrm{E}}{2\pi r^2}\tan\alpha\frac{3l^2}{4\rho^4}(4e^2-r^2)(1-5\sin^2\alpha) \quad (\mathrm{A})$$

This becomes 0 in two cases,

(1) $1 - 5\sin^2\alpha = 0, \therefore \sin^2\alpha = \frac{1}{5}, \tan^2\alpha = \frac{1}{4}$

so that $\alpha$ is the angle whose tangent is $\frac{1}{2}$.

This is the angle at which Dr. Joule has pointed out that it is best to work for accurate measurements. The first term (A) of the total correction will disappear, therefore, even though the centre of the magnetic needle may not be accurately in the plane of the coil, provided it is on a line perpendicular to the plane passing through the centre. When we work at any other angle the correction is

$$\frac{3l^2E}{8\pi r^2}\tan\alpha\frac{(4e^2 - r)}{(e^2 + r^2)^{\frac{1}{2}}}(1 - 5\sin^2\alpha)$$

The magnitude of the correction will depend therefore on that of the term

$$\frac{(4e^2 - r^2)}{(e^2 + r^2)^{\frac{1}{2}}}\tan\alpha,$$

and will still become zero whatever be the value of $1 - 5\sin^2\alpha$, provided $4e^2 - r^2 = 0$.

The same accuracy is therefore secured by working at any angle with a needle whose centre is at a distance equal to half the radius of the coil from its centre, and along a line perpendicular to its plane.

The next term of the correction becomes

$$\frac{\rho^3E}{2\pi r^2}\tan\alpha\frac{27l^4}{40\rho^8}(8e^4 - 12e^2r^2 + r^4)$$

for the angle $\alpha$ specified. For $e = 0$ (needle at centre), this gives

$$(\rho = r), \quad \frac{r^3E\tan\alpha}{2\pi r^2} \cdot \frac{27l^4}{40r^8} \cdot r^4 = E\tan\alpha . \frac{27l^4}{80\pi r^3}$$

For $e = \frac{r}{2}$, it gives $l^2 = e^2 + r^2 = \frac{5r^2}{4}$

$$E.\tan\alpha \cdot \frac{27}{80\pi} \cdot l^4 \cdot \frac{\rho^3}{\rho^8} \times -\frac{3r^2}{2} = -E.\tan\alpha . \frac{27}{80\pi}\frac{l^4}{r^3}\frac{\frac{3}{2}}{\frac{25\sqrt{5}}{2^5}}$$

$$= - E.\tan\alpha . \frac{27}{80\pi} \cdot \frac{l^4}{r^3}\frac{48}{25\sqrt{5}}$$

The last correction is therefore rather less than the former. There is, therefore, a slight positive advantage in working with the needle centered at $e = \frac{r}{2}$, over Dr. Joule's method, even at the special angle $a = \tan^{-1} \frac{1}{2}$, and in addition the first term of the correction disappears for *all* angles of deviation, instead of for one only.

The accuracy of the formula quoted above turns on the circuit being complete (as it never is), on its being accurately circular, in the vertical meridian plane, and on the $r$ being a definite quantity. If the thickness of the coil be comparable to the magnitudes $r$ and $l$ used, a much more complicated formula would replace that given above.

The instrument in which the needle pivot is at $e = \frac{r}{2}$, possesses this advantage, that we may arrange a considerable number of coils for which this is true, parallel to each other, along the smooth surface of a cone whose axis is $\frac{r}{2}$, and whose base has the radius $r$. It is to be observed, however, that though the second term in the value for $i$ given by each separate parallel coil disappears, the first term

$$\tan a \frac{E}{2\pi} \frac{\rho^3}{r^3} \text{ becomes } \tan a \frac{E}{2\pi} \frac{5\sqrt{5}}{8} r$$

and therefore for one same current circulating through several coils, for each of which there is a different radius, there will not be accurately one same amount of deviation produced by each. Accordingly, the current through two coils has not accurately the effect of a current of double intensity, and has only that effect approximately, so long as the $r$ of the parallel coils is very nearly the same. In fact, no multiplying instruments can approach the accuracy of the ordinary circular single meridian coil with the needle in the centre, when it is worked at $a = \tan^{-1} \frac{1}{2}$, (Joule's arrangement), or of the circular single meridian coil with the needle in axis of coil at distance $e = \frac{r}{2}$ from centre (Gaugains' arrangement).

A curious result of the formula (A) given for the first correction is that this correction is a maximum

$$\text{for} \quad 1 - 4\tan^2\alpha - 10\sin^2\alpha = 0$$

$$\text{or} \quad \sin^2\alpha = \frac{3 - \sqrt{7.4}}{4}$$

Which corresponds to an angle 15° 20′. At this angle, therefore, the correction that must be applied is greatest for all tangent galvanometers. Hence, in delicate research where the needle is in the centre of the instrument, and when accuracy is of great importance, we should take care that our deviations shall not be at or near this amount. In Gaugains' arrangement this angle is of no special importance.

A simple numerical calculation may show the comparative insignificance of the correction remaining when either arrangement is adopted. If we call I the first approximate value of $i$

$$i = \mathrm{I}\left(1 - \frac{45l^4}{64\rho^8}(8e^4 - 12e^2r^2 + r^4)(1 - 14\sin^2\alpha + 21\sin^4\alpha\right)$$

Take $l = \frac{1}{2}$ inch, $r = 10$ inches. Then

(1) Joule's arrangement, $\alpha = \tan^{-1}\frac{1}{2}$, $\mathrm{E} = 0$

$$i = \mathrm{I}\left(1 - \frac{45.}{64} \cdot \left(\frac{1}{20}\right)^4 \times -\frac{24}{25}\right)$$

$$= \mathrm{I}(1 + \cdot 000004)$$

(2) Gaugains' arrangement,

$$\alpha \text{ any value } e = \frac{r}{2}$$

$$i = \mathrm{I}(1 + \cdot 00000027)(1 - 14\sin^2\alpha + 21\sin^4\alpha)$$

a fraction depending on $\alpha$, but always excessively small. It is obvious that inevitable instrumental errors must always produce corrections more considerable than either of these.

"Additional Note on the Galvanometer," by Dr. J. P. JOULE, F.R.S., V.P.

The current-meter has enabled me to find experimentally the diminution of deflection consequent on an increase of the length of the needle. This was done by comparing the square roots of the weighings of the meter with the deflections occasioned by a coil 15·2 inches diameter in the centre of which needles of 1, 2, and 4 inches length were successively placed. I find the fractions representing the loss on the tangents of deflection as follow:

| | From 1in. to 2in. Needle. | From 1in. to 4in. Needle. |
|---|---|---|
| 68° 21′ | ·02239 loss | ·11258 loss |
| 42° | ·00993 loss | ·04870 loss |
| 27° 2′ | ·00023 gain | ·00254 loss |
| 19° 1′ | ·00287 gain | ·01646 gain |

Hence the total fractions gained or lost by the two needles are:

| | For 2in. Needle. | For 4in. Needle. |
|---|---|---|
| 68° 21′ | ·02985 loss | ·12009 loss |
| 42° | ·01324 loss | ·05194 loss |
| 27° 2′ | ·00032 gain | ·00230 loss |
| 19° 1′ | ·00383 gain | ·01756 gain |

Since my last communication I have made trial of many bars and needles, and find that the magnetic length is about $\frac{7}{8}$th of the actual in most cases. If therefore instead of $l$ we take the actual length of the needle $n$, the correction to be applied to the tangent of deflection will be nearly

$$\frac{1}{4}\left(4\tan^2\theta-1\right)\frac{n^2}{d^2}\sin 2\theta.$$

The fractions of loss or gain calculated from this formula for the deflections experimented with are as follow:

| | For 2in. Needle. | For 4in. Needle. |
|---|---|---|
| 68° 21′ | ·02873 loss | ·11493 loss |
| 42° | ·01072 loss | ·04290 loss |
| 27° 2′ | ·00029 loss | ·00116 loss |
| 19° 1′ | ·00406 gain | ·01625 gain |

Very curious phenomena present themselves as the needle is prolonged. There ultimately becomes a point of stable equilibrium on the other side of the meridian. The following rough results obtained with a coil of 7¼ inches diameter will give an idea of these changes:

| Length of Needle. | Deflection on the usual side. | Deflection on the opposite side. | Unstable Equilibrium on the opposite side. |
|---|---|---|---|
| 1in. | 74° | — | — |
| 2in. | 72° | — | — |
| 3in. | 68° | — | — |
| 3½in. | 65½° | — | — |
| 4in. | 62° | — | — |
| 4½in. | 56° | — | — |
| 5in. | 50° | — | — |
| 5½in. | 41½° | — | — |
| 6in. | 34° | — | — |
| 6½in. | 23° | 35° | 35° |
| 6¾in. | 21° | 47° | 26° |
| 7in. | 17° | 57° | 14° |
| 7¼in. | 13° | 59° | 13° |
| 7½in. | 8½° | 59° | 6° |
| 8in. | — | 63° | |
| 9in. | — | 62° | |
| 10in. | — | 61° | |
| 11in. | — | 59° | |
| 12in. | 11° | 58° | |
| 13in. | 17° | 56° | |
| 14in. | 20° | | |
| 15in. | 23° | | |

"Notes on the Smallest and Simplest Forms of Matter and its Relations to Mechanical Forces," by J. C. Dyer, Esq., V.P.

The author's object in this paper is not so much to enlarge the present sphere of our knowledge, as it is to show how little we really know, about the essence of matter, or of the exertion of *any forces* by it, excepting such as are open to direct experiment and observation. If there be any recondite forces that sustain the variable forms of bodies, all we can know about them must be derived from the sensible properties of matter; for any assumed properties, or motions of bodies, or their component parts, if hidden from our powers of perception, they are so because they afford no tests, by weight or measure, of their existence as physical realities. Still, with a view to obtain some conception of the laws that govern the visible and tangible forms of bodies, several hypotheses have been suggested to account for phenomena attending the mutations of bodies. It has been *assumed* that certain *internal motions* and *forces* are called into action by the changes that take place in the visible forms of bodies—to explain these we have had the *theories* of *atoms*, of *corpuscules*, of *molecules* and the like. By these theoretic views of the composition of bodies, the smallest particles in them are supposed to be endowed with *functional properties*, which exert mechanical forces, according to the distances apart of the atoms and molecules, in all ponderable bodies. The author does not object to these theories, if they may in anywise serve to clear the paths of inquiry into the physical properties of bodies and their mutations; but whatever may be the *real* or *ideal* internal action of the particles of matter upon each other, or upon their masses, the questions they involve do not admit of any solution, or even elucidation, by any course of mathematical investigation that has been or can be applied to them, and this for the obvious reason that those particles (whether called atoms, molecules or otherwise) can have no conceivable weight or extension apart from their masses, and therefore no *functional value* can be assigned to them as the basis of any kind of moveable or statical forces in them. Whether such atoms and molecules be considered as in a state of "vibration," "oscillation," "rotation," or as fixed by their balanced forces of attraction and repulsion, they are in each case absolutely beyond the grasp of the *mathematical* investigator, however profound he may be, because he has not the shadow of any ground for assigning either *size*, *motion*, or *force* to the minute parts of matter apart from its aggregate forms, and all we can even know on these obscure matters must be inferred from observed physical effects.

For reasons which he states the Author dissents from the dynamical theory of heat, and ventures to maintain the theory of a *calorific element* having not only the properties *sui generis* ascribed to it by the old school — Boerhaave to Dalton — but also as being the real source of all other forms of heat, and likewise as constituting in itself the essence of the elastic force in bodies.

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"Variable Star Ephemeris for 1867, calculated from the Elements given in Dr. Schönfeld's Catalogue," by J. R. Hind, F.R.S., &c., Superintendent of the Nautical Almanac, and Honorary Member of the Society.

The times are those of *Maxima* unless otherwise stated.

| | |
|---|---|
| R Androm. | January 28. |
| T Piscium | *Max.* April 4·4, and August 28·8. |
| | *Min.* June 25·4, and November 18·8. |
| S Piscium | February 12·4. |
| R Piscium | 1866, Dec. 17, and 1867, Nov. 29. |
| R Arietis | February 19·3, and August 23·5. |
| *o* Ceti | November 25·9. |
| R Persei | June 29. |
| R Tauri | September 15·7. |
| S Tauri | May 21. |
| R Orionis | August 22. |
| R Aurigæ | December 6. |
| R Geminorum | February 16·5. |

| | |
|---|---|
| R Canis Min. | July 23. |
| S Canis Min. | January 21, and December 23. |
| S Geminorum | *Min.* June 8. |
| T Geminorum | January 28, and November 11. |
| U Geminorum | Feb. 11·0, May 19·1, Aug. 24·2, Nov. 29·3. |
| R Cancri | October 15·6. |
| U Cancri | July 13. |
| S Hydræ | 1866, December 21·4. and 1867, Sep. 3·8. |
| T Cancri | *Min.* December 16. |
| T Hydræ | August 24. |
| R Leonis Minoris | March 13. |
| R Leonis | September 24·6. |
| R Ursæ Maj. | April 26. |
| S Leonis | March 6, and September 12. |
| R Comæ | August 30. |
| T Virginis | October 16. |
| T Ursæ Maj. | March 5·9, and Nov. 18·2. |
| R Virginis | March 9. |
| S Ursæ Maj. | March 10·6, and October 21·4. |
| U Virginis | January 19·4, and August 15·0. |
| V Virginis | January 13, and September 22. |
| R Hydræ | December 17·0. |
| S Virginis | June 13·6. |
| S Bootis | January 3, and October 1. |
| R Camelop. | June 20·8. |
| R Bootis | July 28·5. |
| S Serpentis | January 23. |
| S Coronæ | June 29. |
| R Serpentis | April 29. |
| R Libræ | 1868, March 27. |
| R Herculis | 1867, March 31. |
| U Herculis | August 4. |
| S Ophiuchi | February 9 and October 1. |
| S Herculis | August 21. |
| R Ophiuchi | July 30. |
| T Herculis | *Max.* April 20, October 2. |
| | *Min.* January 30, July 14, December 26. |

| | |
|---|---|
| R Lyræ | Feb. 13, Mar. 31, May 16, Jy. 1, Aug. 16, Oc. 1. Nov. 16, 1868, Jan. 1. |
| R Aquilæ | February 14. |
| R Sagittarii | June 29. |
| R Cygni | August 13. |
| S Vulpeculæ | *Max.* Feb. 25·8, May 4·7, July 11·6, Sept. 17·6, Nov. 24·5.<br>*Min.* Jan. 26·0, April 3·9, June 10·8, Aug. 17·8, Oct. 24·7, Dec. 31·6. |
| χ Cygni | 1866, Dec. 25·4.—1868, Jan. 15·5. No *max.* in 1867. |
| S Cygni | April 15. |
| R Capricorni | August 19. |
| S Aquilæ | March 5, July 5·5, November 5. |
| R Sagittæ | *Min.* March 4·2, May 13·8, July 23·4, Oct. 2·0, Dec. 11·5. |
| S Delphini | July 22. |
| T Delphini | May 31. |
| U Capricorni | 1868, January 9. |
| T Aquarii | June 27. |
| R Vulpeculæ | *Max.* April 20·0, and September 5·6.<br>*Min.* Feb. 18·0, July 7·0, and Nov. 22·0. |
| T Capricorni | August 23. |
| S Cephei | *Min.* March 27 ±. |
| μ Cephei | *Min.* May 1, *Max.* 1868, January 12. |
| T Pegasi | November 27. |
| S Aquarii | August 2·5. |
| R Pegasi | June 28. |
| R Aquarii | 1866, December 24, 1868, January 16. |
| R Cassiopiæ | July 3·5. |

## Ordinary Meeting, April 16th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Henry Charles Beasley was elected an Ordinary Member of the Society.

"On a New Form of the Dynamic Method for Measuring the Magnetic Dip," by Sir WILLIAM THOMSON, M.A., D.C.L., F.R.S., &c., Honorary Member of the Society.

Seven years ago an apparatus was constructed for the natural philosophy class of the University of Glasgow, for illustrating the induction of electric currents by the motion of a conductor across the lines of terrestrial magnetic force. This instrument consisted of a large circular coil of many turns of fine copper wire, made to rotate by wheel work about an axis, which can be set to positions inclined at all angles to the vertical. A fixed circle parallel to the plane containing these positions, measured the angles between them. The ends of the coil were connected with fixed electrodes, so adjusted as to reverse the connections every time the plane of the coil passes through the position perpendicular to that plane. When in use, the instrument should be set as nearly as may be in the magnetic meridian. The fixed electrodes being joined to the two ends of a coil of a delicate Galvanometer, a large deflection is observed when the axis of rotation forms any considerable angle with the line of magnetic dip. On first trying the instrument I perceived that its sensibility was such as to promise an extremely sensitive means for measuring the dip. Accordingly, soon after I had a small and more portable instrument constructed for this special purpose; but up to this time I had not given it any sufficient trial. On the occasion of a recent visit, Dr. Joule assisted at some experiments with this instrument. The results have convinced us both that it will be quite practicable to improve it so that it may serve for a determination of the dip within a minute of angle. I hope, accordingly, before long to be able to communicate some decisive results to the society, and to describe a convenient instrument which may be practically useful for the observation of this element.

"Further Remarks on the Galvanometer," by **William Jack**, M.A., Professor of Natural Philosophy at Owens College.

Dr. Joule's results, given in the last number of the *Proceedings*, suggested to me to inquire whether the ordinary formula which I gave in my last communication allowed a deviation of different sign for a short magnet and for one of considerable length. When the needle is at the centre of the circle, the formula gives, putting

$$\frac{l^2}{r^2} = n, \text{ and } \sin^2 a = x,$$

and making some simple transformations

$$\frac{64}{945} \cdot \frac{E \cdot r . \tan a}{2\pi i} = \underset{= E}{\left(nx - \frac{315n+120}{945}\right)^2} \underset{- F}{- \frac{180.315}{945^2}} . \left\{ \overline{n + \frac{84}{315}}^2 - \frac{87696}{315^2} \right\}$$

This always gives a + result for $n$ small, as may be readily seen from the formula in its original shape. Its complete solution in $x$ for every value of $n + i$ would require us to solve an algebraical equation of the form

$$A\sqrt{\frac{x}{1-x}} = (Bx-C)^2 - D$$

which would be difficult, if possible. But we may see that the second term can never become negative, unless F is positive, since E is essentially positive. No negative result can be obtained, therefore, for tan $\alpha$, unless

$$\left(n+\frac{84}{315}\right)^2 > \frac{87696}{315^2}$$

$$\therefore\ n > \frac{212\cdot 1}{315}$$

Whenever $n$ is greater than this value, E—F *may* become negative, and if it become so for some value of $x$ $(=\sin^2\alpha)$ less than 1, we may always adjust $i$ so as to give $\frac{64}{945}\cdot\frac{E\cdot r\cdot\tan\alpha}{2\pi i}$ an equal value, where $\alpha$ is the negative angle in the first quadrant the square of whose sine is defined as above. Apparently, therefore, there ought to be at all events one solution implying deviation opposite to that for a small magnet, for some value of $i$, whenever $\frac{l^2}{r^2} > \frac{212\cdot 1}{315}$.

The results in several cases in which different values have been assigned to $\frac{l^2}{r^2}$ are given below.

1. $\frac{l^2}{r^2} = \frac{212\cdot 1}{315}$ (limit value)

$$\text{then } E = 0,\ \text{for } x = \frac{332\cdot 1}{636\cdot 3} = \sin^2\alpha.$$

but in order that the original equation should hold, we must have $i$ indefinitely great.

$$2.\quad \frac{l^2}{r^2}=\frac{216}{315}=\frac{24}{35},$$

A negative tan $a$, when $(648x-336)^2 \angle 180\cdot 315\left(\frac{300^2}{315^2}-\frac{87696}{315^2}\right)$

$$\text{for } x=\sin^2 a \angle \frac{372\cdot 2}{648} \qquad a=a_1$$

$$\text{And also for } x \mathrel{7} \frac{300}{648} \qquad a=a_2$$

$i$ being taken so as to give a deviation between $a_1$ and $a_2$, we shall have a possible solution in opposite direction to that for small magnet.

$$3.\quad \frac{l^2}{r^2}=1,\ \ x=\sin^2 a \angle \frac{637}{945} \mathrel{7} \frac{233}{945}.$$

These examples only profess to show that when the intensity is suitably taken, the result specified by Dr. Joule is verified. I should expect to find, when the intensity is gradually changed through a wide range, the sign of the deviation also change, and then, after a certain continued increase of $i$ (longer continued, as $l$ bears a greater proportion to $n$) generally change back again.

It will be noticed that $\frac{l^2}{r^2}$ must be $\mathrel{7} \frac{212\cdot 1}{315}$.

Now Dr. Joule points out that if L = actual length of needle, $l=\frac{7}{8}$ L approximately. Assuming this

$$\therefore \frac{49}{64}\cdot\frac{L^2}{r^2} \mathrel{7} \frac{212\cdot 1}{315}$$

$$\therefore \frac{L^2}{r^2} \mathrel{7} \frac{64}{49}\cdot\frac{212\cdot 1}{315}$$

$$\therefore \frac{L}{r} \mathrel{7} \frac{8}{7}\cdot \times \cdot 82$$

$$\mathrel{7} \cdot 94$$

And $7\frac{1}{4}$ inches × ·94 = 6·8 inches.

Dr. Joule finds the effect begins at 6·5 inches.

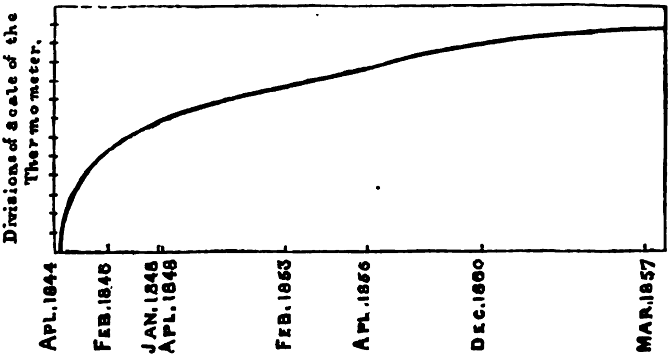
"Observations on the Alteration of the Freezing Point in Thermometers," by Dr. J. P. JOULE, F.R.S., V.P.

Having had in my possession, and in frequent use, for nearly a quarter of a century, two thermometers, of which I have from time to time taken the freezing points, I think the results may offer some interest to the Society. Both thermometers are graduated on the stem, and are, I believe, the first in this country which were accurately calibrated. Thirteen divisions of one of them correspond to one degree Fahrenheit. It was made by Mr. Dancer, in the winter of 1843-44. My first observation of its freezing point was made in April, 1844. Calling this zero, my successive observations have given

| | |
|---|---|
| 0 | April, 1844. |
| 5·5 | February, 1846. |
| 6·6 | January, 1848. |
| 6·9 | April, 1848. |
| 8·8 | February, 1853. |
| 9·5 | April, 1856. |
| 11·1 | December, 1860, |
| 11·8 | March, 1867. |

The total rise has been, therefore, ·91 of a degree Fahrenheit. The other thermometer is not so sensitive, having less than four divisions to the degree. The total rise of its freezing point has been only ·6 of a degree; but this is probably owing to the time which elapsed between its construction and the first observation being rather greater than in the case of the other thermometer. The rise of the two thermometers has been almost identical during the last nineteen years.

A projection of the observations given above is shown in the following diagram :—



Mr. E. BOWMAN, M.A., and Mr. A. BROTHERS, F.R.A.S., illustrated the phenomena of "sonorous" and "sensitive" flames by a series of interesting experiments.

"On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes, Part IV.," by JAMES NASMYTH, Esq., C.E., Corresponding Member of the Society.

In this part of his paper the author gives detailed descriptions, illustrated by diagrams, of his methods of mounting the specula of reflecting telescopes, and of testing the figure of the great speculum; he also offers some very useful remarks on the general management of Newtonian reflecting telescopes, and on the atmospherical circumstances which affect their performance.

## PHOTOGRAPHICAL SECTION.

April 9th, 1867.

J. **Baxendell**, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. **Brothers** read the following "Note on Photography in 1787."

It is generally supposed that the earlier attempts to use nitrate of silver for producing pictures of lace, leaves, and other objects on white leather or paper were made by Wedgewood and Davy about the year 1802; but it will appear from the following extract that at least fifteen years earlier than the date named. and within ten years of the time when Scheele investigated the subject of the action of light on the salts of silver, the possibility of utilizing the action of light was known. The title of the book from which the extract is taken is "Rational Recreations in Natural Philosophy," &c., by W. Hooper, M.D., 1787; and the paragraph is headed "How to print letters by sunlight."

"Dissolve chalk in aqua fortis to the consistence of milk, and add to that a strong dissolution of silver. Keep this liquor in a glass decanter well stopped, then cut out from a paper the letters you would have appear and paste the paper on the decanter, which you are to place in the sun in such a manner that its rays may pass through the places cut out of the paper and fall on the surface of the liquor. The part of the glass through which the rays pass will turn black, while that under the paper will remain white. You must observe not to move the bottle during the time of the operation."

Mr. COOTE exhibited some snow scenes, the negatives of which were taken on collodio-albumen plates. Some of these beautiful views were slightly defective in the high lights, a number of vein-like markings appearing in the sky and foreground.

Mr. WARDLEY stated that these defects were entirely caused in the development, and had no connection with the character of the collodion used, or with the preparation of the plate. He considered that the imperfections were produced entirely by the repellent or nonmiscible nature of the solutions, containing acids and salts, used in development, acetic acid being one of the chief causes of the defects. Another source of the evil may be a low temperature and the developing solution being allowed to rest, even for a moment, on the plate. Such defects may be produced in abundance on any kind of dry plate if the developing solution is allowed to rest.

## Annual Meeting, April 30th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., F.C.S., &c., President, in the Chair.

Professor Joseph Henry, Secretary of the Smithsonian Institution, Washington, was elected an Honorary Member of the Society.

It was moved by Mr. BROTHERS, seconded by Mr. W. B. JOHNSON, and resolved unanimously,—"That the system of electing Sectional Associates be continued during the ensuing session."

The following Report of the Council was then read by one of the Secretaries:—

The Council again refer with satisfaction to the Treasurer's account, copies of which are already in the hands of the members. It will be seen that the balance in hand on the 1st of April, 1867, was £322 12s. 1d., against £269 4s. 3d. on the 1st of April, 1866.

The number of ordinary members on the roll of the Society on the 1st of April, 1866, was 183, and five new members have been elected during the year. The losses have been by resignations, four; defaulters, three; and one ordinary member elected an honorary member. The number of members on the roll on the 1st of April, 1867, was therefore 180.

It is a noteworthy circumstance that no deaths have occurred amongst the ordinary members during the year; but the Society has lost three of its honorary members—Professor Eilert Mitscherlich; Mr. John Mercer, F.R.S.; and William Hopkins, Esq., M.A., LL.D., F.R.S.; and two corresponding members, Mr. Benjamin Dockray and Professor A. D. Bache. One new honorary and two corresponding members have been elected during the year.

Mr. Mercer was a remarkable instance of a self-made man. Beginning the world without any advantages derived from education or easy circumstances, by indomitable energy and perseverance he attained a considerable position both as a man of science and a manufacturer. The best portion of his life was devoted to calico printing, to the advancement of which he contributed by the invention of many useful and ingenious processes. He was born February 21st, 1791, at Great Harwood, near Accrington. His father had a small cotton factory, and he himself worked in early youth at the loom. In 1807, being then 16 years old, he was much struck by the bright colour of a child's dress, which was an orange dyed with annotto. The wish to dye this and similar colours possessed him, but he did not know how to begin. In an outline of his life, drawn up by Mr. Mercer for his sons, he says:—"How to begin was my difficulty. There was no fancy dyer in our village; I had no acquaintance with any one who knew anything of dyeing, and I had no books that would help me. The villagers wore mostly dyed cotton at that time, and got them dyed at Blackburn. I therefore went to a druggist at Blackburn, and got a little of all the kinds of stuffs the dyers used, *with the names written on each*. Weavers at this time having plenty of cotton fents, I was thus well provided with all that was required, and by ringing changes and labelling all the results I was able to dye all the colours then in use. Not having convenience where I lived, I took a man to join me, who had a sufficient building for a dyehouse, with water near, and we dyed for the inhabitants of Great Harwood and the surrounding villages." From 1809–10 Mr. Mercer was employed in the works of Mr. Fort, calico printer, at Oakenshaw, near Harwood. During the following years he devoted himself with much ardour to the study of chemistry, to which his attention had been directed by the purchase at an old bookstall of a copy of "Parkinson's Pocket Chemistry." He then began to experiment on colours for calico printing, and at the end of 1817 had succeeded in applying some totally new colours to calico. Amongst these was the orange sulphuret of antimony, a colour so much brighter than the one up to that time dyed with annotto as to immediately attract the notice of the calico printers in the neighbourhood. His success led to his appointment as chemist in Mr. Fort's works, at Oakenshaw. In 1825 he was admitted a partner in the concern, and continued in this capacity till 1848, when the firm, having been very successful, gave up the business, Mr. Mercer retiring with a handsome fortune. He thenceforward devoted himself to theoretical chemistry, and was during later years in correspondence with many of the first chemists of the day.

It would be difficult to name all Mr. Mercer's inventions in connection with calico printing. It may suffice to mention those with which his name is chiefly associated. The long popular style of manganese brown, with white discharge and wood-colour illuminations, was his invention, and hundreds of thousands of pieces in this style must have been printed. He contributed largely to the chemistry of indigo dyeing, and invented an ingenious process for discharging indigo blue by means of alkali and red prussiate of potash. He was the original maker of dry stannate of soda, now so largely used by calico printers, Mr. Steiner having previously introduced the liquid stannate of potash, a much more costly article. He was the first to apply arseniates as a substitute for cow's dung in madder dyeing, phosphates having previously been employed for the same purpose. The chlorinating of woollen fabrics and mixed fabrics of wool and cotton, whereby a property of seizing and retaining colours with greater facility is given to wool, was, as all printers of these fabrics will admit, a step of very great importance, and had Mr. Mercer patented this invention a very large sum would probably have accrued to him. It was, however, given to the trade with no reservation whatever. To Mr. Mercer must be attributed the discovery of the peculiar action of caustic soda and sulphuric acid upon cotton. This singular process, now called "mercerizing" has the effect of untwisting the normally twisted flattened tubes of cotton filaments and converting them into cylindrical tubes. When colours are applied to the cotton so treated they pass more readily through the minute pores of the tubes and are precipitated in denser layers in the interior of the latter, whereby darker and more permanent shades are produced. Calico so treated has become greatly increased in strength, and though hitherto no large quantities of cloth thus prepared have been printed, owing to the expense of preparation, advantage has been taken of the process to prepare the cotton fabric used in the production af the endless web known to calico printers as the indiarubber blanket, which, when made with prepared calico, is rendered much more durable,

Mr. Mercer acted as a chemical juror for the Great Exhibition of 1851, as well as that of 1862. In 1852 he was elected a Fellow of the Royal Society. He was an honorary member of this Society as well as of the Glasgow Philosophical Society. He was put on the commission of the peace in 1861. Mr. Mercer closed a long period of useful labour on November 30th, 1866, after several years of suffering, borne by him with great fortitude and resignation. He had endeared himself to all who knew him by his simplicity of character, large-hearted kindness, and purity of life. To the poor his hand was always freely extended, and having himself been one of the people, he knew how to sympathise with their sorrows and give aid where most wanted. Though dying at an advanced age, his loss is felt by a large circle of friends as that of one whose powers of mind and virtues of heart were of no common order.

The following list of Papers and Communications read at the Ordinary and Sectional Meetings of the Society during the session now ending will show that there is no abatement in the scientific zeal and activity of the members and associates who contribute to the Society's Memoirs and Proceedings:—

*October 2nd*, 1866.—"On the Portraits of Sir Isaac Newton; and particularly on one of him by Kneller, painted about the time of the publication of the *Principia*, and representing him as he was in the prime of life," by Samuel Crompton, M.D.

*October 4th*, 1866.—"On a Process for Printing Photographs in various Colours," by Mr. J. A. Gatty.

*October 8th*, 1866.—"On the Plants Springing up Spontaneously on the fresh turning up of the Pasture Land at Knutsford, Cheshire," by Mr. H. A. Hurst.

*October 11th*, 1866.—"Observations of the Eclipse of the Sun, October 8th, 1866, at Mr. Worthington's Observatory, Crumpsall," by Joseph Baxendell, F.R.A.S.

*October 11th*, 1866.—"Results of a Comparison of the Magnitudes of the Bedford Catalogue with those of Sir John Herschel," by George Knott, F.R.A.S.

*October 11th*, 1866.—"Note on the Combined Magnitude of Two Stars in Close Proximity," by George Knott, F.R.A.S.

*October 11th*, 1866.—Catalogue of Binary Stars, with Introductory Remarks," by Alfred Brothers, F.R.A.S.

*October 16th*, 1866.—"On the Isomorphism of Thallium-perchlorate with the Potassium and Ammonium-perchlorates," by Professor H. E. Roscoe, F.R.S., &c.

*October 30th*, 1866.—"On the Manufacture of Sulphide of Ammonium," by Peter Spence, F.S.A., &c.

*November 8th*, 1866.—"Observations of the New Variable Star T Coronæ," by Joseph Baxendell, F.R.A.S.

*November 12th*, 1866.—"On Mosses New to Great Britain," by Mr. E. G. Hunt.

*November* 13*th*, 1866.—"On Echinus Lividus, Illustrated by Specimens from Roundstone," by Thomas Alcock, M.D.

*November* 13*th*, 1866.—"On the Structure of the Spines of Echini," by Mr. H. A. Hurst.

*November* 27*th*, 1866.—"Observations of the Meteoric Shower of November 13-14, 1866," by Joseph Baxendell, F.R.A.S.

*December* 6*th*, 1866.—"Observation of the Occultation of Aldebaran, November 22nd, 1866," by Alfred Brothers, F.R.A.S.

*December* 6*th*, 1866.—"On some Recent Results of Observations of the Specific Gravity of Sea Water," by Thomas Heelis, F.R.A.S.

*December* 11*th*, 1866.—"On the Recent Suspension by the Board of Trade of Cautionary Storm Warnings," by Joseph Baxendell, F.R.A.S.

*December* 11*th*, 1866.—"On two Fossil Specimens of a Species of Calamites from the Carboniferous Strata of Great Britain," by E. W. Binney, F.R.S., F.G.S.

*December* 11*th*, 1866.—"On the Occultation of Aldebaran by the Moon, January 16, and on the Eclipse of the Sun, March 5, 1867," by Mr. W. L. Dickinson.

*December* 11*th*, 1866.—"Notes on Varieties of Sarothamnus Scoparius and Stachys Betonica, from the Lizard, Cornwall," by Mr. Charles Bailey.

*December* 11*th*, 1866.—"Notes on Wood-feeding Coleoptera." by Mr. Joseph Sidebotham.

*December* 13*th*, 1866.—"On a New Frame for Printing Transparencies on Glass," by Mr. Coote.

*January* 3*rd*, 1867.—"On the Mean Weekly Temperature at Old Trafford, Manchester, for the Seventeen Years 1850–1866," by G. V. Vernon, F.R.A.S., F.M.S.

*January* 3*rd*, 1867.—"On the Rainfall for 1866, at Old Trafford, Manchester," by G. V. Vernon, F.R.A.S., F.M.S.

*January* 3*rd*, 1867.—"Results of Observations of Variable Stars, made at the Mannheim Observatory during the year 1866," by Dr. E. Schönfeld, Director of the Observatory.

*January* 7*th*, 1867.—"On Polymorphina Tubulosa," by Thomas Alcock, M.D.

*January* 8*th*, 1867.—"On Two Remarkable Fossil Insects from the Lower Coal Measures near Huddersfield," by E. W. Binney, F.R.S., F.G.S.

*January* 8*th*, 1867.—"On the Amount of Carbonic Acid contained in Sea Air," by Mr. T. E. Thorpe.

*January* 8*th*, 1867.—"On the Amount of Carbonic Acid contained in the Atmosphere of Tropical Brazils during the Rainy Season," by Mr. T. E. Thorpe.

*January* 10*th*, 1867.—"On 'Braun's' Photographic Copies of Drawings of the Old Masters," by J. B. Dancer, F.R.A.S.

*January* 10*th*, 1867.—"On Mr. Mercer's Process of Printing Photographs in various Colours," by J. B. Dancer, F.R.A.S.

*January* 22*nd*, 1867.—"On Casting, Grinding, and Polishing Specula for Reflecting Telescopes (Part II.)," by James Nasmyth, C.E., Corresponding Member of the Society.

*January* 22*nd*, 1867.—"On Storm Warnings," by Dr. Buys Ballot.

*January* 31*st*, 1867.—"Results of Raingauge, Anemometer, and Ozone Observations, made at Eccles, near Manchester, during the year 1866," by Thomas Mackereth, F.R.A.S., F.M.S.

*January* 31*st*, 1867.—"Observation of the Occultation of Aldebaran, January 16th, 1867, at Mr. Worthington's Observatory," by Joseph Baxendell, F.R.A.S.

*January* 31*st*, 1867.—"Observation of the Occultation of Aldebaran, January 16th, 1867," by Alfred Brothers, F.R.A.S.

*January* 31*st*, 1867.—"On the Elements of the Variable Star R Persei," by Joseph Baxendell, F.R.A.S.

*January* 31*st*, 1867.—"On the Variable Star R Vulpeculæ," by George Knott, F.R.A.S.

*February* 5*th*, 1867.—"On Barometric Waves," by Sir J. F. W. Herschel, Bart., M.A., F.R.S., &c., Honorary Member of the Society.

*February* 19*th*, 1867.—"On Siemen's and Wheatstone's Magneto-electric Machines," by Mr. Henry Wilde.

*February* 19*th*, 1867.—"On the Origination of the System of Issuing Storm Warnings," by Mr. T. H. Babington.

*February* 19*th*, 1867.—"On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes (Part III.)," by James Nasmyth, C.E., Corresponding Member of the Society.

*February 20th*, 1867.—"On the Reversed Action of Light in Photography," by Mr. Joseph Sidebotham.

*March 5th*, 1867.—"On a Section of the Drift and the Underlying Triassic and Coal Measures at Ardwick," by E. W. Binney, F.R.S., F.G.S.

*March 5th*, 1867.—"On Some of the Conditions of Molecular Action," by Arthur Ransome, M.B., M.A., Cantab.

*March 12th*, 1867.—"On Photographs taken during the Eclipse of the Sun, March 6th, 1867," by Alfred Brothers, F.R.A.S.

*March 19th*, 1867.—"Observations of the Eclipse of the Sun, March 6th, 1867," by J. B. Dancer, F.R.A.S.

*March 19th*, 1867.—"On an Apparatus for Determining the Horizontal Magnetic Intensity in Absolute Measure," by J. P. Joule, LL.D., F.R.S., &c., V.P.

*March* 19th, 1867.—"Note on the Tangent Galvanometer," by J. P. Joule, LL.D., F.R.S., &c., V.P.

*March 19th*, 1867.—"On a Section of the Drift Deposits in the Banks of the Ribble, near Balderston Hall," by Edward Hull, B.A., F.G.S.

*April 2nd*, 1867.—"On the Microscopical Examination of Coal Ash or Dust from the Flue of a Furnace, Illustrated by the Microscope," by J. B. Dancer, F.R.A.S.

*April 2nd*, 1867.—"On the Galvanometer," by William Jack, M.A., Professor of Natural Philosophy at Owens College.

*April 2nd*, 1867.—"Additional Note on the Galvanometer," by J. P. Joule, LL.D., F.R.S., &c., V.P.

*April 2nd*, 1867.—"Notes on the Smallest and Simplest Forms of Matter and its Relations to Mechanical Forces," by Mr. J. C. Dyer, V.P.

*April 2nd*, 1867.—"Variable Star Ephemeris for 1867, calculated from the Elements given in Dr. Schönfeld's Catalogue," by J. R. Hind, F.R.S., Superintendent of the "Nautical Almanac," and Honorary Member of the Society.

*April 9th*, 1867.—"Note on Photography in 1787," by Alfred Brothers, F.R.A.S.

*April 16th*, 1867.—"On a New Form of the Dynamic Method for Measuring the Magnetic Dip," by Sir William Thomson, M.A., D.C.L., F.R.S., &c., Honorary Member of the Society.

*April 16th*, 1867.—"Further Remarks on the Galvanometer," by William Jack, M.A., Professor of Natural Philosophy at Owens College.

*April 16th*, 1867.—"Observations on the Alteration of the Freezing Point in Thermometers," by J. P. Joule, LL.D., F.R.S., &c., V.P.

*April 16th*, 1867.—"On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes (Part IV.,)" by James Nasmyth, C.E., Corresponding Member of the Society.

*April 22nd*, 1867.—"On Foraminifera," by Mr. James Linton.

*April 22nd*, 1867.—"On Ashes from Furnace Flues," by Mr. Joseph Sidebotham.

Many of these papers will appear in the forthcoming volume of Memoirs, the printing of which is far advanced, and is expected to be completed before the commencement of the ensuing session.

The system of admitting Sectional Associates has continued to work satisfactorily, and the Council have therefore resolved to recommend its renewal for another year.

The Librarian reports that the Society continues to be in regular receipt of the usual Transactions, Memoirs, &c., issued by the various learned bodies with which the Society is in correspondence. The number of societies, &c., exchanging their publications with this Society is now 256, 74 of which are British and 182 foreign, or an increase of 5 British and 7 foreign upon the number of last year.

Since the date of the last Report the attention of the Librarian has been chiefly given to the acquisition of volumes, parts, &c., which have been wanting for some time past, and it is gratifying to be able to state that unusually large and valuable donations of such desiderata have been added to the Library in the course of the past year. The Philosophical Transactions for 1845 and 1846, referred to in Vol. III. of Proceedings, p. 278, as having been lost, and many other Journals, &c., entered in the catalogue as wanting, have been replaced in this way.

The number of works accumulating in the Library has necessitated the enlargement of one of the book-cases, and the time is not distant when additional space will be required for the increasing number of publications which are coming to hand daily.

The following is a list of serial publications which the Librarian is authorized to purchase for the Library:—

The London, Edinburgh, and Dublin Philosophical Magazine.
The Annals and Magazine of Natural History.
The Quarterly Journal of Science.
The Zoologist.
The Publications of the Palæontographical Society.
„ „ „ Ray Society.
„ „ „ Cavendish Society.
The Quarterly Journal of Pure and Applied Mathematics.
Les Annales de Chimie et de Physique.
Les Annales des Sciences Naturelles; Zoologie et Botanique.
Le Journal de Pharmacie et de Chimie.
Les Archives des Sciences Physiques et Naturelles.
Prodromus systematis universalis regni vegetabilis, par Alphonso de Candolle.
Die Astronomische Nachrichten.
Die Annalen der Physik und Chemie.
Die Annalen der Chemie und Pharmacie.
Die Journal fur die reine und angewandte Mathematik.
Neues Jahrbuch fur Mineralogie, Geologie, und Palæontologie
Die Zeitschrift fur wissenschaftliche Zoologie.
Allgemeine Encyklopädie der Physik.

On the motion of Mr. Carrick, seconded by Mr. Brothers, the Annual Report was unanimously adopted.

The following gentlemen were elected officers of the Society for the ensuing year:—

### President.

EDWARD SCHUNCK, Ph.D., F.R.S., F.C.S.

### Vice-Presidents.

ROBERT ANGUS SMITH, Ph.D., F.R.S., F.C.S., &c.
JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., &c.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
JOSEPH CHESBOROUGH DYER.

### Secretaries.

HENRY ENFIELD ROSCOE, B.A., Ph.D., F.R.S., F.C.S.
JOSEPH BAXENDELL, F.R.A.S.

### Treasurer.

ROBERT WORTHINGTON, F.R.A.S.

### Librarian.

CHARLES BAILEY.

### Other Members of the Council.

REV. WILLIAM GASKELL, M.A.
PETER SPENCE, F.C.S., M.S.A.
GEORGE VENABLES VERNON, F.R.A.S., F.M.S.
JOHN BENJAMIN DANCER, F.R.A.S.
WILLIAM JACK, M.A.
WILLIAM LEESON DICKINSON.

## ROBERT WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER,

## FROM 1ST APRIL, 1866, TO 31ST MARCH, 1867.

**Dr.**

| | | £ | s. | d. | £ | s. | d. |
|---|---|---|---|---|---|---|---|
| 1866. | | | | | | | |
| April 1. | —To Balance in Bank of Heywood Brothers and Co. | 267 | 2 | 3 | | | |
| " | " " Treasurer's hands | 2 | 2 | 0 | | | |
| | | | | | 269 | 4 | 3 |
| 1867. | | | | | | | |
| Mar. 31. | —To *Members' Contributions:* | | | | | | |
| | Arrears, 1864-5 ... 1.. | 2 | 2 | 0 | | | |
| | " 1865-6 ... 18.. | 37 | 16 | 0 | | | |
| | " Half Subscription, 1866 ... 1.. | 1 | 1 | 0 | | | |
| | " Admission Fees, 1866 ... 3.. | 6 | 6 | 0 | | | |
| | Members on the Roll, April 1, 1866, 183 at 42s. .. £384 6 0 | | | | | | |
| | Deduct Compounders ... 3<br>" Honorary Member ... 1<br>" In Arrear, March 31, 1867 ... 18<br>22 } 46 4 0 | | | | | | |
| | | 338 | 2 | 0 | | | |
| | Three Members elected in 1866, April—December | 6 | 6 | 0 | | | |
| | Three Admission Fees | 6 | 6 | 0 | | | |
| | Two Readmissions | 12 | 12 | 0 | | | |
| | Eleven Associates of Microscopical Section, at 10s. | 5 | 10 | 0 | | | |
| | Two ditto Arrears ditto at 10s. | 1 | 0 | 0 | | | |
| | Eight ditto of Photographic Section, at 10s. 6d. | 4 | 4 | 0 | | | |
| | One ditto of Physical and Mathematical Section, at 10s. 6d. | 0 | 10 | 6 | | | |
| | | | | | 421 | 15 | 6 |
| " " | To *Sale of Publications:* | | | | | | |
| | Memoirs— Ser. 2, Vol. XV. | 0 | 7 | 6 | | | |
| | " Ser. 3, Vol. I. | 0 | 15 | 9 | | | |
| | " Ser. 3, Vol. II. | 0 | 17 | 6 | | | |
| | " Ser. 3, Vol. VIII. | 0 | 7 | 0 | | | |
| | Proceedings | 1 | 2 | 4 | | | |
| | Memoir of Dalton | 0 | 3 | 6 | | | |
| | | | | | 3 | 13 | 7 |
| " " | To *Sundry Income:* | | | | | | |
| | Sec. Contrib. Microscopical ... £4 4 0 | | | | | | |
| | " " Photographic ... 2 2 0 | | | | | | |
| | " " Physical and Mathematical ... 2 2 0 | | | | | | |
| | | 8 | 8 | 0 | | | |
| " " | Interest allowed by Bankers | 13 | 6 | 4 | | | |
| | | | | | 21 | 14 | 4 |
| | | | | | £716 | 7 | 8 |

| | £ | s. | d. |
|---|---|---|---|
| Composition Fund | £72 | 10 | 0 |
| General Balance | 250 | 2 | 1 |
| | £322 | 12 | 1 |

**Cr.**

| | | £ | s. | d. | £ | s. | d. |
|---|---|---|---|---|---|---|---|
| Mar. 31, 1867. | | | | | | | |
| By | *Charges on Property:* | | | | | | |
| | Chief Rent | 12 | 14 | 0 | | | |
| | Fire Insurance | 5 | 12 | 6 | | | |
| | Property Tax | 2 | 16 | 8 | | | |
| | Inhabited House Duty | 6 | 7 | 6 | | | |
| | | | | | 27 | 10 | 8 |
| " | *House Expenditure:* | | | | | | |
| | Water, Gas, Candles, and Coal | 19 | 2 | 5 | | | |
| | Cleaning and Petty Expenses | 2 | 15 | 9 | | | |
| | Bookcase and Furniture renewed, &c. | 26 | 11 | 0 | | | |
| | Repairs | 2 | 17 | 3 | | | |
| | Tea and Coffee at Meetings | 17 | 12 | 8 | | | |
| | | | | | 68 | 19 | 1 |
| " | *Administrative Charges:* | | | | | | |
| | Wages—Keeper of Rooms | 49 | 4 | 0 | | | |
| | Attendance on Sections | 8 | 8 | 0 | | | |
| | Postage and Parcels | 29 | 0 | 9 | | | |
| | Stationery, Printing Circulars, &c., and Receipt Stamps | 10 | 12 | 6 | | | |
| | | | | | 97 | 5 | 3 |
| " | *Publishing:* | | | | | | |
| | Memoirs—Printing, Engraving, &c. | 80 | 10 | 0 | | | |
| | Printing Proceedings | 48 | 7 | 0 | | | |
| | Editor of Memoirs and Proceedings | 50 | 0 | 0 | | | |
| | | | | | 178 | 17 | 0 |
| " | *Library:* | | | | | | |
| | Periodicals, &c. | 18 | 16 | 7 | | | |
| | Subscription to Palæontographical Society | 1 | 1 | 0 | | | |
| | " Ray Society | 1 | 1 | 0 | | | |
| | | | | | 20 | 18 | 7 |
| " | *Sundry Expenditure:* | | | | | | |
| | Returned Honorary Member for Vol. of Memoirs charged in error | | | | 0 | 5 | 0 |
| | Total Disbursements | | | | 393 | 15 | 7 |
| | Balance in Bank of Heywood Brothers and Co. | 314 | 9 | 1 | | | |
| | " " Treasurer's hands | 8 | 3 | 0 | | | |
| | | | | | 322 | 12 | 1 |
| | | | | | £716 | 7 | 8 |

2nd April, 1866.
ROBERT WORTHINGTON, TREASURER.

Examined and found correct, 12th April, 1867.
GEO. CLIFF LOWE, } AUDITORS.
W. L. DICKINSON, }

"On the Galvanometer" by **William Jack, M.A.**, Professor of Natural Philosophy at Owens College.

The author, referring to his communication entitled "Further Remarks on the Galvanometer," printed in the last number of the Society's Proceedings, points out that a serious misprint had occurred on page 159, where in the third line $n+i$ should be $n$ and $i$. He then suggests that the use of the formula he had given would afford a very satisfactory method of determining the magnetic length of a bar magnet. Thus for $n=\frac{213}{315}$, we should have a negative result for any intensity between such as will make $\sin^2 a_1=\frac{105\cdot3}{213}$ and $\sin^2 a_1=\frac{116\cdot7}{213}$. Thus, if the very first entire length of magnet for which this reversal of effect takes place (trying all possible intensities) be L, since $\frac{l^2}{r^2}>\frac{212\cdot1}{315}$ and $\frac{L^2}{r^2}=a$, a known fraction $\frac{l^2}{L^2}$ must be $>\frac{212\cdot1}{315}\,\frac{1}{a}$, and as I have shown above that it is probably very possible to obtain by sufficient care instead of the vague $>\frac{212\cdot1}{315}$ something which is at least between $\frac{212\cdot1}{315}$ and $\frac{213}{315}$ $\frac{l^2}{L^2}$ can be determined with great accuracy and $\frac{l}{L}$ will lie between $\sqrt{\frac{212\cdot1}{315}\cdot\frac{1}{a}}$ and $\sqrt{\frac{213}{315}\cdot\frac{1}{a}}$. Probably in practice much narrower limits might be found determinable, with ordinary values of $i$. Thus even for $n=212\cdot2$, $x=\sin^2 a$ would lie between $\frac{111\cdot13}{212\cdot2}$ and $\frac{110\cdot33}{212\cdot2}$ which gives a fair range for $a$, so that it is unlikely that a good experimenter, carefully raising or lowering the intensities, would fail to observe the change in sign of deviation which corresponds.

## STORM WARNINGS.

Mr. BAXENDELL, in reply to a question from Mr. Brothers, F.R.A.S., stated that General Sabine had not yet offered any reply to the letter he had addressed to him on the 26th March commenting upon the proceedings of the President and Council of the Royal Society in reference to the Meteorological Department of the Board of Trade and the suspension of storm warnings. It was evident that these proceedings would not bear examination and discussion, and as a consequence, no confidence could now be placed in the so-called "Scientific Committee," which had been appointed by the President and Council, to superintend the Meteorological Department. This committee had shown itself to be utterly regardless of public opinion and feeling, and quite unfitted to carry out efficiently the duties which had been so ably and so usefully discharged by the late Admiral Fitzroy and his assistant Mr. Babington; and in the interests of science, commerce, and humanity it was to be hoped that the Board of Trade would again take the management of the Meteorological Department into their own hands, and appoint Mr. Babington, or some other competent and responsible person, to resume and carry on the system of storm warnings.

As an illustration of the lamentable consequences of the suspension of storm signals it might be stated that among the wrecks caused by the unusually heavy gale of the 10th of April there were four vessels which had sailed from Liverpool just before the commencement of the storm. Three of these vessels had been thrown on the Lancashire coast and totally lost, and the lives of three of the crew of one vessel had been sacrificed. The fourth vessel had foundered in the Irish Sea and all hands were lost. Now the meteorological phenomena immediately preceding the occurrence of this storm were of such a nature that there could have been no difficulty in giving notice of its approach in ample time to prevent the sailing of these vessels. In fact, without the aid of telegrams from distant stations, its approach was confidently announced in Manchester so early as the evening of Monday the 8th of April, and the direction in which the wind would blow during the most violent period of the storm was also stated at the same time. The grounds of this prediction were explained, and it was shown that even if the rules of meteorology were not yet established "on a strictly scientific basis," the plea put forward by the President and Council of the Royal Society for discontinuing storm signals could only be regarded as frivolous. It is understood that the expense of the Meteorological Department, under the new management, is now more than double the amount it cost under the management of the late Admiral Fitzroy, and yet the most important and practically useful portion of its duties, and that which involved the greatest current expense, has been given up; and while even the Scientific Committee cannot deny that our present knowledge of the laws of meteorological phenomena affords us the means of saving immense amounts of valuable property and many valuable lives, yet this committee has nevertheless presumed to deprive the public of the advantages to be derived from this knowledge, and has not hesitated to divert the funds which have hitherto been so usefully applied in the interests of commerce and humanity, to the furtherance of schemes and scientific crotchets which are altogether uncalled for by the requirements of meteorological science, and which, however interesting and important they may be to the parties immediately concerned, have certainly no interest whatever for the general public. Surely this is a state of things which calls for the interference of those who are charged with the power and responsibility of controlling the expenditure of the public money.

Mr. **Baxendell** also called attention to the increasing number of vessels which were reported as having foundered at sea, and Mr. **Binney** suggested that it might be owing to the greatly increased length of modern built ships, especially those constructed of iron.

Mr. W. B. **Johnson** said, the fact referred to by Mr. Baxendell admitted of easy explanation. The materials now used in the building of iron ships were greatly inferior, both in strength and quality, to those used in ships built some years ago. In some cases, where a builder formerly employed iron plates three-eighths of an inch thick he now contented himself with plates only three-sixteenths of an inch in thickness, and the strength of the angle-irons, &c., was reduced in the same proportion. It was no exaggeration to say that the weight of the iron now used in building a vessel of a given tonnage was fully one-third less than was generally considered necessary some years ago; and besides this, the iron now used for ship's plates was notoriously of very inferior quality. No recently built iron ship could be found that would bear the bumping and straining which the "Great Britain," built a quarter of a century ago, bore in Dundrum Bay.

## MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 25th, 1867.

A. G. LATHAM, Esq., President of the Section, in the Chair.

Mr. COWARD exhibited specimens of Calluna vulgaris from Newfoundland, with the following memorandum, in the handwriting of the late Robert Brown, on a ticket attached: "Erica vulgaris Linn. at the head of Trepassey and St. Mary's Bay on the south-east coast of Newfoundland, covering considerable tracts. Mr. Mc. Cormack received from Mr. George Don, June 3rd, 1830. In the collection were three specimens, none of them in fructification.—R. B."

Dr. Seemann (*Jour. Bot.*, Oct., 1866) thinks the Newfoundland Heath may prove to be a distinct species, but Dr. Asa Gray considers it identical with the British one. These specimens favour the latter view as far as foliage is concerned.

The Rev. J. E. VIZE exhibited and described the hairs of a foreign Lepidopterous caterpillar. He said the specimen had remained for many years in spirit, and no record had been preserved with it; it was evidently not British, but he had hitherto been unable to identify it with any described foreign species in books to which he had access.

The body was covered with very large compound hairs which gave the entire creature the appearance of a tuft of moss, and might possibly serve for its protection by the likeness to a vegetable growth. The unusual and peculiar development of these hairs suggested a microscopical examination of them, and the result had been interesting as showing that, whether or not the entire mass of the compound hairs serve for protection by concealment, the complex structure of their points, which are capable of protrusion and have each a bag or gland at their base contained within the tubular branch from which they project, fits them to act as most efficient weapons of offence.

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March 25th, 1867.

J. SIDEBOTHAM, Esq., in the Chair.

Mr. HURST said he had received a letter from Mr. R. G. BREAREY, dated February 16th, Douglas, Isle of Man, referring to three living specimens of Echinis sphæra, kindly sent by him, and received here, in good condition. The attempt had been made to keep them alive in artificial sea water, but after several days they appeared to be dying, and were put in spirit. They are now at the service of any members who wish to have them for microscopic examination.

Mr. Brearey mentions in his letter that he had asked the man who procured the specimens for him what he knew of the habits of the Echini, and was told that they walk by by means of their spines, and feed upon any animal matter living or dead; also that they are sometimes eaten boiled, and taste like the roe of the codfish.

The thanks of the meeting were voted to Mr. Brearey for his communication and for the accompanying specimens.

Mr. DANCER, F.R.A.S., read a paper "On the Microscopical Examination of Coal Ash or Dust from the Flue of a Furnace, illustrated by the Microscope." [This paper was afterwards read at the ordinary meeting of the Society, held April 2nd, 1867. See page 143.]

Mr. C. BAILEY exhibited the curious fruits of Pawlownia imperialis, really a native of Japan, but found wild in the woods near New York. Mr. Bailey presented a specimen slide to the Section.

Mr. WATSON read a paper entitled "Further Remarks on the Plumules or Battledore Scales of some of the Lepidoptera, with illustrations by Mr. Sidebotham."

April 22nd, 1867.

ARTHUR G. LATHAM, Esq., President of the Section, in the Chair.

Mr. SIDEBOTHAM exhibited and described specimens of coal ash from the furnace flues of the Strines Printing Company, to which his attention had been drawn by Mr. Dancer's paper.

They consisted in a great part of spherical bodies almost exactly simulating "shot and shell" on a small scale, the aperture for charging the shell bearing a singular resemblance to that in real shell; some of the "shot" were almost the size of dust shot. He stated that many cwt. were annually carted away from these flues, and suggested that a small iron mine had been discovered.

Although under examination by the microscope these bodies reflected distant objects very perfectly, especially when slightly above the focus, their surfaces were much corrugated, and the high polish they apparently possessed when out of focus was shown to be entirely illusive when the surface was accurately examined.

Mr. NASMYTH, to whom he had sent specimens, compared these bodies to the planets (on the nebular hypothesis) thrown off centres of intense heat.

Mr. LINTON read the following paper "On a sample of sand from Dogs Bay, Connemara, skimmed from the surface of the sea by Mr. Glover, in July, 1866."

While examining various samples of sand brought at different times from Dogs Bay it has frequently occurred to me that each packet is very perceptibly different from the others in the forms of Foraminifera which preponderate, and that, while certain forms are common in some of the samples, they appear to be altogether absent from others. I have very little doubt that these differences depend on the time of year at which such gatherings were made, and whether they were taken from the shore or skimmed from the surface of the sea, those obtained by the surface skimming always yielding the most perfect specimens. The samples which I had before examined were none of them at all to be compared for richness with that obtained by the late Mr. Parry by skimming the surface of a piece of water on the higher part of the shore, and to which he gave the name of Burns' Pool. I have prepared a slide containing all the forms found in it, and this I have now brought for exhibition.

Mr. Glover kindly collected fresh samples of sand for me in July last, and again in December. The latter gathering was from the shore, and though it contained a great variety of objects, the more delicate of them were in many cases broken or worn. The July sample, obtained by skimming from the surface of the sea, and unfortunately procurable only in very small quantity, is that to which I wish particularly to draw your attention, as it consists of more perfect and beautiful specimens than I have seen before. I now show these specimens mounted, and present the following list of them. Before concluding I would remark, that it is very desirable for members examining Foraminifera to keep a record, not only of the place, but of the time of year, when their sand was collected.

### *List of Foraminifera from Dogs Bay.*

Lagena vulgaris typica. None.
„ „ var: antiqua. Very frequent.
„ „ var: clavata. Not common but very perfect.
„ „ var: perlucida. Very fine.
„ „ var: semistriata. Very fine and perfect.
„ „ var: striata } … { Very common, several double and many deformed specimens.
„ „ var: interrupta }
„ „ var: gracilis. None.
„ „ var: substriata. Very fine; several varieties, among them one with the striæ extending spirally up the neck.
Entosolenia globosa var: lineata. Not common.
„ costata. Common.
„ marginata. Very plentiful.
„ Williamsoni. Very plentiful.
„ squamosa typica. Very plentiful and much varied in form.
„ „ var: scalariformis. Not common.
„ „ var: catenulata. Not common.
„ „ var: hexagona. Common.
Lingulina. None.
Nodosaria radicula. Occurs sparingly, but perfect.
„ pyrula. None.
Dentalina. None.
Frondicularia. None.
Cristellaria subarcuatula. Rare; no other form of Cristellaria occurs.

Nonionina Jeffreysii. Rather common.

Polystomella crispa. Very common and fine. Some specimens though large have spines like those of the young shell; the same is the case with specimens I have obtained from sand from the Dardanelles.

" umbilicatula. Rare.

Peneroplis. None.

Patellina corrugata. Small, but very perfect.

Rotalina. The sand contains all the varieties excepting ochoracea, inflata and turgida. R. oblonga is fine and plentiful and is very varied in form.

Globigerina bulloides. Common.

Orbulina universa. Very large.

Planorbulina vulgaris. Very plentiful.

Truncatulina lobata. Common.

Bulimina pupoides var: marginata. Rare.

Uvigerina. None.

Cassidulina. Not common.

Polymorphina. All the varieties plentiful.

Textularia cuneiformis. Not common.

Biloculina ringens var: carinata. Rather common.

Spiroloculina depressa. Frequent.

Miliolina. All the varieties.

Spirilina. None.

Dr. CARRINGTON read the following paper "On two Hepaticæ New to Britain."

Jungermannia saxicola, Schrad.

Stems ascending, rigid, dichotomously branched, sparingly radiculose, sub-terete, examphigastriate.

Leaves bifariously imbricated, uniform, unequally bi-lobed, conduplicate, inferior lobe rotundate ovate, obtuse, or shortly pointed, concave, arching forwards; sinus deep, gibbous; dorsal lobe smaller, incumbent, cuneate, clasping the stem. Fruit terminal, or from the growth of innovations axillary, involucral leaves 2-4-fid, lobes unequal, repand-serrate; perianth ovate, 6-plicate, the apex lacinulate-dentate.

*J. saxicola, Schrader Sammlung cryptogamischer Gewächse 1796-7, Syn. Hepat. p. 119.*

*J. resupinata, Lin. Fl. Suecica, ed. 1, 1745 (excl. syn. Dillen.)*

This fine species was discovered June, 1864, at Ronas Hill, Shetland, by A. Mc. Kinlay, Esq. In general habit J. saxicola resembles J. minuta, but the leaves are more closely imbricated, the anterior lobe smaller, and the posterior curved forwards, so that the back of the frond looks terete. The leaves are of firm texture, olive-brown, scarcely altered when dry, areolation dotted.

J. saxicola is not unfrequent in Norway and Sweden, and is also found in alpine districts through Europe; so that I have long looked for it from Scotland. Some years ago the late Dr. Greville was kind enough to lend me his duplicates of Hepaticæ, and in a paper with J. orcadensis from the Southerland mountains, I found two stems of J. saxicola, as if they had been picked out of some tuft; but as there were European specimens in the same packet, I thought they might have got there accidentally.

The synonymy of J. saxicola was long in a confused state. Linnæus, in the Flora Suecica, confounded it with J. resupinata *Dillenius*, which was probably Scap. œquiloba.

S. capania Bartlingii N. ab E.

*Plagiochila Bartlingii M. et N. Hep. Eur. III. p. 520.*

Stems short, creeping at the base.

Leaves patent, conduplicate, amplexicant, the lobes narrowly ovate, entire, cuspidate, those of the upper leaves obtuse, sinus shallow, gibbous, margin undulate.

Perichdyætium obovate, compressed, mouth truncate, entire.

*J. cuspiduligera N. ab E. Hep. Eur. I. p. 180.*

Stems ½in. to 1in. high, fasciculately branched, leaves pale green, the lobes equal, both short, with acute points, the sinus reaches only about ¼th of the leaf, base of the leaf clasping the stem, margin entire, repand-undulate, and looking somewhat crisped when dry.

I first collected this species April, 1858, by the Strid, Bolton Woods, Yorkshire, and more recently have received specimens from Mr. Mudd, from Teesdale.

In general habit J. Bartlingii resembles some aquatic forms of S. arcosey, Ehrharti (J. emarginata), more than a S. capania. The lobes are more nearly equal, and the sinus shallower, than in any other species with which I am acquainted, and are not recurved.

From J. æquiloba it may be distinguished by the entire leaves—those of J. æquiloba are coarsely toothed, and the anterior lobe, except in the apical leaves, smaller than the under one.

From S. compacta it may be known by the lobes in that species being nearly round,—whereas in J. Bartlingii they are ovate, acute, and undulate.

Dr. ALCOCK read a paper "On the hair of the Musk-deer."

He said that he found no full account of it in the books he had referred to, though most of its characters were separately mentioned by different authors.

The hair is about two inches long, thick, rigid, brittle, without elasticity, and scarcely at all flexible; it has nearly a uniform thickness for three-fourths of its length from the base, and then gently tapers to a fine point; its section is not quite circular but slightly flattened on two sides, the other sides having a wavy outline in consequence of the hair being bent alternately in opposite directions; in the length of the hair there are about fourteen of these bendings, they are most strongly marked in the middle third, at which part the extent of the alternate lateral divergences is about equal to the thickness of the hair, but they are absent for some distance both from the base and tip. The bulb is glassy and transparent, showing no structure, flask-shaped, and joined to the body of the hair by a rather long, slender, transparent neck. All this glassy part is embedded in the skin, and, at the point where the hair projects beyond, it suddenly swells out to its full thickness, forming a hemispherical base. A remarkable character of these hairs is their singular lightness, which may be seen by letting them fall from the hand, when they float through the air like feathers. With the exception of the bulb, the entire hair has a cellular structure, and what might be called its cortical portion is nothing more than a surface-layer of small polygonal cells without any appearance of horny substance, while all the inner parts consist of a very coarse cellular structure in which no regularly-shaped cells can be seen, but there is rather the appearance of shapeless cavities separated by very thin membranous partitions. This inner structure is so delicate that the hair can be crushed in and flattened by very slight pressure, though there can be no doubt that its strength is greatly increased by the wavy form already mentioned, this being an arrangement similar to the ridge-and-furrow of iron structures, which is so important an element of their strength. Dr. Carpenter, in his work on the Microscope, mentions that the hair of the Reindeer has a very similar structure to that of the Musk; it appears, in fact, to be identical in plan, and this complete cellular structure, so different from that of ordinary hair, is found not only in the Reindeer, but in the common Stag, the Rusa Deer, and the Roebuck, that of the Roebuck showing the nearest approach in general character to the hair of the Musk-deer. With regard to the Caribou, which may be presumed to have hair of similar structure to that of the European Reindeer, the statement of Dr. Richardson that the skin dressed with the hair on forms a clothing so impervious to cold that a person dressed in a suit of this material and wrapped in a mantle of the same, may safely pass the night in the snow during the intensity of an arctic winter, throws light on the importance of this cellular hair to animals fitted for living in cold climates and at the same time having a constitution which requires them to be provided with a very warm external covering. The whole interior of these cellular hairs is occupied by air, which, as is well known, forms the best possible non-conductor of heat, and, in considering their efficiency for this purpose, the manner in which they are implanted in the skin should be noticed. In the Musk-deer there appears to be absolutely no fine wool or under-fur on the back and sides (which were the only parts examined), and the thick cellular hairs are so closely set in the skin that they necessarily stand out from it like the spines of a hedgehog, the consequence being that the thickness of the warm covering is not much less than the entire length of the hairs; and it should be further noticed that their effect is increased by the uniform thickness which they preserve for so great a proportion of their length, and also by the wavy form already described, which allows them to fit, as it were, to one another.

In the Roebuck there are a few slender hairs having the ordinary appearance of under-fur scattered among the large, rigid, cellular hairs which are seen standing out like quills; these latter are of the same length as in the Musk, though not quite so thick, but they are still so closely set in the skin that they form a warm covering on the same principle. In the true Deer which have been found to possess these wavy cellular hairs the perfect development, as seen in the Musk, is further departed from, and they become longer in proportion to their thickness, wiry, and more scattered, and at the same time mixed with a greater quantity of under-fur.

The **President** stated that, in addition to the Foraminifera found in Dogs Bay Sand, he had noticed the following objects:

Seeds.
Parts of Insects.
Echini spines, disks, pedecellariæ and shell.
Portions of Zoophytes.
Opaque and transparent globes not yet identified.

Mr. **Sidebotham** exhibited fossils from the plateau of the Great Pyramid, among them a very perfect Echinus.

Dr. **Alcock** exhibited a number of bones from a cave in North Wales, which, on examination, he had identified as belonging to the common frog.

A Letter was read from Mr. **Hepworth**, accompanying lithographs of the magnified Male Itch Insects and Cancer Cells, for distribution among the members, and the thanks of the meeting were accorded to him.

## Annual Meeting, May 6th, 1867.

The Annual Report having been read by the Secretary, was unanimously adopted, and ordered to be printed in the usual form and circulated among the members.

The following Officers were elected for the ensuing Session:—

**President:**

JOHN B. DANCER, F.R.A.S.

**Vice-Presidents:**

ARTHUR G. LATHAM.
JOSEPH SIDEBOTHAM.
JOSEPH BAXENDELL, F.R.A.S.

**Secretaries:**

HENRY ALEXANDER HURST.
THOMAS ALCOCK, M.D.

Treasurer:

JAMES G. LYNDE, F.G.S., F.R.M.S., M.Inst.C.E.

Of the Council:

W. C. WILLIAMSON, F.R.A.S.
JOHN WATSON.
ALFRED BROTHERS, F.R.A.S.
SAMUEL COTTAM.
CHARLES BAILEY.
THOMAS NEVILL.
E. W. BINNEY, F.R.S., F.G.S.
THOMAS COWARD.

The Rev. J. E. Vize read a paper "On Testacella."

He described the general appearance of this slug, and gave an account of its habits as he had observed them in the neighbourhood of Trowbridge, where it is common. He also exhibited several mounted tongues of Testacella, and pointed out the peculiar characters of the teeth.

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## July 18th, 1867.

J. B. Dancer, F.R.A.S., President of the Section, in the Chair.

"Some Further Observations on the Cause of Rotation in the Cells of Vallisneria," by James G. Lynde, F.G.S., F.R.M.S.

In a paper read by me at a meeting of the Section on the 16th February, 1863, "On the Action of Magenta Dye upon Vegetable Tissue," I described a series of experiments upon cuttings of Vallisneria, made chiefly with a view to ascertain, if possible, the cause of the rotation of the chlorophyl vescicles within the cells.

I then concluded that the rotation was due to the action of cilia on the inner surface of the cell wall, and was confirmed in this opinion not only by the appearance of the luminous stratum or so-called ciliary wave which had been observed by Dr. Branson, Mr. Wenham, and others, but also by the appearance on the cell walls of certain markings revealed by the action of the dye on the suspension of the vital action.

The above experiment was exhibited to the Section, and many of the members present attributed these markings, as I had done, to the presence of cilia.

I have since, from time to time, pursued my experiments on the subject in the hope that I might be able to adduce more positive evidence as to the cause of the wave of light on the interior of the cell wall.

After many fruitless experiments I at length determined to try the effect of polarized light, and on the application of it with a ⅛th-inch objective, having an aperture of 130° to 162°, and so arranging Darker's series of selenite plates as to give a dark blue ground, there appeared over the surface of all the cells brilliant gold-coloured scintillations which had all the appearance of cilia in motion.

The portion of leaf under examination exhibited very sluggish circulation, and was therefore in a very favourable state for the observation.

I have since repeated the experiment several times, and have never failed witnessing the same appearance.

Notwithstanding all that I have seen I cannot say that I am convinced the appearances can be attributed to nothing else but cilia; it is possible they may be due to the presence of active corpuscles, as suggested by Mr. Wenham in his paper on the leaf cells of Anacharis alsinastrum published in the *Microscopical Journal* for 1855, which corpuscles may be Vibrionia or Zooglæa, described by Dr. Cohn in his "Researches on the Development of the Microscopic Algæ and Fungi," as representing the developmental condition of a plant, but it is only by further research that this point can be definitely settled.

The result of my observations so far appears to be that in addition to the wave of light already seen, the separate objects causing that wave may now be observed in the manner I describe; what these objects are is still a matter to be determined, but at present I am inclined to believe them to be cilia on the cell wall, while at the same time there are also independent moving corpuscles within the cell; some of these bodies have the appearance of crystals, and in one specimen I observed a great number of starch granules in the cells.

In investigating this subject the smallest step in advance cannot but be deemed of importance, and I trust that in giving the results of my observations as they occur, I may be the means of saving time and trouble to others who may be investigating the same class of objects and of inducing them to follow up so interesting a subject, as it is only by many and independent observations the truth can be ascertained.

For the information of members I may state in detail the method of observation I have found most successful.

The microscope I made use of is one of Smith and Beck's largest sized binocular, with one of Beck's most recent ⅛th-inch objectives.

The illumination was by means of an Argand gas burner, the light passing into a right-angled prism below the stage in a line with the axis of the object glass.

Immediately beneath the stage was the achromatic condenser, used both with and without the central stop in the diaphragm, the object being seen equally well in both ways with different effects of light.

Below the achromatic condenser was fixed Darker's series of selenites, and below this the polarizing prism, the analysing prism being inserted immediately above the objective.

I made use of the low Huyghenian eye-pieces, and used the microscope either as a binocular or single tube, the field being well illuminated in each case.

I need not add that the most careful centering and adjustment are essential.

In preparing the object I made a section of a small portion of the leaf laid on a piece of cork in water under a simple microscope, separating only one layer of cells; I laid this on a slip made of thin covering glass, and over it applied a thin glass cover, a small feeding bottle and thread being made use of to prevent the object being dried by evaporation.

I then covered the stage and object with a piece of black velvet, to prevent interference from other lights in the room.

Mr. SIDEBOTHAM reported the plentiful occurrence, in June, of Thlaspi arvense, L., on the borders of Carrington Moss. It has very rarely been noticed in the neighbourhood of Manchester.

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### ON THE ABSORPTION OF GASES BY CHARCOAL.

The following Letter from Dr. R. ANGUS SMITH, F.R.S., to Dr. J. P. JOULE, F.R.S., was received by the Editor, June 20th, 1867.

Manchester, June 17th, 1867.

My Dear JOULE,

You asked me about my experiments on the absorption of gases by charcoal. I certainly seem to delay them, but I have little spare time. In 1848 I illustrated the oxidising power of porous bodies, referring chiefly to sand. In 1862, when speaking of the absorption of gases by charcoal, I ventured to say that the physical and chemical action could not be separated. I have been anxious to obtain more direct evidence.

I had worked a good deal with the mixed gases, but lately thought it better to return to the simple, although unwilling to question results got by others. Five of these gases have been tried, and they are found to be absorbed by charcoal in whole volumes and not in fractions of a volume, hydrogen being taken as one. In three cases hydrogen, oxygen, and carbonic acid, the numbers are 1, 7·99 and 22·05 extremely exact volumes, with a relation the same as our ordinary atomic weights. Saussure's numbers treated thus give 5·3 and 20, that for nitrogen being 4·2.

It is only by taking the average of many experiments that these results have been obtained, but, in doing so, every one has been added without selection. The numbers from which the averages are obtained diverge so much that I suppose others have not thought of obtaining anything definite. This is caused by the difficulty of finding perfectly uniform charcoal.

I have not found the other numbers to be the same as the equivalent, although still whole. Equivalents promise here to enlarge their bounds. I cannot believe that these numbers can be the results of any accident. They must be distinguished from chemical equivalents by weight.

I am,
Yours sincerely,
R. ANGUS SMITH.

J. P. JOULE, Esq., L.L.D., F.R.S., &c.

# PROCEEDINGS

OF THE

# LITERARY AND PHILOSOPHICAL SOCIETY

OF

# MANCHESTER.

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# NOTE.

THE object which the Society have in view in publishing their Proceedings, is to give an immediate and succinct account of the scientific and other business transacted at their meetings, to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

# INDEX.

Alcock Thomas, M.D.—On a living Frog-tadpole of unusual size, p. 207.

Bailey Charles, Hon. Lib.—Preservation of Dried Plants, p. 164. Notes on some of the characteristic Plants of the Channel Islands, p. 194. On the Shell-beach at Herm, p. 212.

Baxendell J., F.R.A.S., Hon. Sec.—On Solar Radiation, part I, p. 36. On a supposed Variable Spot on the Moon's Surface, p. 75. On Solar Radiation, part II., p. 97. On the supposed Influence of the Moon on the Temperature of the Atmosphere near the Surface of the Earth, p. 124.

Binney E. W., F.R.S., F.G.S., V.P.—On some Specimens and Sections of Volkmannia, p. 1. On Pholas Burrows, p. 8. On the Posidonian Schists of the Isle of Man, p. 27. On the Age of the Hæmatite Iron Deposits of Furness, p. 55. On Pholas Borings, as described in Mr. Charles P. Jopling's Sketch of Furness and Cartmel, p. 76. An Original Letter of the late Sir Walter Scott, p. 131. Description of a Dolerite at Gleaston, in Low Furness, p. 148.

Birt W. R., F.R.A.S.—Variable Spot on the Moon's Surface, p. 73.

Brockbank W.—On Hæmatite Iron Deposits, p. 59.

Brothers A., F.R.A.S.—Note on the Colour of the Moon during Eclipses, p. 52.

Broughton S.—Colour of a Solar Spot, p. 165.

Chary C. Ragoonatha.—On the Use of the Slide Rule in Calculations of Eclipses, p. 75.

Cockle Chief-Justice, M.A., F.R.S., F.C.P.S.—Memorandum on the Evolutions of Integrals, p. 67.

Crompton S., M.D.—On the Portraits of Sir Isaac Newton, p. 3.

Dancer J. B., F.R.A.S.—Jupiter as observed at Ardwick, August 21st, 1867, p. 10. Some Remarks on Crystals containing Fluid, p. 88. Microscopical Examination of the Solid Particles from the Air of Manchester, p. 157. Remarks on Molecular Activity as shown under the Microscope, p. 162.

Darbishire R. D., F.G.S.—On Pholas Burrows in the Limestone Hills, near Buxton, p. 7. Notes on some Superficial Deposits at Great Orme's Head, and the Period of its Elevation, p. 12.

Darling W. H.—Researches on Di-Methyl, p. 173.

Dyer J. C., V.P.—Brief Notes on the Laws of Physical Force, p. 108. Brief Notes on the Mutations of Imponderable Elements, p. 165. Brief Notes on Latent Heat, p. 198.

Hart Peter.—Note on the Occurrence of Sulphocyanide of Ammonium in Gas Mains, p. 9.

Herschel Sir J. F. W., Bart., M.A., D.C.L., F.R.S.—On a Statement in Mr. Dyer's Paper "On the Mutations of Imponderable Elements," p. 193.

Hurst H. A.—On Plants appearing in successive years on land prepared for Plantations in Cheshire, p. 62.

Joule J. P., LL.D., F.R.S., V.P.—On a Thermometer unaffected by Radiation, p. 35. On a Method of determining the Absolute Horizontal Intensity of the Earth's Magnetism, p. 78. Note on the Resistance of Fluids, p. 223.

Kirkman Rev. Thomas P., M.A., F.R.S.—Note on "An Essay on the Resolution of Algebraic Equations, by the late Judge Hargreave, LL.D., F.R.S.," p. 133. On the Solution of Algebraic Equations, p. 141. Note on the Correction of an Algebraic Solution, p. 221.

Latham Arthur G.—On Silk-producing Worms from Natal, p. 24.

Mackereth T., F.R.A.S., F.M.S.—A Comparison of Solar Radiation on the Grass, and at six feet from the ground, p. 50. Solar Radiation Observations, made at Eccles, near Manchester, p. 79. Results of Rain-Gauge and Anemometer Observations made at Eccles, near Manchester, during the year 1867, p. 179. On Ozone and its probable connection with Solar Radiation, p. 213.

Mitchell Captain.—On Ciliated Infusoria in Dew Drops on Leaves, p. 23. On the Structure of the Tongue of the House Fly, p. 24.

Ogilvy Thomas.—On the Aurora Borealis as a Weather Prognostic, p. 118.

Roscoe Professor H. E., F.R.S., Hon. Sec.—On the Properties of Vanadium, p. 107.

Schmidt J. F. G.—On the Lunar Crater Linné, p. 107.

Schunck E., F.R.S., P.—On some Constituents of Cotton Fibre, p. 91.

Sidebotham J.—Note on the Ship Barnacle, p. 33. Notes on some of the Rarer Plants found near Llandudno, p. 83. Further Notes on Wood-feeding Coleoptera, p. 207.

**Smith J.**, M.A.—On the Origin of Colour, and the Theory of Light, p. 137.

**Smith R. Angus**, F.R.S., V.P.—On Exhibitions on the Continent, p. 28. On the Examination of Water for Organic Matter, part I., p. 72. Part II., p. 78. A Search for Solid Bodies in the Atmosphere, p. 154.

**Vernon G. V.**, F.R.A.S., F.M.S.—Solar Radiation Observations, made at Old Trafford, Manchester, p. 46. On the Rainfall at Old Trafford, Manchester, during the year 1867, p. 128. Notes of Comparisons of Ozone Papers, p. 219. Increase of Ozone with Increase of Altitude, p. 221.

**Vize Rev. J. E.**—On the Mite of the Wood Pigeon, p.211.

**Wilkinson T. T.**, F.R.A.S.—On some Points in the Restoration of Euclid's Porisms, p. 68.

*Meetings of the Physical and Mathematical Section.*—Annual, p. 179. Ordinary, pp. 52, 79, 97, 118, 213.

*Meetings of the Microscopical and Natural History Section.*—Annual, p. 211. Ordinary, pp. 23, 62, 116, 161, 207, 210.

*Report of the Council.*—April 28th, 1868, p. 183.

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## ERRATA.

Page 194, line 23, for "L'Ancrene," read "L'Ancresse."
" " " 28, for "ophioglonifolius," read "ophioglossifolius."
" 195 " 6, for "Branica adprena," read "Brassica adpressa."
" 196 " 13, for "enincarnatum," read "eu-incarnatum."
" " " 15, for "Tm.," read "Ser."
" " " 21, for "thyrssidens," read "thyrsoideus."
" " " 28, for "Exholtzia," read "Escholtzia."
" " " 33, for "L'Ancrene," read "L'Ancresse."
" 197 " 9, for "parrifolius," read "porrifolius."
" " " 16, for "Imila," read "Inula."
" " " 17, dele "the."
" " " 32, for "Bonley," read "Bouley."
" 198 " 3, for "vernation," read "venation."
" " " 14 & 31, for "L'Ancrene," read "L'Ancresse."

# PROCEEDINGS

OF

# THE LITERARY AND PHILOSOPHICAL SOCIETY.

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## Ordinary Meeting, October 1st, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S, &c., President, in the Chair.

Mr. E. W. BINNEY, F.R.S., &c., said that in the *Dundee Advertiser* of the 12th ultimo a report is given of some remarks on *Calamiteæ* and fossil *Equisitaceæ*, by Mr. William Carruthers, of the British Museum, who stated "that the most important characters were obtained by botanists from the fructifications, and the author had obtained, through the kindness of Dr. Hooker, sections of vegetable structures prepared by Mr. Binney, whose extensive acquaintance with coal plants was well known. He had discovered in some of the fruits which belonged to *Calamites* so beautifully preserved that the most minute details could be determined with the help of his diagrams in the various points in which they agreed from the fruits of the recent *Equisitaceæ*."

Now some twelve years since the specimens said to be "prepared" by him (Mr. Binney) were discovered and immediately slided, polished, and sent up to London to a most eminent scientific man, who had agreed to join him (Mr. Binney) in publishing a description of them, and whose name it was not then necessary to give. With the specimens sent up was written, 29th November, 1854, as follows: "You will be delighted to find such beautiful sections of *Volkmannia* in the large slide I now send. They are well worth the trouble and expense of the slide. These doubtless belong to *Calamites*." When he saw the same gentleman in London soon after he pointed out to him under the microscope the small spore-like bodies in the Sporangia.

At that time the whole of the structure of *Calamodendron* had been made out with the exception of the centre of the stem and the connection of *Volkmannia* with it. The valuable papers of Dr. Ludwig on *Calamite* fruits, and Dr. Geoppert on *Aphyllostachys* afforded him much information, but he obtained the evidence of *Volkmannia* being the fruit of *Calamodendron* from the similarity in structure of the central axes. About two years since he cleared up both the above points to his satisfaction, and then immediately wrote to the gentleman to whom he had sent his specimens and urged upon him the necessity of their immediately publishing. The latter replied that his engagements would not allow him to join, he having a short time previously returned the specimens which had been so long in his possession. He (Mr. Binney) then, unaided, lost no time in commencing to write a description of his specimens, which is now in the press and intended to be shortly published by the Palæontographical Society. Some of the plates were engraved a year ago, and nearly all of them were in the engraver's hands and would have been done had the author's health allowed. A few months since he thought the paper would be published in October, but owing to delays over which he had no control such has not been the case. All he asks is for the public to give him credit or discredit, as may be, for any discoveries he may make, as well as to the parties who have, without his consent, obtained possession of his specimens, even although the latter may anticipate him in publishing a few weeks. No doubt the observations are independent of each other—may differ in some respects

—and his may be less valuable than those of Mr. Carruthers; still it is not unreasonable that he should wish to be considered as an original investigator rather than a preparer of specimens for other people.

Dr. **Crompton**, alluding to the paper he read in October, 1866, "On the Portraits of Sir Isaac Newton," said, that while preparing his essay for publication in the Memoirs of the Society, he had opened up fresh sources of information, and become possessed of facts of considerable interest respecting the portraits of Newton. He had examined about twenty portraits of the great philosopher, all of which were considered to be originals, and most of which were undoubtedly painted a long time ago; but he had seen no portrait which, in his opinion, was of equal importance and interest with the Kneller Newton of 1689, to which he last year directed the attention of the Society, as by far the most valuable portrait of the great philosopher in existence, and of which he exhibited an admirable engraving by Mr. Oldham Barlow. His extended inquiries into the subject of the portraits of Newton had led him to the conclusion that there are several (if not many) which pass current as portraits of him which are most decidedly representations of other persons. In the National Portrait Exhibition at Kensington of the present year there were four portraits of Newton, two of which he feels sure have no real claim to be regarded as authentic or genuine. These two are pictures contributed by the Earl of Dartrey and by the Marquis of Exeter. The former purports to be a portrait of Newton when he was a Bachelor of Arts, and to be painted by Lely. It represents a young man with his hand resting on a globe; and there is an engraving of the picture, done many years back, but he had no hesitation in saying that the picture had no right to be considered a portrait of Newton. The features were not Newton's, and it was most improbable that the poor Trinity College Sizar would, when a Bachelor of Arts (that is, between January, 1665, and July 7, 1668) have cared, or, if he had cared, could have had the means to obtain a portrait of himself. He apprehended that because the portrait represented a young man with his hand on a globe, some imaginative person had supposed that it must be a representation of the great philosopher who had explained the system of the universe. But this portrait represents one of the poorest specimens of humanity; and certainly not a Newton, but rather a Simple Simon. The Marquis of Exeter's picture has hardly any greater claim to be regarded as a Newton. It represents a man with a bald head, but we have the clearest evidence that Newton was not bald.* The two Vanderbank portraits representing him the year before his death without his wig, show that he had a beautiful head of silver white hair. There is another portrait of Newton by Thornhill, taken at an earlier period, which represents him without his wig, but with short white hair and no trace of baldness. The Portsmouth Kneller of 1689, which represents him when he was 47 years old, shows him without a wig and with abundant grey hair. This is a sufficient ground for rejecting this picture; but no one who has studied the portraits of Newton could be brought to believe that this is a representation of the great philosopher. Last year when reading his paper, Dr. Crompton remarked that he had been unable to discover where the original Kneller of Newton, engraved by Houbraken, was to be found. He then pointed out that in some impressions of the Houbraken print it was stated to be in the possession of Mr. Conduit. Dr. Crompton went to the Earl of Portsmouth's seat at

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* Mr. Conduit, Newton's nephew by marriage, when describing Newton's personal appearance, says, "With a fine head of hair as white as silver, without any baldness, and when his peruke was off, was a venerable sight."—Brewster's *Life of Newton*, 1855, vol. ii., p. 413.

Hurstbourne Park, and there found the original picture. It is dated 1702 and signed by Kneller. The Earl of Portsmouth, Newton's collateral descendent, therefore possesses the two most important portraits of Newton. This portrait of Newton was engraved also by Smith, about 1712. A replica, if not an early copy, without any name of an artist upon it, is in possession of the Duke of Devonshire at Holkar. His Grace informs me that he cannot positively trace its coming into the possession of his family, but he conjectures that it may have been at Holker before the marriage of one of the Lowthers with Lady Mary Cavendish, through whom this Holker property passed to the Cavendishes. There is an exact replica or early copy of the Portsmouth Kneller of 1689 at Lord Galway's, except in not having the name of Newton or of Kneller upon it, which Dr. Crompton has examined, and which, in an old catalogue of the pictures, is said to be painted by Bond. Dr. Crompton said that he had examined the three oil paintings of Newton in the Royal Society's collection; also the Kneller at Hampton Court, which is dated 1689, and differs from the Portsmouth Kneller of 1689 only in the position of the hands; also all the portraits of Newton belonging to Trinity College, Cambridge; the Indian ink drawing of Newton in the Pepysian Library at Magdalen College; and a portrait at Mrs. Miles', at Firbeck Hall, Yorkshire, which resembles very much the Egremont Newton by Kneller, engraved as a frontispiece to Sir David Brewster's first Life of Newton, published in the Family Library. Dr. Crompton exhibited this edition of Brewster's life, as well as the larger one in two vols. In the text of both the engravings, which differ very greatly from each other, are said to be from a picture in the possession of Lord Egremont. Sir David Brewster is unable to explain how it is that two engravings from distinct and different portraits should thus happen to be spoken of in the text, except that his publisher must have selected the second portrait and got it engraved. It is, I think, unmistakeably taken from Smith's print of 1712 or an early impression of Houbraken's engraving. The portrait prefixed to the small Life of Newton is the same as that engraved in Lodge's portraits. At present Dr. Crompton is unacquainted with the historical evidence regarding the genuineness of this portrait. It is signed by Kneller and dated 1716; but Dr. Crompton has not yet seen it, nor one in the possession of Mr. Turner, of Stoke Ashford, near Grantham.

In Lord Portsmouth's collection at Hurstbourne, besides three genuine and authentic portraits of Newton, there is a fourth picture with the name of Newton painted upon it. It was described to Dr. Crompton by a gentleman who had had an opportunity of a very close examination of it, as the earliest and most important portrait of the great philosopher. But Dr. Crompton found neither date nor artist's name upon it, and the picture, which is mounted upon panel, has a crest in red wax behind it, which is not the crest of the Portsmouth family, but could not be sufficiently determined what it was for want of a magnifying glass. The history of this picture will be investigated further; though it is certain that it cannot be a portrait of Newton, for the features are not his, and the eyes are brown, while Newton's were bluish grey. It is most earnestly to be desired that all the portraits of Newton might be collected together at Kensington next year for comparison with each other, and that the portraits of other great men (where there are several) should be thus exhibited in juxtaposition. It would probably then be evident that there exist many spurious ones, and an opportunity would be thus afforded of determining which are the best as well as the true.

Mr. R. D. **Darbishire**, F.G.S., referred to a paper "On the Existence of a Seabeach on the Limestone Moors near Buxton" (*Trans. Manchester Geol. Soc.*, V., p. 273), in which Mr. John Plant, F.G.S., had described as sea beach the surface of the limestone rock as the same is seen when bared of sward and surface clay above the quarries on Grin Edge and Harper Hill, south-west of Buxton, and to Mr. Plant's conjecture that this worn surface probably extended nearly to the crown of the hills.

His own observation had marked on each hill, above the stratum whose upper surface exhibited those indications of wear, a stratum of somewhat different texture still subsisting in the shape of a slight vertical cliff or reef. This bed had not worn in the same manner as the "beach," that is to say, with many interlacing fissures leaving a close *chevaux de frize* of limestone points, but rather in great blocks with round, curved edges or holes.

In connection with this bed Mr. Darbishire had obtained specimens from each hill exhibiting what he believed to be the remains of the burrows of Pholas shells.

On the top level of Grin Edge, close to the ruins of the tower, one stone had a group of seven holes. They were placed like Pholas holes as he had collected them on Great Orme's Head; and, though the surface of the stone about them was much worn, taken along with the specimen next described, it seemed more fitting to ascribe to them a similar origin than to attribute them to the natural wear of the stone, notwithstanding the variety and singularity of many of the forms in which atmospheric or aqueous corrosion affect the limestone rock.

This stone lay amongst a heap of others near the ruins of the tower, and had doubtless been brought up a few feet. The height above the sea of the tower is stated on the Ordnance Map as 1,435 feet.

On Harper Hill, in two large blocks of the overlying stratum, he had detected more characteristic holes. Both blocks were lying in the sward above the "beach" surface, and were a few feet below the rock *in situ*, from which they had evidently been detached. Of the first of these specimens he exhibited a photograph. It showed in the under side of the edge of a projecting ledge or table of stone six well marked holes from ¾in. to 1in. in diameter, and one an inch deep, and traces of two others. The holes were grouped just as Pholas holes usually are, and apparently were quite independent of the structural fissures of the stone.

According to measurement with an aneroid barometer, the stone in which those holes occurred was about 1,380, and the reef from which it had fallen about 1,400 feet in elevation.

If the holes were really Pholas burrows, they would indicate the elevation of these hills since the period of glacial action by sea or land.

Mr. **Binney** F.R.S., F.G.S., remarked on the great elevation of these remains if the observations were accurate, and observed that they were on the west side of the hills. Mr. Prestwich had discovered shells in shingle on the western slopes of the Axe-edge hills towards Macclesfield, at the height of 1,150 feet. He would like to hear of observations on the eastern side of the Derbyshire hills if any traces of marine action were to be discovered there.

Mr. **Darbishire** had not the specimens at hand, but would produce them, in connection with a series of similar remains from Orme's Head, to which he proposed to call the attention of the Society at an early meeting.

## Ordinary Meeting, October 15th, 1867.

J. P. JOULE, LL.D., F.R.S., &c., Vice-President, in the Chair.

Among the donations announced was a photograph of the memorial recently erected in the Salford Cemetery to the memory of the late Mr. John Parry, presented by Mr. Cottam.

The thanks of the Society were voted to Mr. Cottam for his donation.

"Note on the Occurrence of Sulphocyanide of Ammonium in Gas Mains," by PETER HART, Esq.

A few months ago while some gas mains in the street were being replaced, I was induced to examine the scale which forms in the interior, being under the impression that probably I should find sulphide of iron the result of long-continued action of sulphide of hydrogen on the iron. On placing a portion of this scale in pure hydrochloric acid I perceived an intense reddening, much more than would be accounted for by the simple solution of peroxide of iron, and being aware of the fact of sulphocyanogen being one of the products of the distillation of coal, I at once suspected its presence. A portion of these scales were boiled in water. The clear filtrate from this gave off much ammonia on the addition of alkali, and on the addition of a dilute solution of perchloride of iron it gave at once the intense coloration so characteristic of the sulphocyanides. The insoluble portion remaining on the filter was then boiled in dilute caustic soda; the filtrate from this made acid, and a solution of ferric oxide again added, this time with the production of a blue precipitate indicative of a ferrocyanide. This must have existed as ferrocyanide of iron, which on boiling with the alkali became oxide of iron and ferrocyanide of sodium. There appears to me something curious in the fact of these bodies being carried such a distance (in this case fully a mile from the gas works) by the gaseous current. I should think the ferrocyanogen is the result of a reaction between the sulphocyanogen and the metallic or oxide of iron. The amount of these bodies must, I think, be far too small to have any bad effect on the health of gas consumers.

"Jupiter as observed at Ardwick on the night of August 21st, 1867," by J. B. DANCER, F.R.A.S.

The somewhat rare phenomenon of Jupiter without a visible satellite extraneous to his disc (as the Rev. W. R. Dawes has correctly designated it) has been described in the *Astronomical Register*. I am not aware, however, that any notice of it has appeared before this Society, and as few observers appear to have enjoyed such favourable atmospherical conditions as we did in this locality, I am induced to offer a few remarks on the appearance which Jupiter presented on the night of August the 21st.

During the early part of the evening the S. and S.E. portions of the heavens were covered with thick haze; between 8 and 9 o'clock this gradually disappeared, and the atmosphere became clear and unusually favourable for astronomical observations. Preparations had been made for noting the times of the phenomena, but from various circumstances this was abandoned. The observations were made with an achromatic telescope of 7½ feet focus and 6¼ inches clear aperture. Powers employed, 90, 175, and 300. At 9 o'clock Jupiter was clearly defined and presented a remarkable appearance. On the broad south belt three spots were distinctly visible, and at the same time three satellites appeared off the disc. The first spot in the order of transit was very black—this was the shadow of the third satellite. The next spot was brown in colour when contrasted with the belt—this was the third satellite; and the third spot, which was the shadow of the fourth satellite, was very dark and well defined. At this time the fourth satellite appeared very faint in comparison with the first satellite, which was just below it. The second satellite, which was on the west edge of the planet, was soon eclipsed, and attention was entirely directed to the fourth satellite, which was the next to transit. As this satellite entered on the disc it was visibly brighter than the planet; after a short time it disappeared, and then, to my surprise, it became visible again as a dark spot, and at intervals it was as black as its own shadow. The alteration in the colour of this satellite during its transit has been noticed by several observers, but I was not prepared for such a complete change from a bright object to one as black almost at times as an ink spot. During these observations the first satellite had entered on the disc of the planet, and when it was about half its diameter on the planet it appeared to me to be in contact with its own shadow. After a short time I could only distinguish the first satellite at intervals, closely following its own shadow. Before the third satellite passed off the disc it became nearly invisible, and when close to the edge of the disc it appeared brighter than the surface of the planet. The fourth satellite was not observed at the end of its transit.

Mr. Robert Worthington, F.R.A.S., observed some of the phenomena with me, and remarked at the time that he did not recollect having seen Jupiter so sharply defined as on that evening. The bays and various markings on the belts were most beautifully distinct.

"Notes on some Superficial Deposits at Great Orme's Head, and the Period of its Elevation," by R. D. DARBISHIRE, B.A., F.G.S.

After referring to Professor Ramsay's exposition of successive glacial periods in North Wales and its elevation; (*Quarterly Journal Geol. Soc.* viii., and *Old Glaciers of Switzerland and N. Wales,* 1860) to a paper by Mr. Binney, F.R.S., on "the Drift Deposits found about Llandudno" (*Geol. Soc. Trans. Manchester,* April, 1861), and a paper by Mr. Bonney "On Traces of Glacial Action near Llandudno" *Geol. Magazine* iv., July, 1867), the author shortly described the configuration of the Head and the Llandudno Isthmus. He supposed the superficial outline of the Head to be due to marine denudation, possibly aided at some early period by the friction of floating icebergs, but in the last instance and ever since its first emergence to long continued and extremely tranquil elevation and concurrent sæcular subaerial degradation and deposit. The isthmus, he described as ancient sea bottom, gradually elevated and denuded in the process by current or tide wash, and then augmented by beach accumulation and blown sand.

Detailed notes as to the superficial formations were then read as follows:

### I.—MARINE FORMATIONS.

(1) Yellow cherty clay in Gwydfyd Valley, as described by Mr. Maw, F.G.S., *Geol. Mag.* ii., 1861.

(2) Glacial and boulder clays of a later age:

(*a*) Tenacious dark blue clay, full of small pebbles of dark slaty rock, at the foot of the western cliff of the isthmus and on the beach, as described by Mr. Bonney, *Geol. Mag.* iv., p. 292.

(*b*) Close greyish olive coloured rather sandy clay, with rounded stones, in similar situations (possibly a variation of (*a*.)

(*c*) Chocolate brown clay, with boulders, some scratched and occasionally slightly bedded, forming the general base of both eastern and western cliffs of the isthmus. Formerly well exposed where the new sea wall and boatstairs are near Tygwyn, Llandudno. At this spot fragments occurred of Tellina solidula, Mactra solida, Astarte arctica, Cardium edule.

The same bed appears at Colwyn ballast cutting under sandy clay and old beach shingle, with fragments of the same species and of Saxicava.

(*d*) A very similar bed of lighter coloured reddish clay with rounded and angular stones, in the west cliff, and generally over the isthmus.

In the cliffs occurred fragments of Mya truncatia, Tellina solidula, Mactra ——, Cardium edule, Mytilus edulis, and Buccinum undatum.

The same bed and like fragments of shells are seen at 70—80 feet above the sea near the bath house, and according to Mr. Maw, as high as 170 feet up the Gwydfyd Valley.

Mr. Binney had found Turritella terebra, and had pointed out the similarity of those beds with those at Blackpool. Mr. Maw had identified the boulder clay also in a terrace about 170 feet high on the south.

The occurrence of travelled boulders indicated the former existence of the bed at higher levels; thus a mass of greenstone had occurred above Gwydfyd farm at 380 feet elevation.

These boulder clays, the author supposes to have been deposited at the close of the glacial epoch, and to be the result of redistribution of older beds on the coast of a rising land, mixed with old drift and land drift, swept down by the glaciers of Snowdonia, in whose recesses the astarte and other shells occur on ancient beaches.

### I. B.—SEA BEACH AND BEACH MARKS.

(3) Mr. Binney had described a bed of shingle at 400 feet in the valley of the old road.

(4) Mr. Bonney had described as an indication that the surface of the higher ground has in many cases been affected by ice, large blocks of limestone scattered about, especially on the north-west of the hill, differing from the rock on which they lie, but of a stone found *in situ* on a slight eminence in the neighbourhood,

The author believed these rocks to have no glacial origin, but to be raised beach—the remains of the rage or play of waves about the new-born island.

He had not found any boulder of other stone than that of the hill, and could not indicate any other origin for these elsewhere. Puffin Island and Anglesey to the west are much lower, and Penmaen Rhos to the east lies out of any hitherto described extension of the North Wales glacier system.

(5) The plateaus furnished other indications of sea-wear, as on many of them the limestone stratum is uniformly denuded of all superincumbent stone, and cut and worn in fissures and other forms very similar to those of like beds between tides below. Whether this erosion is that of ancient waves, or of rain water, since elevation the even stripping of the overlying stratum indicates aqueous denudation.

A similar or even more marine looking scar forms the top of Little Orme's Head.

The vertical outlines consisting, when not hidden by talus, of alternate cliff and scar, and the grand curves of successive contour lines of the Head are thoroughly marine.

Mr. Bonney had noticed the hollows of wave wear in the face of these cliffs.

These longitudinal caves record a long continuance of tide wash on a stationary cliff. The undisturbed horizontality of many of these wave marks indicates great uniformity in the process of elevation.

(6) Distinct traces of raised beach occurred on the east side of the Head near the bath house, where masses of shingle hang cemented to the face of the rock 50—60 feet above the sea.

(7) On the boulder clay of the isthmus, especially developed in the western cliff, is a deposit of varying thickness of beach shingle, courser or finer; seen also in foundations in Llandudno, and probably continuous with the present eastern beach.

(8) On the last named beds lies much blown sand. Mr. Glover mentions that within forty or fifty years this sand has converted into solid ground a tract of impassable swamp, and even overlaid it.

(9) Unmistakeable proofs of beach wear occurred in the shape of the burrows of Pholades, necessarily made while the rock was near if not below low water mark.

These remains occur at various heights all over the Head, under certain conditions, of sufficient consistency of stone and sufficient shelter from atmospheric wear, either under the edges of tables of rock, *in situ* or fallen, or in rounded beach stones, on or in the sward on the talus slopes of the hill. All the stone in which holes had been observed, had evidently suffered some superficial waste, hence only the ends of deeper burrows remain.

The following list is arranged along a rude profile of the Head, from N.E. to S.W.

(*1*) Two slabs near the stile on the path from the archery ground to Gwydfyd farm. 216 feet.

(*2*) In the cutting over the yellow clay at Gwydfyd farm in a large loose boulder, three large and very perfect burrows and part of a fourth, possibly of pholas crispata, a species of well ascertained quaternary occurrence. 350—60 feet.*

The same spot furnished a second boulder with ten holes.*

(*3*) The rugged hillside above, and to the west of the clay pit, yielded in the edges of beds *in situ*, well preserved groups of holes at 370 feet and 435 feet, above the sea.

(*4*) The first rock that breaks the sward on the south side of the path, up to the old telegraph station, after leaving the mine plateau, showed good holes, not to be confounded with the effect of atmospheric corrosion. 440—450 feet.*

(*5*) In a reef which breaks the sward in the plateau S.W. of the telegraph hill, with worn beach stones, occurred a fragment with two holes. 570 feet.*

(*6*) On the exposed reefs towards the south, the holes are not unfrequent. On the 2nd or 3rd reef, marked by a loose spherical boulder at its eastern end, a fallen slab of very hard stone, showed 27 holes. 570 feet.*

(*7*) The next reef yielded good specimens at 540 feet,* and at 520 feet.*

(*8*) The burrows occur in considerable abundance and good preservation, at Little Orme's Head, especially on the topmost plateau.

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* In these cases specimens were produced.

In the preservation of these holes in loose beach stones, and in the edges of out-cropping layers of stone, in the level sea stripped scars, and in the limestone boulders of the higher plains, the author submitted irrefragable proof that the surface of these anciently submarine hills had not been touched by iceberg or glacier for some time before or at any time after they emerged from the waves.

He had not been successful in characterising any rock surface on the Head pointing as montonné, nor in finding the rock which appeared to Mr. Binney, to have "been scored and polished probably by ice," below the bath house.

### I.—C. OLD LAND.

(*9*) Between tide marks at the western foot of the Head some years ago, after the removal of shingle, a light blue clay without stones was seen, and traces of a vegetable deposit of leaves. At Abergele and Colwyn to the east, and under Penmaenbach to the west this vegetable bed occurs with roots and stems of forest trees. From a cutting near Penmaenbach, where a part of the bed appears as peat of very modern aspect, had been obtained bones and a tooth of an ox, and a large horn of red deer. A similar horn was found near Abergele when the railway works were in progress.

### II.—SUBAERIAL DEPOSITS.

(*10*) On each side of the Head there appears on the rock unstratified reddish calcareous clay with more or less frequent, larger or smaller, angular fragments of the overhanging rocks, not seldom so numerous as to form a breccia. Here and there, at considerable elevations both north and south of the hill, this bed yields rounded and larger or smaller pebbles of greenstone, and other non-calcareous rocks; and not unfrequently pieces of limestone with the softly curved surfaces of sea wear.

(*11*) On most of the less elevated or less steep inclines, the last mentioned deposit is overlaid by, and passes upwards into a very similar layer, distinguishable by a darker colour, and more earthy consistency, and containing like angular sea and worn fragments. In this bed, the flatter fragments of stone are noticeably disposed in planes parallel to the slope of the sward. It is this last-named bed which Mr. Binney traced as blackish brown clay, with angular fragments up to 262 feet above the sea.

The position of these two beds of clay at the foot of sea worn cliffs, the almost total absence of rolled stones, the entire want of tidal or current assortment of material, and on the other hand the predominance of sharp angular stones, their promiscuous aggregation, and the slight bedding along the surface line mark them as not marine and as subaerial. The upper formation is much modified by the gradient of slope and the luxuriance of vegetation.

The clayey matrix probably arises from decomposition of the rock fragments; but the occasional occurrence of rounded pebbles of greenstone, &c., may indicate a redistribution of patches at least of boulder clay.

It is in these two beds, and especially in the upper one, that observers have found shells of mussels, oysters, limpets, and periwinkles. Mr. Dancer apparently was the first to mention these remains in Manchester, and Mr. Sidebotham (from whom Mr. D. received his first specimens), shortly after exhibited large collections. In 1861, Mr. Binney mentions having tracked these shells up the hill towards a bed of shingle at a height of 400 feet, and deduced proof of the elevation of this Head at least that height during a very modern epoch.

In 1867, Mr. Bonney (*Geol. Mag.* iv. 289) noted a deposit of shells near Gwydfyd farm, in clay which appeared to him "not to have undergone much denudation during the process of upheaval." Mr. Maw had stated, *Geol. Mag.* iv. 377, his conviction that Mr. Bonney's shells had been deposited by man. Mr. Darbishire pronounced the shells of the red clay with angular fragments also to be of non-marine deposit. These latter shells are of the species Mytilus edulis, Cardium edule, Ostrea edulis, Patella vulgata, Littorina littorea, Purpura Capillus, and Buccinum undatum. These occur either massed in heaps or layers (generally of one or two kinds together) or more sparsely distributed. They are all of species eaten by birds or man. All the specimens are full grown, or at least of edible size, and in nearly every instance whole, the exceptions being Mytilus, a shell peculiarly liable to disintegration, and the univalves, which sometimes occur broken about the mouth, as if to allow of extraction of the animal. There are neither any other species nor young specimens of what do occur. The shells do not appear as if deposited under any natural original conditions, nor, if they were so, does it seem likely that they would have remained during a slow elevation through the tides of ages. The fact of elevation is undoubted, but these shells are no evidence of it.

It is not necessary to give separate lists of the lots found at successive heights. They occur on all sides of the Head, but have been especially collected in the old road valley, and on the Hendafarn road towards the south-east. Along this district they have been noted at 120, 140, 180, 200, 300, and 315 feet above the sea. On the old road section, Mr. Darbishire had found the shells not unfrequently accompanied by teeth, and fragments of bone of domestic animals, and bits of charcoal. Thus there occurred:

At 120 feet, part of the jaw of a pig.

At 180 feet, sheep's teeth and frequent fragments of bone.

At 300 feet, the molar of a horse and fragments of bone.

At 315 feet, on the north side of the road were found with patella and littorina, a cow's tooth, part of the jaw, and other bones, and teeth of a pig, fragments of other bones, bits of charcoal, and two human molars.

At 365 feet, fragments of bone.

The bones have all lost much animal matter.

On the whole these shells and bones must be attributed to deposits made by birds or men. The shells are such as sea birds often carry to land for food, and (except the oysters), most of them may now be found in suitable places on the Head so transported.

In Connemara shells of the very same kinds may be frequently found and traced to or from a cabin or the site of one, on or under the sward according to the overlying of the deposit.

Shells lying on exposed ledges, or shells or bones cast out, would in the course of years be distributed down hill along with and in time be buried in and under the superficial detritus. * Wind and rain and chance footsteps would assist in scattering such objects.

It is probable that the deposit of these remains on the Head is of very ancient date.

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* At the Tunara sand flats, north of Gibraltar, I have watched a levanter roll along heavy shells of Cardium tuberculatum, and Pectunculus, and even a massive valve of Panopaea glycimeris.—R. D. D.

## III.—REFUSE HEAPS AND OTHER UNDISTURBED RELICS OF HUMAN OCCUPATIONS.

*(12)* Mr. Bonney's bed of shells at Gwydfyd pit has been mentioned. He had collected Mytilus Ostrea, Patella, and Littorina with three valves of a Tapes doubtfully identified as pullastra.

The deposit was entirely within the uppermost layer of blackish earthy clay. Mr. Darbishire's collections from this spot comprised Tapes decussatus (but no pullastra), (in pairs and valves, whole and broken, all full grown and some large), Cardium edule, Mytilus edulis, Ostrea edulis, Patella vulgata, Littornia littorea, Littorina littoralis, Purpura lapillus, and Buccinum undatum; all species still eaten. No bones or implements occurred. The deposit is now removed by the enlargement of the clay pit. It is undoubtedly human, and probably (from its depth), older than the next deposit.

*(13)* A very extensive midden or collection of such deposits has been exposed in the slope of talus below the western inland cliff, where it is itself cut by the waves into a low shore cliff, north of the site of what is now the house of the Dean of Christ Church. It has long been open on the cliff section, and for some years also on the slope in a cutting there, to an extent of over 30 yards up the incline, and 150 at least along it.

Mr. Bonney has published full observations on the cliff sections. *Geol. Mag.* iv. 343.

The remains consist of layers or heaps of Mytilus edulis, Patella vulgata, Littorina littorea, with some oyster shells and Purpura lapillus, and at least one heap of Buccinum undatum. At the lower edge the heaps lie massed. Higher up the process of distribution has begun. The whole is grass covered.

Bones, teeth of ox, sheep, and roebuck, and of some birds occur. Burned bones not unfrequently, and once a small bit of red earthenware occurred, but as yet no implements.

Nothing appears in the situation or condition of these remains which indicates great antiquity.

(*14*) Lastly, Mr. Bonney mentions as occurring at intervals in the sand cliffs on the Conway Bay shore seams of Mytilus edulis, which he assumes to indicate "a period of depression, during which the mussel beds are formed," and of subsequent upheaval.

These must be set down as appertaining to the most recent human period.

The practice of dredging great quantities of mussels in the estuary, of boiling them on the shore, in order to search the animal parts for pearls, is of old standing, and has been frequently described. See *Loudon's Magazine of Nat. Hist.*, 1830, as to the mussel beds and heaps on the western shore. Mr. Thomas Glover has often seen them in operation on the eastern side of the bay, *i. e.*, making these beds.

Mr. D. concluded with a notice of Mr. Binney's bed of shingle at 400 feet elevation, which called for special consideration, and postponed these remarks for discussion on a future occasion.

Mr. Sidebotham's accidental absence was much regretted, as that gentleman had paid much attention to the deposits on the Head. A full series of specimens of pholas holes from Great Orme's Head and Little Orme's Head was exhibited and examined, as were also specimens from Grin Edge and

Harper Hill, Buxton, referred to by Mr. Darbishire at the last meeting. The character of the holes as true pholas burrows, was in each case fully recognised.

Mr. **Bottomley** remarked that the specimens of limestones appeared to be decidedly argillaceous, which would tend to make them less liable to atmospheric decay.

## MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 7th, 1867.

J. B. **Dancer**, F.R.A.S., President of the Section, in the Chair.

The **President** in his address to the members of the section, described the various additions and improvements which had taken place in microscopes and apparatus, and gave a summary of the Microscopical researches which had been communicated to various societies, both English and Foreign, during the past year.

The following extract of a letter dated 27th of August, 1867, from Captain Mitchell of Madras, was read:—

One or two things have come under my notice, which a friend who arrived in Madras from London a short time since, said, he believed were unknown to microscopists in England, I therefore send you a brief notice of them, in the hope they may interest some of the members of the society.

The first is the presence of ciliated infusoria in dewdrops on leaves. I have to confess I took this subject up in jest, in consequence of a remark in an article published in one of our local papers; about sunrise I placed an animalcule cage under the point of a leaf and transferred the drop of dew gathered there to the glass plate, and then examined it with the microscope. I repeated the experiment on two other occasions, and the result was, that in about one drop out of every two I found one, and sometimes two infusoria.

As the dew did not begin to fall until midnight, they must have been produced from the germ between that time and 6 a.m.

On the first occasion I saw also what I took to be one or two spores of fungi. I supplied the cage with distilled water and put it by until the next morning, when I found a perfect forest which continued to multiply so long as I supplied it with distilled water, which I did for several days. It will be interesting to ascertain if infusoria are found under similiar conditions in England.

Some time ago Mr. Ross sent me a binocular body for my stand (one of his father's make). I have in my cabinet the tongues (so called) of two house flies, which I had mounted some years back. I was always under the impression that the divided absorbent tubes were enclosed between the two membranes that form the upper and lower surface of the two lobes, and I believe that is the general opinion of their structure; now on placing one of these specimens under a half-inch glass with the binocular body, I was not a little astonished to see the tubes standing out above the surface of the membrane on the lower or under part of the tongue.

I was always at a loss to understand the use of these very curious vessels, but it now seems evident that fluids may be taken up by them and conveyed to the larger central tube into which they all run. It is I presume known that these tubes all open on the upper surface of the lobes by the other and narrower extremity.

Mr. **Latham** read the following communication on Silk-producing worms from Natal :—

In the *Natal Herald* of the 8th August last, there are copies of a correspondence between the Chamber of Commerce of this place, and some gentlemen at Natal, regarding certain silk-producing worms, as they are termed, found near Graham's Town by Mr. Hillier, feeding on the leaves of the mimosa thorn or acacia.

The result of the correspondence was, that some of the cocoons were presented to the Chamber here to have them reported on and their true value ascertained.

They came into my hands, and the following remarks on them may be interesting to the Society:—

From one of the cocoons the moth now exhibited emerged shortly after its arrival in England, and though much crippled, I have, through the kindness of Mr. Jansen, had it clearly identified with the insect "Pachypasa effusa," of which there are several specimens in the British Museum collection from Natal.

The moth laid about 50 eggs, of which I have mounted two or three and they are here for examination.

The eggs under the microscope exactly resemble in texture those of the ostrich, but each has a small black point, probably of a softer substance than the rest of the egg, and through which the caterpillar may emerge.

From one of the cocoons I extracted the chrysalis also exhibited, and further the cast skin of the caterpillar rolled into a small ball as usual; by boiling this for some time in caustic potass, it became so softened that it was possible to get it to its original size, and to show its original form. You have it before you dried, and the series is therefore complete, egg, caterpillar, chrysalis, moth, and cocoon.

The original cocoon is, you will notice, a hard woody-like substance, but by certain processes, Mr. Hillier states soaking in a solution of soda, the cement agglutinating the silk is dissolved and a soft silky-looking bag remains.

This consists of a thick outer covering, a loose middle lining, and a thinner internal lining, all of silk, which I hardly think could be wound but might possibly be carded.

As regards the commercial value of the article, an eminent silk broker writes as follows :—

"It is a carded cocoon, the waste silk is the outer covering of the cocoon and appears of tolerable fine fibre, but is bad in color and not of a good merchantable appearance, if in quantity worth perhaps 1s. 6d. per lb. all round."

"Enquiries of a similar nature have been made from time to time by customers in the East, where silk does not form one of the staples of the country, and among other places, from Natal, but we do not find that any good can arise from efforts made to produce silk in any quantity; it is probably the most difficult of all known staples to establish in a new land."

## Ordinary Meeting, November 12th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. John Barrow was elected an Ordinary Member of the Society.

Mr. BINNEY, F.R.S., F.G.S., said that in the Rev. J. G. Cumming's excellent History of the Isle of Man that author, at page 132, says, "the different layers of the Posidonian Schist vary both in their lithological texture and organic contents. The finest and most compact layer, which is worked for ornamental purposes, is characterised by an abundance of Posidonia and the relicts of tree ferns, which we must necessarily regard with interest as indicating an approach, though still at a considerable distance from the coal formation of Great Britain." As the discovery of fossil tree ferns in the mountain limestone would be of great interest, he had lately been over to Poolvash Bay, the locality named by Mr. Cumming, and spent a considerable time in searching the black limestones there for those fossils; but although he met with plants, the remains of vegetables common to the carboniferous formation, such as *Stigmaria ficoides*, *Calamites*, and other coal plants, he found nothing resembling tree ferns. The state in which the roots (*Stigmariæ)* were found in this limestone led him to believe that Mr. Cumming had mistaken them for tree ferns. The depressed areolæ in this fossil have the lower portion of the radicle attached to them so as to give the appearance of a scar not much unlike that of a tree fern, but he convinced himself that they were unquestionably *Stigmaria.*

This portion of *Sigillaria* is often met with in the beds of limestone in the Yoredale series of Professor Phillips, as well as much lower down in the carboniferous series in the limestone of West Calder, near Bathgate, Scotland, where it occurs in great abundance. He also stated that the lower coal seams of Scotland containing workable beds were of considerably earlier date than the Posidonian Schists of the Isle of Man.

Dr. R. ANGUS SMITH, F.R.S., said that he had frequently visited some parts of the Continent, and had been a student of exhibitions such as included the useful arts. He was quite prepared to agree with those who saw a very great improvement in the touch of the workman, in the countries nearest to us in the north of Europe especially. In France the advance made was very great, and so also in Germany; but in neither of these cases did he consider that the exhibition showed the true state of the arts among the people. A true exhibition would give the proportion of bad and of good manufactures. There was no attempt to do this except in cases where the objects were viewed rather as curiosities, as for example, when costumes and architecture were introduced. When a number of knife makers showed their manufactures, it was clear that they all showed their best, but they did not show how many made knives that would scarcely cut, and even if they had done so, it would not have been sufficient to exhibit the state of the arts among the people. It would not inform us how many of the population had no knives set down to them at dinner, and how many houses had not a fork of any kind. In one of the countries which had made greatest progress and showed beautiful work of all kinds at the exhibition, he had been in an hotel where no fork whatever existed, and when he asked for a knife, the landlady handed him one from her pocket. Yet she and the landlord were very respectable looking people, and the house was clean. True, it was only in a village, but it was only a few miles from a considerable town. The people were not the poorest, but probably the richest in the village. He had not found any similar case in Great Britain. These were decent people of the lower classes in want of what we may consider the ordinary tools requisite in modern civilisation only three years ago. He did not wish to speak of any manufacture for the study of which he may have had special opportunity, but, speaking generally, he did not think that the modern changes had penetrated all classes of the community so deeply in France and Germany as the exhibition represented, whereas England had done more than it showed at the Paris exhibition. This is quite independent of the question which is best, and relates only to the useful arts.

It is clear, however, that wonderful advances have been made, and who makes them? He considered that they were made by the upper classes of the manufacturers, and used by the upper and middle classes, but had not descended universally even to the middle classes. When the general descent takes place, the manufacturers will have a much larger home market than they have at present. It is for the interest of England that this advance among the people should take place, as in some instances it will prevent competition in foreign markets; but whatever the commercial result may be, there is one lesson which we may all learn. Within the last thirty or forty years, the violent attempts to teach the people here by schools, mechanics' institutes, and lectures given or promoted by benevolent persons attaching themselves to various societies, have wearied the souls of all who have cooperated or even looked on with interest. In Germany, without any commotion, calmly and pleasantly, the youths have been trained in schools and colleges without number, and so thoroughly that they are able to supply foremen and managers to their own manufacturers' establishments, and to send a supply also to foreign countries, without diminishing the supply of that higher order of men of learning that have so long made Germany famous. In other words, whilst we have failed with the most violent efforts and with much noise to teach our own, they have succeeded not only to teach their own citizens, but to assist in educating the rest of the world. In this matter of education, the Governments have been able to mould the nation's mind, and to alter the habits of a leading portion of it in a very few years. A careful education would probably show its influence in less than ten years. True, many questions arise; and we may ask if education would not smooth down the peculiarities of the national character, and prevent it trusting to individual will and genius. Pedantic education might do this, but training is certainly as requisite in civil as in military affairs, and no one says that discipline diminishes the power of an army. It may be a question whether the uniform organisation of education in France is not calculated to produce too much equality in the minds of the nation, but it will certainly produce immense power in the aggregate. The numerous small states of Germany, each fostering its own schools and universities, seem best fitted to produce an intellectual activity. We see there, many universities, each developing its own peculiarities. They must make the nation more many-sided, and they have done so. At any rate, the present lesson seemed to be that the more intelligent part of a nation may be fashioned anew in a few years by its instructors as easily as a boy may be taught to be either a shoemaker or a tailor; cases of original bent excepted. We desire schools in this country, but cannot find them; there is no organisation for making them to the extent they are required. The genius of this nation, great as it really is, impatient of details, and looking vigorously to results, will never compete in a wild state with the disciplined army of thinkers and of manufacturers abroad. Those who do succeed here must train themselves, and to that training they owe their success, but it is hard work, and not to be expected of many. However, it is clear that an entirely new spirit may be made to animate society in the course of a single generation, and a nation may be born in our own day; also, as the result has been, so far as the useful arts are concerned, exactly that which the rulers desired, we must believe that governments or large combinations of men have the change in their power.

It was said that this progress in the arts did not necessarily go down to the whole of society. There may be several reasons for this: the upper classes may be far advanced and the lower very poor, even when there is much good feeling. In some of the mining districts of Germany, sending out the most intelligent miners to distant parts for centuries, and teaching their own with great care in schools which have been imitated, but not surpassed, we find a population extremely poor. Nature has presented little to them. We cannot expect all farmers to be equally rich, when soils differ so much. In such cases, however, we can expect a careful superintendence and a thoughtful mode of husbanding resources, mitigating the evils of poverty, and producing content where otherwise abject misery would exist, and that we find.

The exhibition shows how much may be done for the active minds of nations by a government fostering education, and the state of the same countries shows that intelligence, comfort, and wealth have been promoted also. Whilst the poorer parts alluded to, in Saxony for example, show that when from natural causes wealth has not been accumulated, education has produced intelligence to mitigate those evils which would otherwise have crushed the people. This education is owing to the activity of the governments. We learnt the lesson in this island once, and forgot it. We must be humble enough to learn it again from the world, instead of teaching it to them as we ought to have done.

The unassisted working classes in this country have raised themselves to an enjoyment of the products of civilisation not equalled probably anywhere in Europe, and their progress has been of longer duration; as we love our country we are still disposed to believe in its power of keeping in advance. But how could a regiment, however brave, advance with longbows against modern artillery?

We require education in the fundamental principles of physical science; the moral principles as found in literature are not, when alone, sufficient either for the higher cultivation of the mind or every pursuit of the useful arts. This applies to the rich and not merely to the poor.

## MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 4th, 1867.

J. Sidebotham, Esq., in the Chair.

The Rev. J. E. Vize showed and presented to the section four specimens of insects beautifully mounted in balsam by himself.

Mr. Sidebotham read the following "Note on the Ship-barnacle."

On the 28th of September I was at Lytham with my family. The day was very stormy, and the previous night there had been a strong south-west wind, and evidences of a very stormy sea outside the banks. Two of my children came running to tell me of a very strange creature that had been washed up on the shore. They had seen it from the pier and pointed it out to a sailor, thinking it was a large dog with long hair. On reaching the shore I found a fine mass of barnacles, *Pentalasinus anatifera,* attached to some staves of a cask, the whole being between four and five feet long. Several sailors had secured the prize, and were getting it on a truck to carry it away. The appearance was most remarkable, the hundreds of long tubes with their curious shells looking like what one could fancy the fabled Gorgon's head with its snaky locks.

The curiosity was carried to a yard where it was to be exhibited, and the bellman went round to announce it under the name of the sea lioness, or the great sea serpent.

I arranged with the proprietor for a private view, took my camera and a collodio-albumen plate, and obtained the photograph I now exhibit. The afternoon was very dull, and the plate would have done with a little longer exposure, but this along with the specimens I show, will give some idea of the strange appearance of this mass of creatures.

This barnacle is of interest as being the one figured by Gerard as the young of the barnacle goose. As some of our members may not have seen the book and read the quaint description, I have brought my copy of Gerard's Herbal for their amusement.

I may just mention that another mass of barnacles was washed up at Lytham, and also one at Blackpool, the same day or the day following. I did not see either, but, from description, neither was so fine as the one I have described.

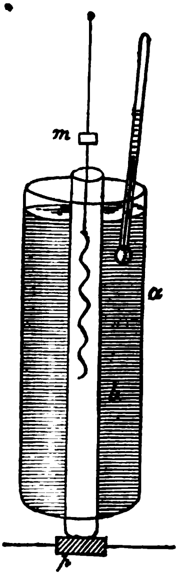
This mass of barnacles was evidently just such a one as that seen by Gerard at the Pile of Foulders. It is rare to have such a specimen on our coasts. The sailors at Lytham had never seen anything like it, although some of them were old men who had spent all their lives on the coast.

## Ordinary Meeting, November 26th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. James Stanley Kipping was elected an Ordinary Member of the Society.

"On a Thermometer unaffected by Radiation," by Dr. J. P. JOULE, F.R.S., &c.



In the annexed figure *a* is a copper tube about one foot long, and has a tube open at both ends in the centre. Water is poured into the space between the two tubes. In the centre tube there is a spiral of fine wire suspended by a filament of silk, and having a mirror at *m*. There is a lid at *p* which can be removed at pleasure from the lower end of the tube. When *p* is situated as in the figure, there can be no draught, and consequently the spiral with its mirror is at zero of the scale. But when *p* is removed, there is a current of air which turns the spiral, if the air in the tube has a different temperature from that of the outside atmosphere. In my apparatus, one degree Fah. produces an entire twist of the filament. I find that the temperature in the tube is generally warmer than in the outside atmosphere of a room, which must be owing to the conversion of light and other radiations into heat on coming into contact with the copper tube. I have tried the apparatus in the open air on a still day, with the same result. Of course when there is wind the effect is masked, but I feel confident that by increasing the length of the tube, making it 30 feet for instance, and using certain precautions, this difficulty may be overcome.

"On Solar Radiation," by JOSEPH BAXENDELL, F.R.A.S.

Although observations of solar radiation have now been regularly made for several years at various public observatories, and by many amateur meteorologists, I am not aware that any useful or important result has yet been deduced from them. It seems to be generally supposed that the disturbing influences which affect the indications of the black-bulb thermometer are so uncertain and irregular in their action as to render it almost hopeless to expect that any new and valuable result can be obtained from them. On comparing sets of observations made by different observers, the most startling and discouraging discrepancies are often found to exist, for which, in the absence of any information as to the exact circumstances under which the observations were made, it is impossible to account satisfactorily. A few years ago an enquiry in which I was engaged, led me to undertake a discussion of the Greenwich solar radiation observations; but the results proved so perplexingly anomalous and unsatisfactory, that I could not venture to place any reliance upon them. Having, however, recently become possessed, through the kindness of the Rev. Robert Main, F.R.S., and the Trustees of the Radcliffe Observatory, Oxford, of copies of the volumes of Radcliffe Observations for the years 1858 to 1864, and finding that they contained a valuable series of solar radiation observations, I have been led to resume the subject, and although the enquiry is still incomplete, I have thought that some of the results already obtained are sufficiently curious and remarkable to render it desirable to bring them before the society, in order that attention may be drawn to a much neglected but highly interesting branch of enquiry, and to the necessity of devising and adopting a more reliable and systematic method of determining the intensity of solar radiation than the one at present in use. It is much to be regretted that no regular and long-continued series of observations has ever been made with Sir John Herschel's actinometer, since there can hardly be any doubt that such a series would yield much more accurate and reliable results than can be obtained by the use of either the ordinary or the *vacuo* black-bulb thermometer.

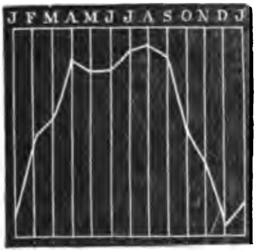
The Oxford solar radiation observations commenced on the 2nd of January, 1856, and were made with a thermometer supplied by Negretti and Zambra. On the 3rd of November, 1858, this thermometer was unfortunately broken, but a new thermometer being obtained, the observations were resumed on the 1st of December, 1858, and continued uninterruptedly till the 11th of September, 1864, when the second thermometer was also broken; a third thermometer was obtained and brought into use on the 17th of September, and the observations continued with it to the end of the year. The entire series of observations during the nine years 1856–1864, must therefore be regarded as consisting of two distinct series, made with different thermometers, and therefore not strictly comparable with each other. I have, in consequence, thought it desirable to confine the discussion, in the first instance, to the observations made with the second thermometer during the years 1859–1864, more especially as in 1857 the readings of the first thermometer were only to the nearest whole degree, and in 1858 the month of November was omitted; and in order to make six complete years, I have included the observations made with the third thermometer from the 17th of September to the end of 1864.

The following table shows the mean monthly and annual differences between the maximum temperature in the shade and that in the sun during the six years 1859–1864, and the monthly and annual means of the entire series:—

| | 1859 | 1860 | 1861 | 1862 | 1863 | 1864 | Monthly Means |
|---|---|---|---|---|---|---|---|
| | ° | ° | ° | ° | ° | ° | ° |
| January... | 7·3 | 5·7 | 5·7 | 4·2 | 4·9 | 5·3 | 5·51 |
| February.. | 10·9 | 11·5 | 9·0 | 6·8 | 9·2 | 7.6 | 9·16 |
| March ... | 11·7 | 11·2 | 10·4 | 7·6 | 10·2 | 9·4 | 10·08 |
| April ...... | 13·2 | 16·1 | 15·4 | 11·6 | 11·8 | 11·3 | 13·23 |
| May ...... | 15·0 | 13·5 | 13·5 | 11·6 | 11·5 | 12·3 | 12·90 |
| June ...... | 16·0 | 12·3 | 14·4 | 10·1 | 13·2 | 11·6 | 12·93 |
| July ...... | 17·8 | 12·8 | 12·4 | 13·4 | 14·2 | 12·3 | 13·81 |
| August ... | 16·3 | 12·5 | 15·1 | 13·3 | 12·5 | 14·5 | 14·03 |
| September | 16·3 | 11·8 | 14·1 | 12·5 | 12·8 | 14·8 | 13·71 |
| October... | 11·1 | 8·7 | 11·0 | 8·8 | 8·4 | 10·0 | 9·66 |
| November | 12·4 | 8·4 | 7·9 | 8·2 | 5·4 | 6·5 | 8·00 |
| December. | 6·2 | 4·9 | 6·0 | 4·4 | 3·3 | 2·2 | 4·50 |
| Annual Means | 12·85 | 10·78 | 11·24 | 9·37 | 9·78 | 9·81 | |

A projection of the mean monthly values gives the curve in diagram A, from which it will be seen that the maximum amount of radiation occurs in August, or about a month later than the time of maximum temperature; whilst the minimum occurs in December, about a month earlier than the time of minimum temperature. There is also a slight secondary maximum in April. This curve, therefore, differs from that of any other element of temperature; but observing that the time of minimum corresponded exactly with that of maximum in the curve laid down from the numbers given in a table which I communicated to the Physical section on the 5th of March, 1863, showing the monthly sums of the oscillations of mean daily temperature at Greenwich during the thirteen years 1848-60, and also the mean daily values for the different months; and that the time of maximum agreed nearly with that of minimum disturbance of mean daily temperature, it occurred to me that the two phenomena might be closely connected with and dependent upon each

A



other, and that the annual values of the solar radiation might bear a constant ratio to the corresponding values of the oscillations of mean daily temperature. The annual sums and mean daily values of the oscillations of mean daily temperature at Oxford for the six years, and the corresponding ratios, were therefore calculated, and found to be as follows:—

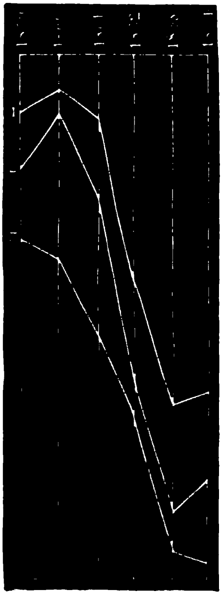
| | Annual Sums.<br>° | Daily Means.<br>° | Ratios. |
|---|---|---|---|
| 1859 | 1182·3 | 3·24 | 3·96 |
| 1860 | 953·0 | 2·60 | 4·14 |
| 1861 | 1054·9 | 2·89 | 3·88 |
| 1862 | 1043·4 | 2·85 | 3·28 |
| 1863 | 1198·4 | 3·28 | 2·98 |
| 1864 | 1158·0 | 3·17 | 3·09 |

The numbers in the last column showed that the ratio was not even approximately constant, but that, on the contrary, it was subject to considerable change. I therefore concluded that the calorific intensity of the sun's light was also subject to variation; and a glance at the course of the numbers at once suggested that this variation would be found to follow that of solar spot frequency. Referring to Schwabe's observations of the solar spots, we have the following numbers of groups observed by him:—

| | | | |
|---|---|---|---|
| 1859 | 205 | 1862 | 160 |
| 1860 | 211 | 1863 | 124 |
| 1861 | 204 | 1864 | 130 |

A projection of these numbers, and of the corresponding ratios of solar radiation to the oscillation of mean daily temperature is shown in diagram B; No. 1 is the curve of sun spot frequency, and No. 2 that of the ratios; and it will be seen that the similarity

B



in form of the two curves is remarkably striking, and apparently conclusive, as to the connection between the two classes of phenomena.

Assuming that changes in the heating power of the sun's rays follow the course of the changes in solar spot frequency, it seemed probable that the ratio of the difference between the maximum temperature in the shade and in the sun, to the difference between the mean daily temperature,—or better, perhaps, the temperature of evaporation,—and the maximum temperature in the shade, would also exhibit corresponding changes. It will be seen that the following results strongly support this view :—

| | Max. Temp. in Sun, less Max. Temp. in Shade.<br>° | Max Temp. in Shade, less Mean Temp. of Evaporation.<br>° | Ratio. |
|---|---|---|---|
| 1859 | 12·85 | 10·06 | 1·27 |
| 1860 | 10·78 | 8·63 | 1.25 |
| 1861 | 11·24 | 9·52 | 1·18 |
| 1862 | 9·37 | 9·28 | 1·09 |
| 1863 | 9·78 | 10·38 | 0·94 |
| 1864 | 9·81 | 10·57 | 0·93 |

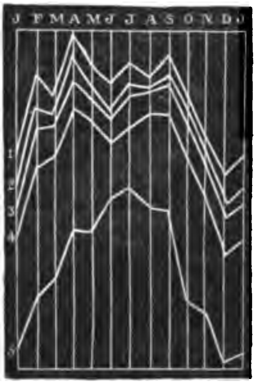
The line No. 3 in diagram B is a projection of these ratios. It would probably be better to employ the maximum instead of the mean temperature of evaporation, but this is not given in the printed observations.

The mean values of solar radiation given above in the first table, are deduced from observations made on every day in the year, and therefore in every possible state of the atmosphere—clear, cloudy, rainy, foggy, calm, stormy, &c.—but it was evidently desirable to determine the calorific intensity of the sun's rays on days when the sky was cloudless at the time of maximum temperature. The printed observations, however, do not always show when this was the case, and it became necessary to adopt some arbitrary principle of selection. As the one which appeared to me to be least open to objection, I selected, in the first instance, the highest value in each month of the six years, and taking the means obtained the following numbers, a projection of which is shown in curve No. 1 of diagram C.

| | ° | | ° |
|---|---|---|---|
| January | 16·4 | July | 21·1 |
| February | 20·5 | August | 20·3 |
| March | 19·3 | September | 21·8 |
| April | 22·6 | October | 19·6 |
| May | 20·9 | November | 17·2 |
| June | 20·1 | December | 15·3 |

This curve exhibits two principal maxima in April and September, and a low minimum in December.

C



Taking next the means derived from the three highest values in each month, and then those of the five highest, we have curves No. 2 and No. 3. The slight irregularities in the first curve have now disappeared, and curve 3 has two well defined maxima in April and September, a principal minimum in December, and secondary minimum in June.

Proceeding now a step farther and taking the ten highest values in each month, we have curve No. 4. The first maximum still occurs in April, but a slight change has taken place in the time of the second, which now occurs in the latter part of August instead of the middle of September; but no change has taken place in the times of the two minima.

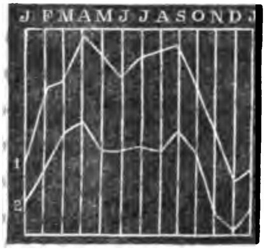
Finally, taking the ten lowest values in each month we have curve No. 5. Here the first maximum has almost disappeared, though still occurring in April, and the second maximum has advanced into July. The principal minimum is still in December, but the secondary minimum occurs in May instead of June, though, like the first maximum, it has almost disappeared.

It seems therefore, from a comparison of these five curves, that the general curve in diagram A, which is laid down from the monthly means of all the observations, may be regarded as compounded of two primary curves,—one having two well marked maxima in April and September, and two minima in December and June, and the other having only one maximum in July and one minimum in December. From curves 3 and 4 it appears that the influence of the second primary curve upon the general features of the first, does not become apparent while the number of selected days is limited to five per month. The first primary curve therefore represents the monthly changes in the calorific intensity of the sun's direct rays on cloudless or nearly cloudless days, and it leads us to this remarkable conclusion that the heating power of direct sun-light on clear days in the latitude of the British Islands, is greater in the months of April and September than in the month of June, when the sun attains his greatest meridian altitude.

The second primary curve represents the intensities when the solar rays are more or less intercepted and dispersed by clouds and haze, and it approaches in form the annual curve of temperature; but the first curve, of which curve 3, diagram C, may be taken to be a fair representation, is unlike that of any other thermometric element. It has, however, a remarkably close resemblance to the curve representing the monthly changes of one of the magnetic elements, namely, that of the monthly means of the diurnal ranges of the magnetic needle. In the volume of the Greenwich Observations for 1859, the Astronomer Royal has given a table showing the monthly means of the diurnal ranges of the magnetometer at Greenwich from ten years' observations, 1848–1857. The numbers in this table are as follow:—

| | ′ | | ′ |
|---|---|---|---|
| January | 9·5 | July | 12·7 |
| February | 11·3 | August | 12·6 |
| March | 13·1 | September | 13·5 |
| April | 14·0 | October | 12·2 |
| May | 12·7 | November | 9·3 |
| June | 12·6 | December | 8·2 |

D

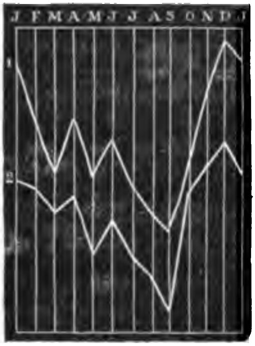


The line No. 2, in diagram D, is a projection of these numbers, and the line No. 1 is a repetition of curve 3, diagram C. It will be seen that the maxima and minima of the one correspond exactly with those of the other, and we are therefore entitled to conclude that the two phenomena are intimately connected, and that the causes which produce variations in the intensity of solar radiation also affect the diurnal oscillations of the magnetic needle.

It was supposed by Sir William Herschel that the emission of heat from the sun varied according to the greater or less frequency of solar spots, and he attempted to support this view by a comparison of the prices of grain in years when the solar spots were numerous, with those in years when few or no spots were seen; but this kind of evidence was generally regarded as unsatisfactory, and it is, I believe, now generally admitted by meteorologists that the many valuable series of thermometrical observations which have since been made in various localities, have hitherto failed to afford any decided indications of a periodical change in the element of mean annual temperature. The question therefore naturally arises, if the intensity of solar radiation varies in a period corresponding with the period of solar spot frequency, why does not the element of mean annual temperature exhibit a similar periodical change? For the present, I will merely suggest that a clue to the correct explanation of this apparent anomaly may perhaps be found in the conclusion at which Professor Forbes arrived from a discussion of his observations made in Switzerland, in conjunction with Professor Kaemtz, on the intensity of solar radiation at different elevations in the atmosphere, namely, that the heating rays of the sun consist of two kinds,—one kind, of high intensity, which suffers little or no loss in passing through the atmosphere; and the other, of much lower intensity, and therefore much more absorbable. Now, if we suppose that the relative quantities of the two kinds of rays undergo periodical changes, such that the maximum of the one corresponds to the minimum of the other, it will be evident that when the lower strata of the atmosphere receive less heat by absorption, owing to a diminished supply of the rays of low intensity, the ground will receive more from the simultaneous increase in the quantity of the rays of high intensity, and this being communicated to the lower atmosphere by conduction and convection, as well as by radiation upwards, will restore the equilibrium, and tend to produce uniformity in the mean annual temperatures.

On a former occasion I urged the desirability of giving more attention than has hitherto been done to the oscillations of mean daily temperature, and have shown, in the present discussion, their importance in connection with the subject of solar radiation. I may now add that they appear to have an intimate connection with magnetic phenomena. Among the tables given by the Astronomer Royal in the volume of Greenwich observations for 1859, is one showing the monthly mean horizontal magnetic force at Greenwich for the nine years 1849–1857, corrected for secular variation. The line No. 2, in diagram E, is a projection of the numbers in this table, and the line No. 1, in the same diagram, shows the monthly mean daily oscillations of temperature. It will be seen that every rise or fall in one curve has a corresponding rise or fall in the other. It is, in fact, rare to meet with so close an agreement between curves representing phenomena so widely different as the earth's horizontal magnetic intensity and the mean daily temperature of the atmosphere.

E



The conclusions arrived at from this discussion may be briefly recapitulated as follows:—

1st.—That the calorific intensity of the sun's light is subject to periodical changes, the maxima and minima of which correspond respectively with those of solar spot frequency.

2nd.—That the intensity of a ray of direct sunlight on its arrival at the earth's surface, in the latitude of Oxford, is greater in April and September than in June, when the sun's meridian altitude is greatest.

3rd.—That the curve representing the mean monthly values of solar radiation on cloudless days has its times of maxima and minima corresponding with those of the curve representing the mean monthly diurnal ranges of the magnetometer.

4th.—It seems probable that the heating rays of the sun consist of two kinds differing considerably in intensity, and being subject to periodical changes, the times of maximum of one kind, and those of minimum of the other, corresponding respectively to the times of maximum frequency of solar spots.

5th.—That the oscillations of mean daily temperature are intimately connected with the changes which take place in the earth's horizontal magnetic intensity.

I have said that the results derived from the Greenwich observations of solar radiation were anomalous and unsatisfactory. It may, therefore, be necessary to state that these observations appear to have been made under circumstances not favourable to the accurate determination of the intensity of solar radiation at all seasons of the year. The values derived from them appear to be too high in summer, and too low in winter. Take, for instance, any winter month, say December, 1857. The mean difference for the month between the maximum in sun and the maximum in shade was only 1·7°: at Oxford, it was 6·1°. The highest value during the month was 4·5° at Greenwich: at Oxford, it was 19·0°. At Greenwich there were only seven days on which the difference exceeded 3·0°: at Oxford there were seventeen. And yet at Greenwich the month was unusually fine and dry, only 0·36 of an inch of rain fell, and several days appear to have been nearly, if not quite, cloudless. Under these circumstances, it is difficult to understand why the black-bulb thermometer, if properly exposed, did not register much greater differences. In 1859 there was a remarkable and unaccountable falling off in the summer values, and the mean for the year was decidedly lower than the mean of any of the three preceding years; but at Oxford it was higher. I can only account for this remarkable difference by supposing that some change was made in the position of the solar radiation thermometer at Greenwich in the early part of the year.

From the beginning of 1860 to the end of 1864 the observations at Greenwich were made with a black bulb thermometer in vacuo, and it is satisfactory to find that the course of the annual means is in tolerably fair agreement with that of the results obtained at Oxford with the ordinary black bulb thermometer.

"Solar Radiation Observations, made at Old Trafford, Manchester," by G. V. VERNON, F.R.A.S., F.M.S.

Mr. Joseph Baxendell, F.R.A.S., having at a recent meeting of the Physical Section, read a paper on this subject showing some peculiarities of the distribution of Solar Radiation throughout the year, I have at his suggestion reduced and tabulated my observations bearing upon this point down to the end of 1866, making eleven years in all. Unfortunately the month of August is deficient in six years out of the eleven. The observations were made with Negretti and Zambra's patent maximum thermometers, neither thermometer having any index error, but reading correctly with the standard throughout the scale.

Taking the differences between the mean maximum black bulb reading in the sun, and the mean maximum reading in the shade for each month, we find a minimum in December and a maximum in July. The maximum occurs in the warmest month, but the minimum appears to occur some time before the coldest month arrives, viz.: January. If we take the differences between one month and the succeeding one we find the following values:

| | ° |
|---|---|
| January to February | + 2·02 |
| February to March | + 4·64 |
| March to April | + 4·06 |
| April to May | + 1·67 |
| May to June | — 0·09 |
| June to July | + 0·56 |
| July to August | — 2·39 |
| August to September | — 2·07 |
| September to October | — 3·46 |
| October to November | — 3·21 |
| November to December | — 1·32 |
| December to January | + 0·59 |

From my observations the greatest increase in the amount of solar radiation appears to take place in March, and nearly as much in April, afterwards, up to July, the warmest month, the increase is remarkably small, and in June shows a slight amount of decrease. These figures do not quite agree with those deduced from the Oxford observations by Mr. Baxendell, but they very clearly confirm the fact that a large amount of solar heat is dispersed in some way, and without a corresponding effect upon the thermometer, or the difference between the maximum in the sun and the maximum in the shade would show a large increase in the months of maximum temperature.

The greatest effect of solar radiation, therefore, appears to occur in the spring, and this is in accordance with the very rapid growth of vegetation we often see suddenly take place in early spring.

### OLD TRAFFORD, MANCHESTER.—SOLAR RADIATION.

| YEAR. | JANUARY. | | | FEBRUARY. | | | MARCH. | | |
|---|---|---|---|---|---|---|---|---|---|
| | Max. Bk. Bulb in Sun. | Max. in Shade. | Diff. | Max. Bk. Bulb in Sun. | Max. in Shade. | Diff. | Max. Bk. Bulb in Sun. | Max. in Shade. | Diff. |
| | ° | ° | ° | ° | ° | ° | ° | ° | ° |
| 1856 | 45·4 | 43·9 | 1·5 | 49·1 | 46·9 | 2·2 | 54·6 | 48·2 | 6·4 |
| 1857 | 43·1 | 41·7 | 1·4 | 49·9 | 46·4 | 3·5 | 54·4 | 47·7 | 6·7 |
| 1858 | 49·8 | 44·8 | 5·0 | 47·9 | 44·1 | 3·8 | 56·3 | 50·7 | 5·6 |
| 1859 | 51·1 | 46·0 | 5·1 | 53·6 | 47·4 | 6·2 | 57·6 | 50·8 | 6·8 |
| 1860 | 46·4 | 42·9 | 3·5 | 47·1 | 41·9 | 5·2 | 54·3 | 46·4 | 7·9 |
| 1861 | 41·3 | 40·4 | 0·9 | 52·2 | 46·8 | 5·4 | 60·4 | 49·6 | 10·8 |
| 1862 | 44·6 | 43·2 | 1·4 | 49·7 | 46·3 | 3·4 | 56·8 | 47·9 | 8·9 |
| 1863 | ... | ... | ... | 55·2 | 49·6 | 5·6 | 62·8 | 51·6 | 11·2 |
| 1864 | 43·4 | 41·7 | 1·7 | 45·5 | 41·6 | 3·9 | 59·8 | 47·9 | 11·9 |
| 1865 | 43·6 | 41·3 | 2·3 | 46·8 | 42·3 | 4·5 | 57·0 | 44·1 | 12·9 |
| 1866 | 49·0 | 47·3 | 1·7 | 52·9 | 46·4 | 6·5 | 58·7 | 47·6 | 11·1 |
| Means | 45·77 | 43·32 | 2·45 | 49·90 | 45·43 | 4·47 | 57·52 | 48·41 | 9·11 |
| | APRIL. | | | MAY. | | | JUNE. | | |
| 1856 | 66·3 | 55·9 | 10·4 | 70·6 | 59·7 | 10·9 | 76·4 | 64·3 | 10·1 |
| 1857 | 65·6 | 53·8 | 11·8 | 75·2 | 62·2 | 13·0 | 82·6 | 73·6 | 8·0 |
| 1858 | 66·5 | 57·9 | 8·6 | 72·6 | 59·5 | 13·1 | 82·7 | 73·8 | 8·9 |
| 1859 | 61·9 | 53·9 | 8·0 | 79·4 | 65·7 | 13·7 | 83·5 | 68·8 | 14·7 |
| 1860 | 63·0 | 54·4 | 8·6 | 81·7 | 65·4 | 16·3 | 81·3 | 63·9 | 17·4 |
| 1861 | 72·6 | 55·5 | 17·1 | 73·2 | 59·4 | 13·8 | 86·2 | 68·6 | 17·6 |
| 1862 | 70·8 | 56·0 | 14·8 | 81·2 | 62·7 | 18·5 | ... | ... | ... |
| 1863 | 69·9 | 56·2 | 13·7 | 74·8 | 59·5 | 15·3 | 81·4 | 65·0 | 16·4 |
| 1864 | 76·0 | 59·0 | 17·0 | 81·4 | 66·5 | 14·9 | 81·2 | 66·2 | 15·0 |
| 1865 | 81·5 | 63·5 | 18·0 | 79·3 | 65·0 | 14·3 | 93·0 | 72·8 | 20·2 |
| 1866 | 74·1 | 57·3 | 16·8 | 82·1 | 62·2 | 19·9 | 88·0 | 71·8 | 16·2 |
| Means | 69·84 | 56·67 | 13·17 | 77·37 | 62·53 | 14·84 | 83·63 | 68·88 | 14·75 |

## SOLAR RADIATION—(Continued).

| | July. | | | August. | | | September. | | |
|---|---|---|---|---|---|---|---|---|---|
| | Max. Bk. Bulb in Sun. | Max. in Shade. | Diff. | Max. Bk. Bulb in Sun. | Max. in Shade. | Diff. | Max. Bk. Bulb in Sun. | Max. in Shade. | Diff. |
| | ° | ° | ° | ° | ° | ° | ° | ° | ° |
| 1856 | 80·7 | 68·0 | 12·7 | ... | ... | ... | 74·3 | 63·9 | 10·4 |
| 1857 | 83·6 | 69·6 | 14·0 | 82·5 | 72·0 | 10·5 | 75·8 | 67·4 | 8·4 |
| 1858 | 79·6 | 66·8 | 12·8 | ... | ... | ... | 79·7 | 66·3 | 13·4 |
| 1859 | 86·7 | 74·2 | 12·5 | ... | ... | ... | 76·2 | 63·7 | 12·5 |
| 1860 | 83·8 | 66·8 | 17·0 | ... | ... | ... | 71·8 | 59·5 | 12·3 |
| 1861 | 85·8 | 67·1 | 18·7 | ... | ... | ... | 78·5 | 63·9 | 14·6 |
| 1862 | 80·3 | 64·8 | 15·5 | ... | ... | ... | 74·2 | 62·6 | 11·6 |
| 1863 | 87·3 | 68·9 | 18·4 | 83·4 | 66·0 | 17·4 | 70·1 | 58·5 | 11·6 |
| 1864 | 86·0 | 70·9 | 15·1 | 80·9 | 68·0 | 12·9 | 73·3 | 65·2 | 8·1 |
| 1865 | 94·4 | 76·6 | 17·8 | 81·7 | 68·7 | 13·0 | 82·3 | 73·4 | 8·9 |
| 1866 | 84·2 | 70·3 | 13·9 | 77·8 | 67·0 | 10·8 | 70·0 | 62·4 | 7·6 |
| Means | 84·76 | 69·45 | 15·31 | 81·26 | 68·84 | 12·92 | 75·11 | 64·26 | 10·85 |
| | October. | | | November. | | | December. | | |
| 1856 | 66·3 | 59·4 | 6·9 | 47·6 | 45·7 | 1·9 | 45·4 | 44·8 | 0·6 |
| 1857 | 66·9 | 59·8 | 7·1 | 56·5 | 50·1 | 6·4 | 57·5 | 50·8 | 6·7 |
| 1858 | 63·8 | 55·0 | 8·8 | 51·8 | 46·6 | 5·2 | 50·2 | 45·3 | 4·9 |
| 1859 | 66·1 | 56·5 | 9·6 | 51·5 | 46·9 | 4·6 | 42·6 | 39·8 | 2·8 |
| 1860 | 64·1 | 55·4 | 8·7 | 49·0 | 45·3 | 3·7 | 40·8 | 39·5 | 1·3 |
| 1861 | 64·8 | 60·6 | 4·2 | 47·2 | 45·9 | 1·3 | 44·0 | 44·8 | −0·8 |
| 1862 | 61·9 | 56·5 | 5·4 | 44·9 | 43·7 | 1·2 | 48·2 | 48·0 | 0·2 |
| 1863 | 62·0 | 55·9 | 6·1 | 52·3 | 50·7 | 1·6 | 48·6 | 48·3 | 0·3 |
| 1864 | 61·7 | 57·0 | 4·7 | 49·1 | 48·2 | 0·9 | 41·9 | 43·2 | 1·3 |
| 1865 | 62·4 | 58·6 | 3·8 | 51·7 | 50·5 | 1·2 | 47·8 | 47·4 | 0·4 |
| 1866 | 62·5 | 58·1 | 4·4 | 56·4 | 49·5 | 6·9 | 52·8 | 47·5 | 5·3 |
| Means | 63·86 | 57·47 | 6·39 | 50·73 | 47·55 | 3·18 | 47·26 | 45·40 | 1·86 |

## MONTHLY VALUES.

| | Max. in Sun. | Max. in Shade. | Diff. |
|---|---|---|---|
| | ° | ° | ° |
| January | 45·77 | 43·32 | 2·45 |
| February | 49·90 | 45·43 | 4·47 |
| March | 57·52 | 48·41 | 9·11 |
| April | 69·84 | 56·67 | 13·17 |
| May | 77·37 | 62·53 | 14·84 |
| June | 83·63 | 68·88 | 14·75 |
| July | 84·76 | 69·45 | 15·31 |
| August | 81·26 | 68·34 | 12·92 |
| September | 75·11 | 64·26 | 10·85 |
| October | 63·86 | 57·47 | 6·39 |
| November | 50·73 | 47·55 | 3·18 |
| December | 47·26 | 45·40 | 1·86 |

"A Comparison of Solar Radiation on the Grass, and at Six Feet from the Ground," by **Thomas Mackereth**, F.R.A.S., F.M.S.

As the Corporation of the Borough of Salford has recently erected a structure in front of the Town-Hall for meteorological purposes, and as no better position could be found for the solar radiation thermometer than on the apex of the roof of the shade-stand, which is six feet from the ground, I fixed a similar thermometer in a similar position at Eccles in order that I might institute a comparison of solar radiation between Eccles and the Borough. Though I anticipated very different results from a thermometer thus placed, than I had found from a similar thermometer placed upon the grass, I was not prepared to find them so widely different as I shall here present them. The instruments used are the ordinary exposed black-bulb self-registering thermometers, duly compared. I have also added a column of comparison from results obtained by a self-registering black-bulb thermometer placed *in vacuo* on the grass. The observations are for the month of October only, the only complete month in which all the instruments have been used.

| DATE. | Exposed Black Bulb Therm. on Grass. | Exposed Black Bulb Therm. 6 feet from ground. | Difference. | Black Bulb Therm. *in vacuo* on grass. | Difference from Therm. 6 feet from ground. |
|---|---|---|---|---|---|
| 1867 | ° | ° | ° | ° | ° |
| Oct. 1 | 69·2 | 73·0 | + 3·8 | 83·2 | +10·2 |
| „ 2 | 57·5 | 57·6 | + 0·1 | 62·1 | + 4·5 |
| „ 3 | 61·4 | 62·7 | + 1·3 | 71·0 | + 8·3 |
| „ 4 | 52·5 | 57·0 | + 4·5 | 60·0 | + 3·0 |
| „ 5 | 66·5 | 68·2 | + 1·7 | 72·2 | + 4·0 |
| „ 6 | 64·0 | 68·0 | + 4·0 | 67·7 | — 0·3 |
| „ 7 | 60·7 | 67·2 | + 6·5 | 77·4 | +10·2 |
| „ 8 | 54·9 | 60·1 | + 5·2 | 68·0 | + 7·9 |
| „ 9 | 46·7 | 45·8 | — 0·9 | 48·9 | + 3·1 |
| „ 10 | 62·0 | 67·6 | + 5·6 | 75·8 | + 8·2 |
| „ 11 | 51·3 | 52·5 | + 1·2 | 54·0 | + 1·5 |
| „ 12 | 56·2 | 56·5 | + 0·3 | 56·5 | 0·0 |
| „ 13 | 53·4 | 52·7 | — 0·7 | 57·0 | + 4·3 |
| „ 14 | 60·0 | 61·5 | + 1·5 | 61·2 | — 0·3 |
| „ 15 | 61·0 | 63·6 | + 2·6 | 65·0 | + 1·4 |
| „ 16 | 64·8 | 74·3 | + 9·5 | 71·0 | — 3·3 |
| „ 17 | 63·0 | 74·8 | +11·8 | 71·1 | — 3·7 |
| „ 18 | 62·5 | 70·7 | + 8·2 | 67·5 | — 3·2 |
| „ 19 | 57·3 | 68·2 | +10·9 | 62·0 | — 6·2 |
| „ 20 | 60·5 | 68·9 | + 8·4 | 64·4 | — 4·5 |
| „ 21 | 60·0 | 63·8 | + 3·8 | 63·1 | — 0·7 |
| „ 22 | 72.8 | 82·1 | + 9·3 | 79·8 | — 2·3 |
| „ 23 | 64·0 | 64·8 | + 0·8 | 65·6 | + 0·8 |
| „ 24 | 54·2 | 55·4 | + 1·2 | 56·0 | + 0·6 |
| „ 25 | 51·8 | 52·5 | + 0·7 | 53·0 | + 0·5 |
| „ 26 | 62·0 | 69·3 | + 7·3 | 65·3 | — 4·0 |
| „ 27 | 50·3 | 51·4 | + 1·1 | 53·7 | + 2·3 |
| „ 28 | 52·6 | 61·9 | + 9·3 | 58·7 | — 3·2 |
| „ 29 | 59·4 | 65·4 | + 6·0 | 62·0 | — 3·4 |
| „ 30 | 61·0 | 68·8 | + 7·8 | 67·0 | — 1·8 |
| „ 31 | 58·3 | 64·8 | + 6·5 | 64·2 | — 0·6 |
| Mean… | 59·0 | 62·9 | + 3·9 | 64·6 | + 1·7 |

It appears therefore that, for the month, at a position six feet above the ground, there is a mean difference of solar influence of nearly 4 degrees above that upon the grass; and in some extreme instances the difference has been from 10 to nearly 12 degrees. A thermometer placed *in vacuo* on the grass has only exceeded this difference by 1·7 degree. These results show how important it is that some definite principle should be adopted in the placing of solar thermometers, as certainly no comparison can be made between the amount of solar radiation at any two or more places, unless some common plan of placing the instruments be adopted.

## PHYSICAL AND MATHEMATICAL SECTION.

### October 10th, 1867.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

"On Solar Radiation," by J. BAXENDELL, F.R.A.S.

[This paper was afterwards read at the Ordinary Meeting of the Society, held November 26th, 1867, see page 36.]

### November 7th, 1867.

ROBERT WORTHINGTON, F.R.A.S., President of the Section, in the Chair.

"A Comparison of Solar Radiation on the Grass, and at Six feet from the Ground," by THOMAS MACKERETH, F.R.A.S.

[This paper was afterwards read at the Ordinary Meeting of the Society, held November 26th, 1867, see page 50.]

"Note on the Colour of the Moon during Eclipses," by A. BROTHERS, F.R.A.S.

On the night of October 4th, 1865, there was a partial eclipse of the moon, when about one-third of the disc was obscured. My time and attention on this occasion were chiefly directed to some Photographic experiments, and it will be remembered I then obtained pictures of the eclipse at short intervals, from the commencement to the end. At about the greatest phase I looked at the moon through the telescope with an eye piece of low power, for the purpose of noticing whether the eclipsed portion showed colour, and at once saw that the part of the moon most deeply within the shadow was of a decided copper colour, such as I had seen some years previously during a total eclipse of the moon.

The eclipse which occurred on the night of the 13th September last, was rather more favorable for detecting the presence of colour, as seven-tenths of the moon were covered by the earth's shadow; the weather being clear many persons have recorded their observations, and as the colour of the moon is one of the chief features of a lunar eclipse, this point attracted considerable attention.

Mr. Browning says, in a letter which appeared in the Astronomical Register—"I looked most carefully for colour both with the 10½ silvered glass reflector furnished with an achromatic eyepiece of very low power, and also with a five-feet refractor; with neither could I detect a trace."

Mr. Slack who was also observing with a silvered glass reflector, and in the same locality as Mr. Browning says,—"After 12, the eclipsed limb grew noticeably redder, the red coppery tint chiefly affected the lower parts of the obscured limb, but was visible further in, gradually blending with the inky tints presented by the umbra at its advancing edge.—(Int. Obs. October.)

Mr. Weston, observing at Landsdown, near Bath, says,—"The prevailing colours were red-bluish and grey, *and grey*; the redness increased towards the darkened edge of the moon."—(Monthly Not. 9—xxvii.)

Many other observers speak of the presence of colour, but on the other hand a few say they did not notice any, the eclipsed portion of the moon merely having a darkened appearance.

On this occasion I did not make any Photographs, as I could not expect results materially differing from the last, and I gave my whole attention to observing the progress of the eclipse through the telescope, which is a refractor of 5 inches aperture. As to the question of the presence of colour, I can most distinctly say that *colour* gave the moon a very beautiful appearance, and it seemed to me the most interesting feature of the eclipse. The beauty of the moon's surface appeared to increase as the penumbral shadow stole over its surface, and until the shadow itself was considerably advanced, all the details of the lunar surface could be distinctly made out, and during the whole period of the eclipse some of the brighter points of light within the shadow continued visible, as did also the entire disc with many of the details of light and shade. The colour of the eclipsed limb was of a coppery hue, much brighter towards the part most deeply within the shadow. The part of the moon not eclipsed was of a beautiful bluish-grey colour.

That the appearance of colour cannot be caused by the telescope or by peculiarities in the eyes of the observers is, I think, proved by the fact, that the same colours are seen whether refractors or reflectors, either of metal or silvered glass, be used; and, as the majority of observers see colour, the eyes of those who remark the absence of it are perhaps temporarily afflicted with colour blindness;—the bright light from the uneclipsed portion of the moon may be sufficient to produce this in some persons. If an observer, after looking at the moon through the telescope, attempts to look at objects while the other eye remains closed, it will be found that until the retina has recovered from the excess of light everything will appear misty—in fact, the eye is partially blinded, and it may be that some eyes are sufficiently sensitive to be affected by the diminished lustre of the moon, and may thus be prevented seeing colour, which there can be no doubt the lunar surface presents during an eclipse.

Some observers have remarked on the difficulty of detecting the first appearance of the shadow, and although the exact time is known, the real shadow is not seen until many seconds or perhaps minutes after the predicted time. In the present instance, I watched very carefully for the first appearance of the shadow, and, having previously ascertained the error of my watch, I took my position at the telescope, at the same time I requested a friend to take particular notice of the time at the moment I saw the shadow. The result thus obtained was within twenty seconds of the time given in the Nautical Almanac, clearly showing that under favourable conditions, and, if the attention be given carefully to the subject, the real shadow may be detected very near the predicted time.

## Ordinary Meeting, December 10th, 1867.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Samuel Broughton was elected an Ordinary Member of the Society.

"On the Age of the Hæmatite Iron Deposits of Furness," by E. W. Binney, F.R.S., F.G.S.

The great value of the vast deposits of hæmatite iron ore found in the Carboniferous Limestone of North Lancashire, and in a similar position near Whitehaven, in Cumberland, renders any information as to the date of their formation of considerable importance. There is no doubt that the Carboniferous Limestone was first formed in most instances. Hollows were then made by some agency, whether by aqueous or atmospheric, or both agencies, it is not necessary here to inquire; but most of the cavities bear evidence of some erosive action, their sides being more like old valleys or watercourses than simple fissures. The next question to be considered is when were these hollows filled with the iron ore? No one who has investigated the subject would estimate their age so late as the date of the Magnesian Limestone, but some eminent geologists have supposed them to have been formed during the Lower Permian period, whilst others have adduced reasons for believing that they had been formed during the Carboniferous epoch. There is very little doubt but that large deposits of iron have been met with in both these formations, some of Carboniferous and others of Permian age, but our present enquiry is the geological age of the hæmatite iron ores of Furness.

'In two memoirs printed in the Transactions of this Society, namely, in Volumes VIII. and XII. (second series), he had described deposits of hæmatite iron ore, and given his reasons for believing that they had their origin during the Carboniferous epoch, and were deposited in erosions of the Mountain Limestone in some instances, or in others interstratified in regular beds in the Lower Coal Measures like seams of coal. Long after his papers had been printed, Sir R. I. Murchison, Bart., F.R.S., and Professor Harkness, F.R.S., in a Memoir on the Permian Rocks of the North West of England, printed in Volume XX. of the Quarterly Journal of the Geological Society, at page 152, say, "The mode in which that valuable ore of iron *hæmatite* is found deposited in pre-existing cavities of the carboniferous formation, and sealed up by Crab Rock (Permian breccia) is a matter of great geological interest. Joints, fissures, and caverns were doubtless formed in the older rocks antecedent to the deposition of the Permian strata, and in these the ores of iron, so widely diffused throughout the Permian rocks, have in this portion of the north-western region assumed the character of hæmatite. This circumstance justifies the inference that these hæmatite ores are the result of an agency which ushered in the Permian epoch. The earlier rocks of both England and Scotland are strongly impregnated with iron, their composition consisting principally of silica and an oxide of this metal. This latter substance originated from the same source which, during the commencement of the deposition of the Penrith sandstones, filled up the fissures in the Carboniferous Limestone. This conclusion is applicable not only to the Ulverstone district but also to that of Cleator, south-east of Whitehaven, where valuable deposits of hæmatite are also obtained from the cavities and fissures in the Carboniferous Limestone, which at one time was here covered over by an extension of those Permian breccias and sandstones, now forming an escarpment a short distance west of Cleator Moor. This inference concerning the Permian age of the hæmatite had been arrived at by Professor Phillips, who, in a communication to the British Association, of which a short abstract was published (Report of Brit. Ass. 1858, Trans. Sects. p. 106), illustrated his opinions by maps and drawings."

The authors of the paper illustrated their views in a section from Park to Furness Abbey, shown in a woodcut exhibiting two vertical fissures in the limestone rock, filled with hæmatite and covered up with breccia or crab rock. Now this undoubtedly shows that the limestone rock was first formed, then that the fissures or erosions were made and subsequently filled with hæmatite. But at what time the latter process took place, whether during the Carboniferous or Permian epoch no evidence is adduced by the authors further than that the beds of hæmatite are there covered up by Permian breccia, and some of the latter has been introduced into the fissures. It has been generally supposed by geologists that at the close of the Carboniferous epoch great convulsions took place in the earth's crust, and large quantities of iron were ejected from its interior. Now, the three authors previously named are recognised authorities on the age of hæmatite iron ores, and their opinions deserve great attention. When his papers were written, conclusive evidence could only be given of the age of the hæmatite iron of Ipstones, in Staffordshire, which was clearly interstratified in the Lower Coal Measures between the Rough Rock or Upper Millstone of Professor Phillips and the Geological Survey and the Gannister coal. Some years since, Mr. Bolton, of Swarthmoor, near Ulverstone, showed him, amongst other fossils, a beautiful specimen of *Sigillaria vascularis*, exhibiting both its external characters and internal structure, quite as perfect in every respect as the specimens found in the Gannister or Hard coal at Halifax or the Bullion seam of Burnley, all converted into hæmatite iron. At that time no doubt existed in his mind of its having come from one of the Furness iron mines, but Mr. Bolton could give him no proof of the exact locality where it was found. At a meeting of the Manchester Geological Society, held on the 30th May, 1866, he gave his opinion of the value of this fossil as indicating the carboniferous age of the hæmatite iron ores of Furness. These specimens have been kindly lent him by Mr. Swainson, of Newland, their present owner, and were exhibited on the table for examination. With them were some other interesting specimens from the cabinet of Miss E. Hodgson, of Ulverstone, a lady well known for her acquaintance with the geology of Furness, and who procured the specimens now exhibited from the Water Blain Mines, west of the Duddon. They do not exhibit their external characters so well as Mr. Swainson's specimens do; but one is a *Stigmaria*, the root of *Sigillaria*, and another a *Lepidodendron*, two common coal plants, which indicate the carboniferous age of the deposit in which they were found as clearly as any fossil organic remains can do. There is no doubt about the locality of Miss Hodgson's specimens. They came from the Water Blain Mines, and plenty more may be obtained from the same place. They are all converted into good hæmatite iron, that substance having metallized them in a similar way, as we find plants in the Coal Measures converted into the carbonate of the protoxide or the bisulphide of iron or carbonate of lime.

It was not his intention here to attempt to give any explanation of the process by which the original ligneous structure of the plant has been removed and replaced by per-oxide of iron, that is a subject better fitted for the chemist than the geologist.

Now the discovery of common coal plants not only embedded in but actually formed of hæmatite iron, surely indicates the carboniferous age of the deposits in which they are now found as clearly as it well can be, for the plants must have been floated in with the water which brought the iron, or else they must have fallen into the cavities when such were open at the top, and the iron was in a soft state. Of course, the occurrence of such fossil plants as *Sigillaria*, *Lepidodendron* and *Calamitis* occurring as they do in beds, from the lowest to the highest carboneferous strata, would give little evidence of any particular part of that epoch, but the *Sigillaria vascularis* so far as yet known is confined to the lower coal-field, not far in geological position from the Ipstones hæmatite previously alluded to, and the valuable clay band ironstones now wrought at Hazlehead, west of Penistone.

The deposits of hæmatite in Furness and Cumberland, found in hollows of the Carboniferous Limestone, and covered up by Till, or "Pinel," as it is locally termed, or more rarely by Permian breccia, are so much alike in all their characters that if the origin and age of one of them are clearly proved those of the rest must follow almost as a necessary consequence.

W. Brockbank, F.G.S., agreed with Mr. Binney, that the fossils now exhibited were valuable illustrations of the age of the deposits of hæmatite ore in the Furness district; but they did not afford any evidence of their origin, and were merely brought there with the other alluvium, and deposited with the iron ore, in the cavities of the limestone. The Cumberland district afforded better opportunities for studying the origin of these ores. True veins of hæmatite occur in the older slates, porphyries and syenites of the Lake district. Red Pike derives its name from the presence of veins of this ore, which gives the mountain its ruddy aspect; and iron appears to have been smelted near it, in the time of the Romans. On Black Comb, Dent Fell, and in the valleys above Ravenglass, veins of hæmatite occur in the older rocks, and are worked to a small extent, and the ore carted down to the railway. The richest deposits however occur in those places where clay slate is overlaid with other rocks, and principally in the carboniferous limestone.

In the Cleator district a narrow band of limestone, resting upon the clay slate, curves around the flank of Dent Fell, and in this narrow zone most of the iron ore mines of the Whitehaven district occur. The ore is found generally in large masses or pockets, in the body of the limestone, sometimes in contact with the clay slate, and at others filling cavities and clefts to the surface, and covered by alluvium. The limestone is at places overlaid by whirlstone or millstone grit, and the ore occurs both above and below this rock. In one remarkable mine the ore fills a large cavity in the limestone, and is covered by whirlstone, which forms a perfectly even roof over a large area. Where the hæmatite occurs near the clay slate, it is generally hard and crystalline, and it is much softer and less siliceous where it appears to have been deposited in the limestone, or by alluvium, causing a classification in this district into hard and soft ores, whereas in the Furness district they are generally of the soft kind.

In considering the origin of these ores, we must go back to the time when the limestone and associated rocks of the Carboniferous period had been formed at the bottom of the sea, and were raised therefrom by some vast force of upheaval which would naturally cause great denudation and break up the rock into clefts and fissures. This would appear to have been a time of great volcanic or at any rate of metalliferous activity, and the two mountains of Dent Fell and Black Comb bear evidences of having been very active centres of this action, and their upheaval may very possibly have been connected with the eruption of the hæmatite ores. It is therefore probable that during the early part of the Carboniferous era, the hæmatite ores were formed by these agencies, and by the denudations, which were vast and probably frequent, were partly removed and deposited in clefts and cavities wherever they were carried. The fossils produced by Mr. Binney point also to this period in the Furness district, as that in which the ores were carried into the cavities they now fill. The two mountains of Dent Fell and Black Comb appear to stand as a line of hæmatite ore mines, the country both between them and on their line north and southwards being dotted over with mines, and being most productive in their immediate vicinities. At Hodbarrow, immediately at the foot of Black Comb, occurs one of the richest mines yet discovered. It lies in a cleft of limestone, and appears to be more like a lode or vein than a deposit, and is immediately upon the shore of the Duddon estuary. This vein has been found on the south shore of the estuary just opposite, and it is fully believed that it is continuous through the sea. The richest mines of the Furness district are also immediately to the south of this point. The principal mines of Furness are deposits, as shewn by Mr. Binney, and the evidence of the Cleator district would point to Black Comb as a very possible source for their origin.

In the Cleator district the Carboniferous strata are at places overlaid unconformably by the Permian of St. Bees-Head, and in the lower red sandstone, underlying the magnesian limestone and conglomerate, are found many fossil plants, which are generally coated with red ore instead of carbonaceous matter. The conglomerates of St. Bees and Rougham Point also contain pebbles of hæmatite; both facts pointing again to denudation of hæmatite ore during the Permian era.

## MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 2nd, 1867.

A. G. LATHAM, ESQ., in the Chair.

Mr. DANCER sent for exhibition a Revolving Platform for the Microscope, similar to one shown by Mr. HEPWORTH some years ago at a meeting of the Section; the one now exhibited has been patented in London. He also showed a Paper Slide Cabinet, invented by Mr. PIPER, of London.

"On Plants appearing in successive years on land prepared for Plantations, in Cheshire," by H. A. HURST, Esq., and GEORGE CARTER, Esq,

The struggle for existence between man and his fellow-man hourly going on in all populous countries has long been the theme of the historian, and forms the subject matter of the science of political economy. It is, however, but recently that Mr. Darwin, in his great work on the origin of species, has shown that this same struggle is proceeding with perhaps greater intensity in the animal and vegetable kingdom. With greater intensity because moral force, which mitigates the severity of the conflict among the human race, is, as far as we know, wanting in purely animal and vegetable organisms. Man would recoil from destroying an aged pauper merely because he was useless; but a tree must necessarily destroy most plants growing under its shade. It has no power to do otherwise. Still, the truth of this theory is only partially admitted and its full effect hardly ever realised even by scientific men.

We attribute this, in a great measure, to the recent date of its promulgation and the comparatively few facts bearing on the subject which have been hitherto accumulated. Like Newton's great ideas, it will be left to the future patient student of the facts of natural history to discover the many proofs existing of its accuracy. As the discovery of Neptune by Adams and Le Verrier demonstrated the theory of gravitation, so probably at some future day will the truth of Darwin's assertion of the antagonism, as opposed to the so-called peace, existing in nature be proved by the prediction of some fact in botany of similar importance. In the meantime, it is the duty of all lovers of natural history to go on quietly accumulating facts bearing on the subject; and we therefore submit to your consideration the following observations, not as being in themselves of any value, but as affording an instance of this great struggle for existence going on in the vegetable world observed under unusually favourable conditions.

In Tatton Park, Knutsford, it has been the practice for the last few years to dig up, enclose, and plant with young trees each December, about an acre of land.

The size of the trees planted is so small that the effect on the vegetation around may, at least for some years, we think, be neglected without any error of magnitude being introduced into our results. Hence we have plots of land placed before us at intervals of one, two, three years in a state of nature—the disturbance of deer, cattle, &c., and especially man, carefully guarded against. These appear to be favourable circumstances for the discovery, if not of the indigenous plants, at all events of those which would eventually obtain possession of the land of the district, were it left in a state of nature.

We have not thought it necessary to go farther back than four years, because at that period the question appears to be decided, and the trees artificially planted had then acquired a height which would affect the conditions of the problem.

The plots were examined at two periods—January and the end of July, 1867.

The site is on what is called in the Ordnance Geological Survey, the Upper (Keuper) Red Marl of the New Red

Sandstone, and has an easterly aspect. The ground is dug two spades deep, and is a light sandy soil.

The young trees planted in each plot comprise Larch, Spruce, Scotch Fir, Oak and a few Sycamore.

No. 1. *Cleared January,* 1867.

19. Ranunculus acris, }
20. „ repens, } few plants.
21. „ bulbosus, }
63. Capsella Bursa Pastoris.
136. Viola arvensis.
218. Hypericum humifusum.
267. Trifolium repens.
332. Potentilla Tormentilla.
588. Taraxacum officinale.
643. Senecio vulgaris.
671. Achillea Millefolium.
800. Anagallis vulgaris, red flower, in great abundance.
914. Chenopodium album, one plant.
934. Polygonum lapathifolium.
939. „ aviculare.
941. „ convolvulus.
952. Rumex Acetosella.
1162. Juncus bufonius.
1186. Scirpus setaceus, one plant.
1313. Holcus lanatus, } tufts here
1314. „ mollis, } and there.

21 species. The ground not one eighth covered.

No. 2. *Cleared January,* 1866.

88. Arabis Thaliana.
136. Viola arvensis.
168. Sagina procumbens.
185. Stellaria media.
189. „ uliginosa.
193. Cerastium triviale.
218. Hypericum humifusum.
267. Trifolium repens, more numerous than in No. 1.
282. Trifolium minus.
283. Lotus corniculatus.
291. Ornithopus perpusillus.
297. Vicia Cracca.
332. Tormentilla potentilla.
517. Galium saxatile.
588. Taraxacum officinale.
671. Achillea Millefolium.
675. Campanula rotundifolia.
756. Veronica officinalis.
778. Digitalis purpurea.
844. Prunella vulgaris.
952. Rumex Acetosella, in large quantity, disputing the ground with the Holci.
1251. Carex prœcox, single plant
1271. Anthoxanthum odoratum, scarce.
1291. Agrostis vulgaris.
1313. Holcus lanatus, } occupying equal ground
1314. „ mollis, } with Rumex acetosella.
1528. Poa annua.
1362. Lolium perenne, scarce.

28 species. Holcus lanatus, mollis and Rumex acetosella, the predominant plants, struggling together for supremacy.

### No. 3. *Cleared January*, 1865

In 1866 Galium saxatile was a very prominent plant.

21. Ranunculus bulbosus.
283. Lotus corniculatus.
332. Potentilla Tormentilla.
340. Rubus fruticosus.
517. Galium saxatile.
671. Achillea Millefolium.
750. Veronica serpyllifolia.
756. Veronica officinalis.
952. Rumex acetosella, struggling for existence against the Holci.
1151. Juncus effusus.
1313. Holcus lanatus.
1314. „ mollis.

12 species. Here the Holci are acquiring possession of the ground to the exclusion of other species. R. acetosella alone making a feeble struggle against them.

### No. 4. *Cleared January*, 1864.

21. Ranunculus bulbosus.
283. Lotus corniculatus.
332. Potentilla Tormentilla.
339. Rubus Idœus.
340. „ fruticosus.
588. Taraxacum officinale.
778. Digitalis purpurea.
944. Rumex crispus.
952. „ Acetosella.
1151. Juncus effusus.
1291. Agrostis vulgaris.
1313. Holcus lanatus.
1314. „ mollis.
1339. Dactylis glomerata.

14 species. Here Holcus lanatus and mollis cover more than seven-eighths of the ground, the next plant in number being Rumex Acetosella, the specimens of which are poor and stunted, the species evidently unequal to struggle with the Holci.

The total absence of Umbelliferæ in all these plots is curious, especially as the biennial and perennial plants, Rubus fruticosus and Digitalis purpurea appear in the last plot. Perhaps the Umbelliferæ require the presence of shade.

From the above lists of plants, it follows that the first occupier of the ground in mass is Rumex Acetosella, and that it continues the struggle for two years with Holcus mollis and lanatus, which two species or varieties finally usurp the ground, destroying all competitors with the exception of a few single representatives chiefly of biennial plants.

The absence of any species of Fumaria and Papaver so common, nay almost universal, in similar situations, apparently points to the agency of man in distributing these plants, as it is the only condition wanting.

The nomenclature here adopted is that of the London Catalogue of British Plants of 1867, printed by Robert Hardwicke.

In our remarks we have considered the whole four plots as if they were one observed at different periods; but it is obvious that this is not absolutely correct, but that subsequent investigations will be required to confirm our conclusions.

Mr. J. SIDEBOTHAM wrote, stating that Polygonum convolvulus was the only plant which grew from soil obtained from under the foundations of a wall built some thirty years ago at Bowdon.

## Ordinary Meeting, December 24th, 1867.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

"Memorandum on the Evaluation of Integrals," by the Hon. Chief Justice COCKLE, F.R.S., &c., of Queensland. Communicated by the Rev. Professor Harley, F.R.S.

The processes indicated in my paper "On the Conversion of Integrals"* admit of direct application to certain results which Boole attained by a contemplation of the researches of Mr. Harley† and of Mr. Cayley. The object of this memorandum is to give a more general form and perhaps a more convenient expression to a portion of the processes of that paper. Thus we may generalise and restate (7) by saying that if

$$\mathrm{A}v\frac{d^3\phi}{dvdx^2}+a\frac{d^2\phi}{dx^2}+\beta v\frac{d^2\phi}{dvdx}+\gamma\frac{d\phi}{dx}+\epsilon v\frac{d\phi}{dx}+\zeta\phi$$

wherein A, $a$, $\beta$, . . $\zeta$ are functions of $x$ only and free from $v$, be, when $v$ is treated as the only independent variable, a perfect differential coefficient with respect to $v$ of a function $f_2$, then $\int\phi dv$, which for the sake of brevity we may replace by $y$, satisfies the linear differential equation of the second order

$$(a-\mathrm{A})\frac{d^2y}{dx}+(\gamma-\beta)\frac{dy}{dx}+(\zeta-e)y+\mathrm{A}v\frac{d^2\phi}{dx^2}+\beta v\frac{d\phi}{dx}+ev\phi=f_2+\mathrm{X}\ldots(1)$$

where $f_2$ is obtained, where obtainable, by properly assigning the five ratios of the six indeterminates $A, a, \beta, .. \zeta$ and wherein $X$ is a function of $x$ only which vanishes when the integration with respect to $v$ is definite. If we put $A = a$ then (1) becomes a linear differential equation of the first order and we have still four disposable ratios.

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* *Phil. Mag.* Suppl., July, 1867, p. 537. Art. 9 may be expunged as involving an error of computation, and Art. 10 is susceptible of a perhaps more general expression.

† Mr. Harley and I have since been conducted independently to a generalisation of one of Boole's results, which consists in applying Boole's process to forms of trinomial equations other than that discussed by him. I think it right to say that Mr. Harley's communication to me on the subject included his demonstration of his generalization, mine to him a verification (or perhaps two) only.

*Oakwal*, *near Brisbane*, *Queensland*,
*Australia*, *October* 17, 1867.

"On some Points in the Restoration of Euclid's Porisms," by T. T. WILKINSON, F.R.A.S., Corresponding Member of the Society.

M. Chasles, in his *Aperçu Historique*, p. 282, says that: "Diophante dans ses *Questiones Arithmetiques*, a plusieurs fois employè le mot Porisme,* pour désigner certain propositions concernant la théorie des nombres, sur lesquelles il appuie ses démonstrations, et qui formaient probablement un ouvrage qui ne nous est pas parvenu. Voir, par exemple, les Propositions 3, 5, et 19 du Livre V." This very valuable work was published at Brussels in 1837. The same Author in his restoration of *Les Trois Livres de Porismes D'Euclide*, published at Paris in 1860, gives an analysis of these Porisms in pp. 47—51, and adds to their

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*"Le Porisme est une proposition dans laquelle on énonce une vérité, en affirmant qu'on peut toujours trouver certaines choses qui la complètent." —(Chasles, *Porismes* p. 54.)

This *new* definition regards Porisms as *incomplete Theorems*.—M. Breton (*Porismes*, 1866), regards them rat heras *incomplete Problems*.

"A Porism proposes to demonstrate that one or more things may be found, between which and innumerable other objects assumed after some given law a certain specified relation is to be shewn to exist."—(Leslie, *Geom. Anal.*)

"A Porism is a proposition affirming the possibility of finding such conditions as will render a certain problem indeterminate, or capable of innumerable solutions."—(Playfair, *Edin. Trans.*)

"Porisma est propositio in qua proponitur demonstrare rem aliquam, vel plures datas esse, cui, vel quibus, ut et cuilibet ex rebus innumeris, non quidem datis, sed quæ ad ea quæ data sunt eandem habent rationem, convenire ostendendum est affectionem quandum communem in propositione descriptam."—(Simson, *Opera.*)

"Porisma vero, quod proponitur in porismum, hoc est in inventionem et investigationem propositi."—(Commandine, *Pappus*, Vol. II, p. 160.)

number Prop. 17 and 20, Book III; Prop. 20 and 44, Book IV; and lastly Prop. 12, Book VI.

In this *résumè* of the "Porismes cites par Diophante" he again refers to the lost treatise from which Diophantus is supposed to have quoted; and he here claims to have been the *first* who had fully understood the nature of these properties of numbers.

He there says:—"Diophante ne parle pas expressément des Porismes, comme Proclus, et n'a pas à les définir. Mais il cite dan ses *Questions Arithmetiques* sous le titre de *Porismes*, des propositions extraites d'un ouvrage, apparemment d'un *Recueil de Porismes*, qui ne nous est pas parvenu. Ces propositions, auxquelles je crois que l'on n'avait jamais fait attention, du moins à titre de *Porismes* dans le sens d'Euclide, *avant que nous les eussions signalées* dans l'*Aperçu Historique*, se rapportent aux propriétés des nombres; et ce qui a de l'intérét ici, c'est que, sons le nom de *Porismes*, elles ont dans leurs énoncés la forme des *Données*, la même que nous attribuons aux *Porismes*."

This point is of some importance in the history of the restoration of the Porisms; and hence M. Chasles's claim to this discovery cannot be allowed. I possess a copy of a letter (the original is in the *Chetham Library*), from the Rev. Charles Wildbore, sometime editor of the *Gentleman's Mathematical Diary*, to the Rev. John Lawson, then Rector of Swanscombe, Kent, and brother of the Rev. Charles Lawson, Head Master of the Grammar School, Manchester, which announces the same discovery. The essential portions of this letter, and of the one which gave rise to it, will render this sufficiently clear. Mr. Wildbore was a geometer of a very high order,* and he had been engaged on Porisms before it became known that Dr. Simson had restored them. Mr. Lawson in a letter to him, dated "August 10th, 1775," says "I have very important

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*See his investigation of "Lawson's Theorems," in the Manchester *Memoirs* Vol. II. N. S. pp. 414—452.

news to tell you, which I should much rejoice at, were it not for one consideration, which is, that I am afraid it will in some measure prevent your exaltation to honour. Dr. Simson has restored Euclid's Porisms. They are printed off, and will shortly be published. This intelligence I have from the best authority, viz., the Earl of Stanhope, at whose expense they are printed." Mr. Wildbore replied to this, "August 18th, 1775," and his letter contains the following passages:—"I much rejoice to hear that you have made an acquaintance with that learned and judicious nobleman, the Earl of Stanhope; and his communicating to you the contents of Dr. Simson's posthumous works is the reason of my being in such haste to answer your last agreeable favour. In contemplating what these Porisms of Euclid might be, and to what uses they were applied, I have hit upon something of which I have not met with the least hint in any author What I mean is this: *that the Porisms quoted by Diophantus, and the other remarkable Propositions, assumed without proof by that author*, which do so much honour to Antiquity, *are no other than the Porisms of Euclid*, and that this kind of Porisms composed Euclid's *second* Book But, as this may require something more than bare assertion, I shall here show the method by which some of the most remarkable flow from the two Lemmata* which are the 148th and 149th Propositions of the *seventh* Book of Pappus· From the first-named Lemma the following is evident. *In any right-angled triangle the double rectangle contained under the sum of the hypothenuse and greater leg, and the difference of the hypothenuse and cathetus* [the perpendicular] *is equal to the square of the sum of the said difference and the greater leg.*" He then deduces "the method which Diophantus makes use of in the 22nd Proposition of his third Book, which many of the moderns have been very illustrious about, but for elegance am afraid they must all yield to the ancients." Another corollary gives the "property made use of by Diophantus, in Question 24 Book V.;" and by generalising his method he deduces "the Porism quoted by Diophantus in the *third question of the fifth Book of Arithmetic.*"

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* 1. If upon a right line AB there be taken two points C, D, such that $2AB \cdot CD = CB^2$; then $AD^2 = AC^2 + BD^2$.—(*Pappus*, prop. 148.)

2. If $BA \cdot BC = BD^2$; then (*a*), $(AD+DC)BD = DA \cdot DC$;
(*b*), $(AD+DC)CB = DC^2$; (*c*), $(AD+DC)AB = AD^2$.
(*Pappus*, prop. 149.)

Mr. Wildbore now takes up the second of the two Lemmas previously noticed, and, by an elegant and somewhat elaborate process, he deduces "*the very remarkable Porism* quoted by Diophantus* as the 5th question of the Fifth Book." He further adds that—"there are many other curious properties flowing from the squares here deduced. Thus in the second square, if DC=1, we have question 33, Book II., of Diophantus; and if there be *any* number which is equal to the product of two numbers differing by unity; and if to this number unity be added, the square of the sum may be thus divided into *three* squares. Hence the sum of three squares may be easily divided into three other squares."

Towards the close of the letter Mr. Wildbore gives a *geometrical* solution to the following case of the general *Problem of Inclinations.* "*between two given semicircles, having their bases in the same right line, to insert a right line of a given length which shall verge to an angle of one of the semicircles.*" He says, "it is as curious as any we know that were done by the ancients, and having some time since bestowed pains to solve it without Dr. Horsley's *algebraical* Lemmas, I will here subjoin the solution." He concludes by stating that should Mr. Lawson "communicate the preceding to his lordship, as I am in hopes you will, please to make my most respectful compliments to him, and tell him that I shall wait with patience till I can be favoured with a sight of what Dr. Simson has done."

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* "Etant donnés deux nombres carrés cousécutifs, on peut trouver un troisième nombre ègal au double de la somme de ces deux premiers plus 2, tel, que le produit de deux de ces nombres augmenté, soit de la somme des deux mêmes, soit du troisième nombre, fasse un carré."—Chasles, *Porismes*, p. 49.)

If $x$, $y$, $z$ be the numbers—then $xy+(x+y)$; $xz+(x+z)$; $yz(y+z)$; $x+yz$; $y+xz$; $z+xy$,—are all to be squares under the conditions proposed. Quest. III. requires that $x+a$; $y+a$; and also $xy+a$, be square numbers.

It thus appears that the Rev. Charles Wildbore had discovered that these properties of numbers partook of the nature of Porisms more than sixty years before M. Chasles announced the same fact in his *Aperçu*. I am of opinion that the rev. gentleman was somewhat in error as to the contents of Euclid's Second Book; and his opinions on some other points mooted in this long letter will now require modification. There, however, appears to be sufficient merit in this correspondence to warrant the publication of these extracts, at a time when M. Michel Chasles is astonishing the scientific world by his connection with the Newton and Pascal forgeries.

"On the Examination of Water for Organic Matter," by Dr. R. ANGUS SMITH, F.R.S.

The Author repeated his opinion that the mere expression of organic matter had no such meaning as would allow chemists to measure the impurity of water by its amount. He went more fully into the division of the organic matter into various portions, some acting as unwholesome agents, others being entirely innocent. He said he was glad to find that other chemists were also attending to the quality as well as the quantity of the organic matter, and he insisted also on the condition of the matter being observed. He discussed the methods of Professors Frankland and Wanklyn, considered, however, that they did not supersede his own methods, which made a greater number of subdivisions. He explained the mode in which the organic matter is entirely removed from water, leaving frequently more of its elements behind, unless we include amongst them the inorganic bodies with which they were combined. The body which remains is chiefly common salt, which cannot be removed, and by which more than any other substance animal matter is to be detected in water under certain precautions. He also showed the importance of finding the amount of atmospheric oxygen in water, and its meaning; but as the paper was not concluded the notice is here left incomplete.

## Ordinary Meeting, January 7th, 1868.

**Edward Schunck**, Ph.D., F.R.S., &c., President, in the Chair.

"Variable Spot on the Moon's Surface," by W. R. **Birt**, F.R.A.S., communicated by J. **Baxendell**, F.R.A.S.

The interest attaching to the phenomena presented by the lunar spot Linné is my apology for communicating a few observations on another spot which exhibits similar phenomena. It will be seen that both spots manifest phenomena which appear to be referrable to the presence of a *covering* by which the craters are at times *concealed.* We are not cognisant of any agency such as libration, angle of illumination, or variation of distance which affects the forms and appearances of lunar objects, being capable of rendering a crater *invisible* while its place is occupied by a white *cloud-like* spot of light; nor are any of these agencies capable of rendering an object on the moon's surface *indistinct* while others in its immediate neighbourhood are exceedingly sharp and well defined. With the hope of directing the attention of astronomers to this curious class of lunar objects, may I be permitted to lay the following observations before the Society? They have been made principally by the Rev. W. O. Williams of Pwllheli, who has undertaken the examination of a zone on the moon's surface of 2° of latitude, viz. from 4° to 6° south.

The spot in question is marked IV A*a* 17, IV A*ζ* 39 on the areas of the British Association Lunar Map IV A*a* and IV A*ζ*, and is situated in 2° W. long., and 5° S. lat. It is also situated on the S.W. side of the ridge forming the N.E. boundary of Hipparchus, and has been described as "a bright spot S.S.W. of IV A*a* 7 (Beer and Mädler's Hipparchus F). Its diameter is 5″·94 and magnitude 0°·37, the diameter of Dionysius being regarded as unity. On De La Rue's photograph 1858, February 22, it appears as a spot of about 4° of brightness. It is not so bright as Linné, which is about 5°. In this photograph it is seen to stand upon the east edge of a large *depression* running nearly S. by W.—N. by E. This edge, which forms a low ridge, connects the mountainous boundary of Hipparchus with the mountain IV A*ζ* 37. IV A*a* 7, a bright spot smaller than IV A*a* 17, IV A*ζ* 39, stands upon the west edge of this depression, which also meets the mountain IV A*ζ* 37.

On Rutherford's photograph 1865, March 6, this spot appears brighter than in De La Rue's, viz. 5°. Linné in this photograph is 6°. The observations that have been made of this spot are as under,

| Year. | Date. | | | Authority. | | Character. | Brightness. |
|---|---|---|---|---|---|---|---|
| 1858 | Feb. | 22 | ........ | De La Rue, | Ph. | A bright spot. | 4° |
| 1865 | Mar. | 6 | ........ | Rutherford, | Ph. | A bright spot. | 5° |
| 1867 | May | 11 | 8½...... | Birt, | Obs. | A shallow crater. | |
| 1867 | Oct. | 7 | 8½ to 10. | Williams. | ,, | A very bright spot. | |
| 1867 | ,, | 17 | ........ | Ingall, | ,, | A faint shallow crater. | |
| 1867 | ,, | 17 | 18½ .... | Ingall, | ,, | Drawn as a crater. | |
| 1867 | ,, | 17 | 13 to 15. | Williams, | ,, | A very conspicuous crater.* | |
| 1867 | ,, | 18 | 17 to 19. | Williams, | ,, | Crater very conspicuous, with a small central cone casting a shadow. | |
| 1867 | Nov. | 5 | 9 to 10. | Williams, | ,, | Very bright, a streak of interior shadow on the west. | 7° |
| 1867 | ,, | 6 | 8 to 10. | Williams, | ,, | A bright patch of light, streak of shadow scarcely discernible. | 6° |
| 1867 | ,, | 15 | 18 to 20. | Williams, | ,, | Very bright.* | 10° |
| 1867 | Dec. | 5 | 6 to 8. | Williams, | ,, | A whitish spot, no trace of a crater. | 5° |
| 1867 | ,, | 6 | 9 to 10. | Williams, | ,, | A whitish spot, no crater. | 5° |

* On these occasions Mr. Williams saw a small bright point to the E., which he considered to be the *highest* point of the ridge.

Mr. **Baxendell** stated that on the night of the 3rd instant he had an opportunity of examining the spot referred to by Mr. Birt with Mr. Gladstone's equatorially mounted achromatic of 7¼ inches aperture, using powers from 60 to 250. It was then a well-marked though shallow crater, having a diameter about three-fourths of that of Beer and Mädler's Hipparchus F. The shadow of the western wall was very conspicuous on the floor of the crater.

Mr. **Baxendell** also read the following extract of a letter dated November 27th, 1867, which he had received from Mr. C. Ragoonatha Chary, the first native assistant at the Royal Observatory, Madras: —

"I have prepared the necessary calculations connected with the total solar eclipse to take place in the Indian Peninsula on the 18th of August, 1868, and these, with appropriate description and remarks on the eclipse by N. R. Pogson, Esq., are now in the press and will be published in the leading Madras Almanac. In these calculations I find that a slide-rule constructed for trigonometrical purposes may most advantageously be used even in such intricate cases as the solar eclipse. It saves more than three-fourths of the time and labour; and having calculated independently with the slide-rule as well as by means of logarithms for several places, I found the difference rarely to amount to half a minute in time, which is no great matter in predicting for amateurs, and even for intending observers. Mr. Woolhouse's method is followed, I believe, in the Nautical Almanac. The skeleton forms of this method, which are printed in great detail for logarithmic calculations, may be greatly simplified and facilitated by the use of a slide-rule accurately divided. The one I used was not very accurately divided, and was only two feet in length.

Mr. E. W. **Binney**, F.R.S., F.G.S., said that in the *Geological Magazine* for August of last year Mr. D. Mackintosh, F.G.S., in a paper on Pholas Borings, Denudation, and Deposition in S.E. Devon, says, "The structure and marine denudation of the district between Torbay and Babbicombe Bay has been so ably unravelled by Mr. Pengelly as to leave any other observer comparatively little to say. Among the most important discoveries must be ranked that of lithodomous perforations in limestone rocks at considerable altitudes above the sea." In the September number of the same publication the Rev. William Gibbins describes some Pholas borings and says, "And here I ought to mention that Mr. Pengelly was the first person from whom I learnt the supposed origin of these peculiar marks or holes in rocks near the sea coast." At a meeting of this Society on the 15th October last Mr. R. D. Darbishire, B.A., F.G.S., in a communication described similar holes and markings observed in the rocks near Buxton and on the Great Orme's Head.

No doubt it has been generally considered by geologists that Mr. Pengelly, F.R.S., a most acute and accurate observer, was the first person to notice the traces of lithodomous mollusks on rock surfaces. But it is now clear that such appearances had been observed and given to the public a quarter of a century ago.

In a Sketch of Furness and Cartmel by Charles P. Jopling, published in 1843, in speaking of the geology of Stainton and Adgarley at page 40, the author says, "The beds of the Carboniferous Limestone are well developed and are sometimes of great thickness......On the green lie detached masses of rock as if formed,

Into rude shape by fire with roaring blast
  Impetuously let loose from central caves;
  Or fashioned by the turbulence of waves,
That when o'er highest hills the deluge pass'd.

That at some time they have been submerged by the waters of the ocean seems indubitable, for the cavities in which the lithophagæ burrowed are still fresh, and tell of other aspects and other times."

At page 77, the author again says, in describing the country:—"We will not pretend to trace it through the many changes, or attempt to describe it when the breakers laved the rock of Stainton and the lithophagæ burrowed in those immense blocks which are still left to attest the fact."

The Carboniferous Limestone alluded to by Mr. Jopling in Furness, Lancashire, is only a few miles from the shore of Morecambe Bay, and in the localities alluded to by him will be at an altitude of about two hundred feet above the present sea-level.

He was not competent to form an opinion as to the holes and hollows in rocks seen and described by the above-named observers, but probably such phenomena, even if clearly proved to have been the work of a mollusk, should be classed under three heads, namely:—1st, those in the bare rock; 2nd, those covered by sward and vegetable mould; and, 3rd, those covered by till or other drift. In all these cases the shape of the original hole or hollow made by the Pholas would no doubt be considerably modified by the different causes to which they had been subjected. In the first case, they would be subject to the action of the carbonic acid in the rain-water, and aqueous and atmospheric agencies. In the second case, not only to those effects, but to the decomposing action of organic acids arising from the decaying vegetable matter. And, in the last instance, to long-continued aqueous and atmospheric agencies before the rock was covered up by the drift deposits.

Great care ought to be taken before a hole in a rock should be attributed to a mollusk, especially in limestones, which so frequently exhibit on the surface markings of nearly every variety of form.

Dr. **Joule**, F.R.S., referring to recent magnetic disturbances, described the means he made use of for determining the absolute horizontal intensity. A current of electricity was transmitted successively through a single-coil galvanometer and his current meter, an instrument already described, consisting of a suspended flat coil of wire between two equal fixed coils. If the coils are infinitely close to one another, the current is given by the formula

$$C=\frac{k}{2l}\sqrt{gw}.$$

$k$ being the radius of a circle of equal area to one of the coils, $l$ the length of wire in one of the coils, and $w$ the weight required to balance the suspended coil. The current in the galvanometer is determined by the horizontal intensity and the tangent of deflection. Therefore, by combining the two instruments, we obtain the horizontal intensity, which is equal to the square root of the force of attraction in the current meter, divided by the tangent of deflection in the galvanometer, multiplied by a constant.

"On the Examination of Water for Organic Matter," Part II., by Dr. R. **Angus Smith**, F.R.S.

At present the conclusion only is given, as no abstract was prepared.

The following may be considered as a summary of the results required for sanitary purposes.

1. Quality of the organic matter, *i.e.* what is produced by standing under favourable circumstances for developing vegetable or other life?

2 and 3. Condition of the organic matter. Products of decomposition. Easily decomposed organic matter. These two can be estimated for sanitary purposes sufficiently by permanganate of potash.

4. Nitrates as remnants of organic matter.

5. Nitrites as remnants of organic matter.

6. Chlorides as indicating animal sources.

7. Oxygen as indicating activity of decomposition or destruction.

8. Total organic matter and ammonia, by weighing and other methods.

## PHYSICAL AND MATHEMATICAL SECTION.

December 31st, 1867.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

"Solar Radiation Observations, made at Eccles, near Manchester," by THOMAS MACKERETH, F.R.A.S., F.M.S.

Feeling considerable interest in the paper read by Mr. Baxendell, F.R.A.S., at a recent meeting of the Physical Section, and at a general meeting of the Society, on this subject, I have reduced five years' observations which I had made at Eccles with an ordinary black-bulb thermometer placed on the south side of the shade stand, four feet above the ground. The thermometer had been duly compared at the Kew Observatory. I was chiefly induced to make these reductions because the discussions of Mr. Baxendell on the subject were from observations made at Oxford by means of a thermometer placed somewhat similarly to mine, at least as mine was placed during the five years I have reduced.

Taking the differences between the mean maximum black-bulb reading in the sun, and the mean maximum reading in the shade, for each month, we find two maxima—one in May, and another in August; and two minima—one in June, and another in December.

In order to project the gradual rise and fall of solar radiation for each month, I have adopted the form of table presented by Mr. Vernon in his reductions of similar observations. Thus, if we take the differences between one month and the succeeding one, we find the following values:—

| | ° |
|---|---|
| January to February | + 2·80 |
| February to March | + 2·98 |
| March to April | + 3·16 |
| April to May | + 1·14 |
| May to June | – 0·80 |
| June to July | + 0·38 |
| July to August | + 0·56 |
| August to September | – 1·48 |
| September to October | – 5·28 |
| October to November | – 2·26 |
| November to December | – 3·06 |
| December to January | + 1·86 |

The foregoing table shows a very regular though rapid increase of the amount of solar radiation from January till April, which is only slightly exceeded in May. We have then a slight decrease, and afterwards another maximum in August. Then follows a very rapid decrease to October, which continues, though not so rapidly, to the end of the year. This table shows that the observations at Eccles have a projection very similar to the one presented by Mr. Baxendell on page 38, vol. VII. of the "Proceedings;" and I have no doubt that if the observations had extended over a longer period, the similarity would have been more striking.

Below I present my monthly means as they have been reduced.

| Year. | January. | | | February. | | | March. | | |
|---|---|---|---|---|---|---|---|---|---|
| | Max. Bk Bulb in Sun. | Max. in Shade. | Diff. | Max. Bk. Bulb in Sun. | Max. in Shade. | Diff. | Max. Bk. Bulb in Sun. | Max. in Shade. | Diff. |
| | ° | ° | ° | ° | ° | ° | ° | ° | ° |
| 1862 | 48·0 | 42·5 | 5·5 | 52·7 | 45·4 | 7·3 | 55·1 | 46·7 | 8·4 |
| 1863 | 49·8 | 44·3 | 5·5 | 55·5 | 47·4 | 8·1 | 62·4 | 49·9 | 12·5 |
| 1864 | 44·9 | 40·0 | 4·9 | 50·5 | 40·4 | 10·1 | 57·8 | 45·6 | 12·2 |
| 1865 | 44·6 | 40·3 | 4·3 | 44·8 | 40·0 | 4·8 | 51·8 | 42·1 | 9·7 |
| 1866 | 49·9 | 46·7 | 3·2 | 52·1 | 45·0 | 7·1 | 54·4 | 44·9 | 9·5 |
| Means | 47·44 | 42·76 | 4.68 | 51·12 | 43·64 | 7·48 | 56·30 | 45·84 | 10·46 |

| | April. | | | May. | | | June. | | |
|---|---|---|---|---|---|---|---|---|---|
| 1862 | 66·9 | 53·3 | 13·6 | 77·5 | 61·6 | 15·9 | 73·2 | 60·8 | 12·4 |
| 1863 | 68·6 | 53.5 | 15·1 | 72·8 | 57·3 | 15·5 | 80·5 | 63·9 | 16·6 |
| 1864 | 69·1 | 54·9 | 14·2 | 80·8 | 62·4 | 18·4 | 78·8 | 62·6 | 16·2 |
| 1865 | 73·8 | 60·3 | 13·5 | 71·9 | 60·5 | 11·4 | 79·8 | 67·4 | 12·4 |
| 1866 | 65·4 | 53·7 | 11·7 | 69·8 | 57·2 | 12·6 | 79·0 | 66·8 | 12·2 |
| Means | 68·76 | 55·14 | 13·62 | 74·56 | 59·80 | 14·76 | 78·26 | 64·30 | 13·96 |

| | July. | | | August. | | | September. | | |
|---|---|---|---|---|---|---|---|---|---|
| 1862 | 78·0 | 63·4 | 14·6 | 79·1 | 64·5 | 14·6 | 75·0 | 60·7 | 14·3 |
| 1863 | 82·4 | 66·7 | 15·7 | 82·2 | 65·4 | 16·8 | 69·8 | 56·9 | 12·9 |
| 1864 | 84·1 | 67·8 | 16·3 | 79·5 | 64·4 | 15·1 | 76·3 | 61·8 | 14·5 |
| 1865 | 81·9 | 68·8 | 13·1 | 78·1 | 64·7 | 13·4 | 85·3 | 70·2 | 15·1 |
| 1866 | 78·0 | 66·0 | 12·0 | 78·4 | 63·8 | 14·6 | 69·8 | 59·5 | 10·3 |
| Means | 80.88 | 66·54 | 14·34 | 79·46 | 64·56 | 14·90 | 75·24 | 61·82 | 13·42 |

| | October. | | | November. | | | December. | | |
|---|---|---|---|---|---|---|---|---|---|
| 1862 | 65·3 | 54·7 | 10·6 | 51·8 | 42·4 | 9·4 | 51·8 | 47·5 | 4·3 |
| 1863 | 62·4 | 53·5 | 8·9 | 55·4 | 49·1 | 6·3 | 51·5 | 47·7 | 3·8 |
| 1864 | 62·2 | 54·2 | 8·0 | 53·1 | 47·4 | 5·7 | 45·0 | 42·6 | 2·4 |
| 1865 | 65·9 | 56·3 | 9·6 | 55·5 | 49·3 | 6·2 | 50·4 | 47·2 | 3·2 |
| 1866 | 60·9 | 57·3 | 3·6 | 51·5 | 49·7 | 1·8 | 47·5 | 47·1 | 0·4 |
| Means | 63·84 | 55·20 | 8·14 | 53·46 | 47·58 | 5·88 | 49·24 | 46·42 | 2·82 |

## MEAN MONTHLY VALUES.

| | Max. in Sun. | Max. in Shade. | Diff. |
|---|---|---|---|
| | ° | ° | ° |
| January ...... | 47·44 ......... | 42·76 ......... | 4·68 |
| February...... | 51·12 ......... | 43·64 ......... | 7·48 |
| March ......... | 56·30 ......... | 45·84 ......... | 10·46 |
| April ......... | 68·76 ......... | 55·14 ......... | 13·62 |
| May........... | 74·56 ......... | 59·80 ......... | 14·76 |
| June ......... | 78·26 ......... | 64.30 ......... | 13·96 |

| | Max. in Sun. | Max. in Shade. | Diff. |
|---|---|---|---|
| | ° | ° | ° |
| July | 80·88 | 66·54 | 14·34 |
| August | 79·46 | 64·56 | 14·90 |
| September | 75·24 | 61·82 | 13·42 |
| October | 63·34 | 55·20 | 8·14 |
| November | 53·46 | 47·58 | 5·88 |
| December | 49·24 | 46·42 | 2·82 |

## Ordinary Meeting, January 21st, 1868.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

"Notes on some of the Rarer Plants found near Llandudno," by Joseph Sidebotham, Esq.

North Wales is celebrated for its botanical treasures, and perhaps no portion of it more so than the Great Ormes Head and the country adjoining. In the early spring Gloddaeth Woods are one mass of flowers, some of them by no means common, and the rocks and slopes of the hills afford many of extreme rarity. The cowslip, a rare plant near Manchester, is in abundance, and the hybrid oxlip is in many places where primroses and cowslips are found near together. Cowslips can be seen for more than a month, from the first of May, on the southern slopes and warm fields, till near the middle of June, on the northern slopes of the Little Ormes Head.

I will now mention a few of the rarer species met with during last spring.

Cruciferæ.—*Malcomia maritima*, a species not truly wild, is however met with often on the Great Ormes Head. I have found it in many places all along the cliff walks and near the shore on Conway Bay. It is usually very small, the whole plant with flower not being more than a couple of inches in height.

*Cheiranthus cheiri* is found on most of the old castles and ruins in North Wales, and has evidently been there for a long time, being mentioned by various old writers. It is singular that it should so rarely be found on rocks, except in the immediate neighbourhood of some ruin. This would perhaps favour the idea that the species has been introduced at some very distant time.

*Thlaspi alpestre.* This is very plentiful at Llanrwst, the walls and rocks in many parts, being white with its flowers. This is the subspecies *occitanum* of *English Botany*; the other subspecies, *sylvestre,* I have not gathered, but from the examination of dried specimens of it from Teesdale and Clova, I cannot see a good reason for any division of the species.

*Brassica oleracea.* Found abundantly on the cliffs of the Great Ormes Head, and also on rocks in several places between Conway and Llandudno. It is of special interest as being the parent of the numerous varieties of cabbage, cauliflower, and brocoli, and is, I think, truly indigenous in these localities. I can myself speak to its abundance on the Great Ormes Head thirty years ago, when Llandudno consisted of a very few fishermen's cottages, and I am told it is mentioned as being there more than a hundred years ago.

*Helianthemum canum.* This is one of the most conspicuous plants on the slopes of the Great Ormes Head, in the early part of May, covering the ground with a beautiful pale yellow; about the last week in May it gives place to the other species, *H. vulgare,* the larger flowers of which make even a more beautiful show.

*Helianthemum guttatum.* This occurs on Holyhead mountain. I had previously gathered specimens in seed, and visited the place in May to see it in flower. I was a little early and only met with a few flowers fully out, and as the petals fall at once when touched, it was not easy to secure good specimens. I however brought a few plants in my case, and planted them in a pot, where they flowered outside my window at Llandudno. On the bleak slopes at Holyhead the entire plant in flower does not exceed 1½ inches in height, but in the flowerpot it reached 3 to 4 inches. I made a drawing of one of the specimens, which can be compared with the dried ones. The red spot at the base of the petals is very large; the figure in *English Botany* does not do it justice.

*Viola hirta.* Found in many places about Llandudno and Conway, abundant in Gloddaeth woods.

*Silene nutans.* Abundant on the rocks about Llandudno.

*Astragalus glycyphyllus.* This is by no means a common plant. It is found in various places about Gloddaeth, in the lanes and also in the wood, and in one or two places near the Little Ormes Head. I was too early in May to find it in flower.

*Hippocrepis comosa.* A very handsome plant, on the rocks of the Great Ormes Head, where it forms large masses of flowers in May.

*Cotoneaster vulgaris.* The Great Ormes Head is supposed to be the only locality for this species in Great Britain. I visited Llandudno in the year 1840 chiefly to gather it, and in the same place I again collected specimens in May, 1867. I know of five distinct places where it grows pretty plentifully, and, if not rooted out, it will continue for generations to gratify botanists and supply them with specimens. But I should like here, to protest against the destruction of wild plants, which, if continued as at present, would in a few years entirely eradicate all the rare species, and make some of those now common, very rare. This destruction is not caused by botanists: a true botanist is careful not to destroy a plant, and in case of a very rare one, will content himself with a small sprig, without disturbing the root.

Even a collector of plants who takes no such care, will not do much harm in a general way, because he will only take as many specimens of a plant as he can dry. The real destroyers of plants are those who collect them for the market for sale. In Llandudno, for instance, the market is supplied with ferns and rare plants in prodigious numbers: these are purchased by visitors, as just now it is the fashion to take roots of wild plants on returning home. It is clearly the interest of those persons who supply the markets to clear the neighbourhood of rare plants, so that the visitor who wants them must buy them in the market. A very few years ago *Asplenum adiantum-nigrum* was one of the very common ferns around Llandudno; last May I wished to show it to a friend, and could not find a single frond nearer than a lane at Gloddaeth. One day in May I saw in the market almost a cart load of roots of *Allosurus crispus*, and, what was more to be regretted, a number of roots of *Cotoneaster*. As long as these dealers find purchasers they will continue to root up the rare plants; but if visitors would only consider that the great charm of our wild plants is to see them growing in their native places, and not to take away roots purchased in the market, which will rarely live when they are removed, this destruction would soon cease, and the natural yearly increase of the plants would be sufficient to supply those who would go and gather for themselves.

The neighbourhood of Llandudno is very rich in COMPOSITÆ, but I was too early in May to find many of them in flower.

*Silybum Marianum* is found in many places, and seems quite at home near the summit of the Little Ormes Head.

*Serratula tinctoria, Cichorium Intybus, Helmintha echioides, Hypochœris maculata,* are conspicuous along with several of the rarer species of *Hieracium*. Perhaps the rarest of the Compositæ is

*Chrysocoma Linosyris,* which is found abundantly on the rocks above the town. The specimens now exhibited were gathered late in August, when it was in perfection. This is one of the very few places in the kingdom where it is to be found.

*Scrophularia vernalis.* This is one of the rarest of British plants, and is supposed by some to be a doubtful native. However this may be, it is quite naturalised in a lane near Gloddaeth, where it ornaments the hedge sides for half a mile; it is also found in another lane nearer Conway. It is an exceedingly beautiful plant, with its pale green leaves and lemon coloured flowers. I found on this plant three species of Coleoptera of the genus *Cionus.*

In the ditches on the plain grows *Hottonia palustris* in the greatest perfection, the leaves in many cases growing in dense tufts above the water, and the beautiful spikes of flowers in thousands.

On the southern slopes of the Great Ormes Head, *Scilla verna* begins to show its bright blue flowers the first week in May, and in the middle of the month many places are completely covered with blue, and it may still be found in abundance in June on the northern and eastern sides of the hill; it even occurs in profusion down on the grass above the sea, and in the open places in Gloddaeth woods, where it grows to the height of six or eight inches.

The genus *Statice* is one about which botanists are not agreed as to the species, some authorities making many into species which others call only varieties. At Llandudno are found two very different forms of the species now called *binervosa;* the smaller form grows in abundance on Conway beach, and the larger one on rocks behind the baths. Although so different in appearance, there is no good ground for considering them in any way distinct species.

Among the grasses, *Phleum arenarium* and *Sclerochloa rigida* and *loliacea* are very common on and near the shore at Llandudno. The rarest species is *Knappia agrostidea*, found in Anglesey on the borders of Llyn-Curon, which is the only known locality for it in Great Britain. It is a very small species, and, flowering as early as from the end of March to May, it may have been overlooked in other places; it is extremely abundant on the grassy hillocks on the borders of the lake; the largest specimens I found were about two inches in height, the general size being about one inch.

In the foregoing notes I have only mentioned a few of the plants I met with in North Wales last spring. Specimens of many others are before you, and those who visit Llandudno a month later than I did will find very many more species, especially in the families *Compositæ* and *Cyperaceæ*.

"Some Remarks on Crystals containing Fluid," by J. B. Dancer, F.R.A.S.

The Author gave a brief history of the discovery of fluids in crystals, including Sir H. Davy's chemical experiments on the fluids and gases obtained from the cavities in quartz crystals; Sir David Brewster's discovery of the pressure cavities in the diamond, ruby, emerald, amethyst, chrysoberyl, &c.; the existence of minute crystals in these cavities and the two new and remarkable fluids, which are immiscible, but sometimes found together in the same cavity—one a liquid hydro-carbon, named Brewstoline, the other Cryptoline; his experiments and examination of artificial crystals deposited from aqueous solutions; his examination of the Koh-i-noor diamond and others in the East India Company's museum; and the geological speculations to which these discoveries gave rise. Mr. Dancer mentioned the experiments of his late father and others in producing artificial gems by intense heat, and stated that his own attention was drawn to this subject some twenty-four years since, by Sir David Brewster presenting him with a specimen of topaz containing fluid. Since that time he had examined a large number of crystals, of various kinds, from the collections of friends; and had found fluid in quartz from South America, Norway, the Alps, Ireland, Snowdon, and the Isle of Man; and in fluor spar from Derbyshire; this latter specimen contained a considerable quantity of fluid, which burst the crystal at 180° temperature.* He suggested the employment of the microscope as a valuable assistance in detecting spurious from real gems; very few of the latter are perfect, and the flaws and cavities are so distinct in character from those which are so abundant generally in artificial gems that very little experience is sufficient for the purpose. This mode of testing of course is limited to transparent crystals, but might be employed when the usual methods are not practicable. He also mentioned Mr. Sorby's (F.R.S.) discovery of fluid cavities in the quartz of granite, in the quartz of volcanic rocks, and also in the feldspar ejected from the crater of Vesuvius, and Mr. Sorby's method of determining the temperature at which various rocks and minerals are formed. At the conclusion of the meeting, crystals containing fluid were exhibited under the microscope, and the expansion of the fluid by elevating the temperature of the crystal whilst under examination.

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* After this Paper was written, Sir David Brewster informed the Author that the fluid contained in crystals of fluor spar was water, and that the cavities burst at a temperature of 150°.

## Ordinary Meeting, February 4th, 1868.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

Among the donations announced were several bottles of Chemical Products for the Society's collection, from Dr. F. Crace Calvert, F.R.S., &c.

The thanks of the Society were voted to Dr. Calvert for his valuable donation.

"On Some Constituents of Cotton Fibre," by E. Schunck, Ph.D., F.R.S., &c., President.

It is generally supposed that cotton, when quite pure, consists entirely of woody fibre or cellulose, and that its composition is consequently represented by the formula $C_{12}H_{10}O_{10}$. It is certain, however, that in the raw state, as furnished by commerce, it contains a number of other ingredients, some of which occur so constantly that they may be considered essential constituents of cotton, viewed as a vegetable product. The object of the bleaching process to which most cotton fabrics are subjected is to deprive the fibre of these other ingredients and leave the cellulose behind in a state of purity. Notwithstanding the importance of an accurate knowledge of everything relating to cotton from an industrial point of view, the substances contained in it along with cellulose have never been subjected to a special chemical examination, and very little is consequently known about them. Persoz, in his *Traité de l'Impression des Tissus*, says that the woody fibre constituting the tissues of cotton, hemp, linen, &c., is not pure; it contains 1st, a certain quantity of colouring matter, which is more or less shielded from the action of decolorising agents by the bodies which accompany it, naturally or accidentally; 2ndly, a peculiar resin natural to the fibre, insoluble in water and soluble with difficulty in alkalies, which plays the part of a reserve and protects the colouring matters inherent in the fibre from the action of the agents which ought to destroy and remove them; 3rdly, a certain quantity of fatty matter, of which a very small portion is peculiar to the fibre, the greatest part being derived from the operations of spinning and weaving; 4thly, a neutral substance, either flour, starch, or glue, which has been introduced by the weaver in sizing his warp; 5thly, inorganic saline matters, some of which belong to the fibre, while the others are derived from the water and the matters employed in the dressing of the warp. In the excellent article on Bleaching in the new edition of Ure's *Dictionary of Arts* there is a full account of these and other impurities of cotton fabrics, comprising all that was known at the time when the author commenced his examination.

The object which the author had in view in undertaking his investigation was to endeavour to throw a little more light on the nature of those substances which are contained in or attached to the framework of cellulose, of which cotton fibre mainly consists, and which are together with the latter produced by the plant. All foreign and extraneous matter introduced during the process of manufacture was therefore left entirely out of consideration. The author has further confined his attention to those constituents of the fibre which are insoluble in water but soluble in alkaline lye, and are afterwards precipitated by acid from the alkaline solution. Whether cotton contains naturally any substance soluble in water, or which being originally insoluble is rendered soluble therein by the prolonged action of alkalies is a question on which the author pronounces no decided opinion.

For the purpose of obtaining the substances which he proposed to examine the author employed cotton yarn, which he preferred to unspun cotton for several reasons, the principal being that yarn is comparatively free from mechanical impurities, such as fragments of seed-vessels, &c., while on the other hand, if proper care be taken, no impurity is added to those previously existing during the process of spinning. The yarn was boiled in an ordinary bleacher's kier for several hours with a dilute solution of soda ash. The resulting dark brown liquor, after the yarn had been taken out, drained and slightly washed, was removed from the kier into appropriate vessels, and mixed with an excess of sulphuric acid, which produced a copious, light brown, flocculent precipitate, while the liquid became colourless. This precipitate was allowed to settle, the liquid was poured off, and after being washed with cold water to remove the sulphate of soda and excess of acid it was put on calico strainers and allowed to drain. A thick pulp was thus obtained, which when dried assumed the appearance of a brown, brittle, horn-like substance translucent at the edges. In one experiment 450 lbs. of yarn, made from East Indian cotton, of the variety called "Dhollerah," yielded 0·33 per cent. of the dried precipitate. In another experiment made with 500 lbs. of yarn, spun from American cotton, of the kind called in commerce "middling Orleans," 0·46 per cent. was obtained. The total loss sustained by yarn during the bleaching process amounts to about five per cent. of its weight. Only a small portion of the matter lost is therefore recovered by precipitation of the alkaline extract with acid.

This precipitate formed more especially the subject of the author's investigation. It was found to consist almost entirely of organic substances, and of these the following were distinctly recognised:—

1. A species of vegetable wax.
2. A fatty acid.
3, 4. Colouring matters.
5. Pectic acid.
6. A trace of albuminous matter.

The author described the method employed by him for separating these substances from one another and obtaining them in a state of purity; and he then gave an account of their properties and composition.

The waxy matter is by far the most interesting of these substances. It is insoluble in water, but soluble in alcohol and ether. If a concentrated solution in boiling alcohol be allowed to cool, the greatest part is deposited, causing the liquid to assume the appearance of a thick white jelly, consisting of microscopic needles or scales. When this jelly is filtered off and dried it shrinks very much, and is converted into a coherent cake, which has a waxy lustre and is translucent, friable and lighter than water. Its melting point is between 83° and 84° C. At a higher temperature it is volatilised. When heated on platinum it burns with a very bright flame. The author thinks it probable that this substance covers the cotton fibres with a thin waxy film, and thus imparts to them their well-known property of resisting water. In its properties and composition it approaches very nearly the better known vegetable waxes, such as that obtained by Avequin from the leaves of the sugar cane, and that which is found on the leaves of the Carnauba palm. The author thinks that the name *cotton wax* is sufficient to distinguish it from these and other nearly allied bodies.

The fatty acid has the properties and composition of margaric acid. It is white and crystalline, fuses at 53° C., and gives with alkalies compounds soluble in water which are true soaps. It is, however, probably not a natural constituent of cotton fibre, but rather an impurity derived from the oil of the seed which escapes and diffuses itself among the cotton before or during the process of ginning. It might also have had its source in the oil and fat, used for greasing the cotton spinning machinery, since the author employed yarn in all his experiments. Persons practically conversant with cotton spinning affirm, however, that if ordinary care be taken, it is impossible that the cotton can become contaminated with anything of a fatty nature, during its conversion into yarn.

The colouring matters obtained in these experiments are without doubt the substances to which raw cotton owes its yellowish or brownish colour. The author was able to distinguish two bodies of a dark brown colour, which occurred in all kinds of cotton examined by him. Of these one is easily soluble in cold alcohol, and is left, on evaporation of the solution, as a dark brown, shining, brittle, amorphous resin, which is transparent in thin layers. In boiling water it softens and melts to a pasty mass, which becomes hard and brittle again on cooling. When heated on platinum foil it burns with a bright flame, leaving a very voluminous coal. It is nearly insoluble in ether. It dissolves easily in concentrated sulphuric acid and glacial acetic acid, with a brown colour. It also dissolves with ease in caustic and carbonated alkalies, giving dark yellowish-brown solutions, from which it is re-precipitated by acids in light brown flocks. The other colouring matter resembles this in most of its properties. It is, however, much less soluble in alcohol. Cold alcohol, indeed, dissolves only a trace, but in boiling alcohol it dissolves with tolerable facility, being re-deposited, on the solution cooling, in the form of a brown powder. This powder, when filtered off and dried, forms coherent masses of a colour varying from light to dark brown, which are easily broken, showing a dull earthy fracture. Both colouring matters contain nitrogen, and they differ therefore in constitution from true resins, which they resemble in many of their properties. The peculiar colour of the so-called "Nankin cotton" is probably due to a great excess of these colouring matters existing in the fibre. It is certainly not caused by oxide of iron.

The purification of the pectic acid contained in the brown precipitate produced by sulphuric acid was not effected without difficulty. The best method, according to the author, consists in submitting it to a simple process of bleaching with chloride of lime, by which means the impurity, consisting of brown colouring matter, which adheres to it with great pertinacity, is destroyed. When pure it has the properties and composition ascribed to pectic acid by Fremy. The cotton itself probably contains pectose or pectine, which is converted into pectic acid by the action of the alkaline lye. About three-fifths of the brown precipitate consists of pectic acid. Of the remaining two-fifths the colouring matters constitute by far the largest part, the wax and fatty acid being present in very minute quantities. The albuminous matter was not isolated, but its presence was indicated by the formation of a small quantity of leucine, which took place when the brown precipitate was submitted to the action of hydrate of soda. A large quantity of oxalic acid was formed at the same time, no doubt from the pectic acid.

In conclusion, the author makes some remarks in regard to the part which these bodies may be supposed to play during the process of manufacturing gun cotton. It has been asserted that the instability occasionally observed in gun cotton is to be attributed to the impurities in the raw fibre, forming, by the action of nitro-sulphuric acid, bodies which decompose spontaneously at the ordinary or a slightly elevated temperature. The author's experiments do not support this view, since the substances described by him, when submitted to the action of the mixed nitric and sulphuric acid of the strength employed for making gun cotton, do not yield explosive compounds.

## PHYSICAL AND MATHEMATICAL SECTION.

January 28th, 1868.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

"On Solar Radiation," Part II., by JOSEPH BAXENDELL, F.R.A.S.

It has been shown in the first part of this paper that the curve laid down from the monthly means of solar radiation derived from the five highest values in each month, or those corresponding to the five clearest days, exhibits two maxima and two minima occurring respectively in April and September, and June and December. In order however to determine the influence of the seasons upon the amount of solar radiation on clear days, it will be necessary to correct these means for the difference of meridian altitude of the sun in the different months of the year. For this purpose I have employed the table given in the article on Climate in the *Encyclopædia Britannica*, referred to by Sir John Herschel in a note at foot of page 11 of his admirable *Treatise on Meteorology;* and I have neglected, as being immaterial for our present purpose, any correction which might be necessary in a more exact inquiry in consequence of equal increments in the difference between sun and shade temperatures not corresponding exactly to equal increments of calorific intensity of the sun's rays. This table gives for every fifth degree of altitude the calorific intensity of sunlight, the intensity of the light on entering the earth's atmosphere being taken =1. If the rate of emission of heat from the sun were constant, and change of season exercised no influence on the absorptive power of the atmosphere, the quotient obtained by dividing the mean value of solar radiation on clear days in any month, by the number in this table corresponding to the sun's meridian altitude in the middle of the month, would be a constant quantity. The results given in the following table will however show that according to the Oxford observations this is by no means the case:—

| | Mean Amount of Solar Radiation on Clear days. | Meridian Altitude of the Sun on the 15th of the Month. | Proportion of Light Transmitted. | Number in First Column divided by Number in Third Column. |
|---|---|---|---|---|
| | ° | ° ′ | | |
| January ............ | 13·5 | 17 6 | ·37 | 36·5 |
| February ......... | 17·6 | 25 32 | ·51 | 34·5 |
| March ............ | 17·8 | 36 3 | ·61 | 29·2 |
| April .............. | 20·3 | 47 57 | ·68 | 29·8 |
| May .............. | 19·2 | 57 4 | ·71 | 27·0 |
| June .............. | 18·1 | 61 34 | ·72 | 25·1 |
| July .............. | 19·2 | 59 50 | ·72 | 26·7 |
| August ............ | 19·6 | 52 23 | ·69 | 28·4 |
| September ......... | 20·0 | 41 22 | ·65 | 30·8 |
| October ............ | 18·1 | 29 48 | ·56 | 32·3 |
| November ......... | 15·6 | 19 48 | ·43 | 36·3 |
| December ......... | 13·0 | 14 59 | ·33 | 39·4 |

The numbers in the last column show that the power of the atmosphere to absorb the heating rays of the sun is much greater in the summer than in the winter months, the maximum effect taking place in the month of June, when the sun attains his greatest meridian altitude, and the minimum in December, when his meridian altitude is least. As the amount of aqueous vapour in the atmosphere is much greater in summer than in winter, this result tends strongly to confirm the view taken in my paper "On the Theory of Rain," and since held to be established by Professor Tyndall's experimental investigations, that air charged with aqueous vapour has a much greater power of absorbing and radiating heat than dry air. If however we use the Greenwich observations in this investigation we shall arrive at a totally different result. Taking, for instance, those made in 1857, and treating them by the method employed in discussing the Oxford observations, we have the results shown in the following table:—

| | Mean Amount of Solar Radiation on Clear Days. | Meridian Altitude of the Sun on the 15th of the Month. | Proportion of Light Transmitted. | Number in First Column divided by Number in Third Column. |
|---|---|---|---|---|
| | ° | ° ′ | | |
| January ........... | 9·0 | 17 19 | ·38 | 23·6 |
| February ........ | 16·6 | 25 42 | ·51 | 32·6 |
| March ........... | 18·2 | 36 37 | ·62 | 29·3 |
| April .............. | 23·8 | 48 29 | ·68 | 35·0 |
| May .............. | 24·8 | 57 31 | ·71 | 34·9 |
| June .............. | 26·9 | 61 52 | ·72 | 37·3 |
| July .............. | 26·8 | 59 59 | ·72 | 37·2 |
| August ........... | 24·8 | 52 25 | ·69 | 35·9 |
| September ........ | 22·9 | 41 21 | ·05 | 35·2 |
| October ........... | 20·2 | 29 47 | ·56 | 36·1 |
| November ........ | 9·6 | 19 52 | ·43 | 22·4 |
| December ........ | 3·9 | 15 12 | ·33 | 11·9 |

The numbers in the last column of this table are considerably greater in the summer than in the winter months, thus indicating a greatly reduced absorptive action of the atmosphere in the sun's heating rays in the warmest half of the year, when the quantity of aqueous vapour in the air attains a maximum; a result directly opposed to that derived from the Oxford observations, and to the conclusions drawn by Professor Tyndall from his experiments. If, however, we examine the conditions under which the observations were made at Greenwich, we find that while the ordinary thermometers on the shade stand were placed with their bulbs about 4 feet from the ground, the solar radiation thermometer was placed in an open box about 13 inches high with its bulb about 10 inches above the bottom of the box. Now, a little consideration will show that on a clear calm day in summer the air in this box will be heated to a temperature several degrees above that of the air at 4 feet from the ground; while on the other hand in winter it will often be several degrees colder. The readings of a thermometer placed in it will therefore be too high in summer and too low in winter; and the magnitude of the errors may well be sufficient not only to mask the true action of the varying amount of the aqueous vapour in the atmosphere, but to lead to conclusions directly at variance with the truth.

The heating effect of the sun's rays at the surface of the earth during a given interval obviously depends upon the the greater or less prevalence of cloud or haze, and it is also evident that the amount of the latter will in general depend upon the degree of humidity of the air as determined from observations taken with the dry and wet bulb thermometers. Any alterations therefore which may take place in the calorific intensity of the sun's light ought to be indicated by a comparison of the differences between the mean temperatures of the air and of evaporation, and those between the maximum temperatures in the sun and in the shade. The values of these elements for the years 1859–64 at Oxford, and their ratios, are as follows:—

| | Maximum in Sun less Maximum in Shade. | Temperature of Air less Temperature of Evaporation. | Ratio. |
|---|---|---|---|
| 1859 .............. | 12·85 | 2·67 | 4·81 |
| 1860 .............. | 10·78 | 1·98 | 5·44 |
| 1861 .............. | 11·24 | 2·49 | 4·51 |
| 1862 .............. | 9·37 | 2·72 | 3·44 |
| 1863 .............. | 9·78 | 2·87 | 3·40 |
| 1864 .............. | 9·81 | 2·73 | 3·59 |

A projection of the ratios gives a curve closely resembling the curve No I, diagram B (p. 39), and therefore strongly supporting the view that the calorific intensity of the sun's rays varies in a period corresponding with the solar spot period.

If instead of the difference between the mean tempertures of the air and of evaporation we employ the difference between the mean temperatures of the air and of the dew point we obtain the following ratios.

| | |
|---|---|
| 1859 .............. | 2·33 |
| 1860 .............. | 2·57 |
| 1861 .............. | 2·17 |
| 1862 .............. | 1·66 |
| 1863 .............. | 1·65 |
| 1864 .............. | 1·72 |

It will be seen that the curve representing these ratios is identical in form with that representing the last series of ratios, as indeed might have been expected.

Proceeding now to apply the same method to the treatment of the mean monthly values of solar radiation we have the following table:—

| | Mean Monthly Solar Radiation. | Mean Temperature of Air less Mean Temperature of Evaporation. | Ratio. |
|---|---|---|---|
| | ° | ° | |
| January | 5·51 | 1·31 | 4·20 |
| February | 9·16 | 1·75 | 5·23 |
| March | 10·08 | 2·36 | 4·27 |
| April | 13·23 | 3·00 | 4·41 |
| May | 12·90 | 3·78 | 3·41 |
| June | 12·93 | 3·43 | 3·76 |
| July | 13·81 | 4·15 | 3·32 |
| August | 14·03 | 3·73 | 3·76 |
| September | 13·71 | 2·81 | 4·88 |
| October | 9·66 | 2·05 | 4·71 |
| November | 8·00 | 1·41 | 5·67 |
| December | 4·50 | 1·15 | 3·91 |

Before the true import of the numbers in the last column can be clearly understood, it will be necessary to apply corrections for the changes of meridian altitude of the sun in the different months of the year, and employing for this purpose the table in the article on climate in the *Encyclopœdia Britannica* already referred to, we obtain the following corrected numbers:—

| | | | |
|---|---|---|---|
| January | 1·59 | July | 2·39 |
| February | 2·66 | August | 2·59 |
| March | 2·60 | September | 3·17 |
| April | 3·00 | October | 2·63 |
| May | 2·42 | November | 2·44 |
| June | 2·70 | December | 1·29 |

The highest values occur in April and September, the months in which the diurnal oscillations of the declination magnetometer are greatest, and the general course of the numbers indicates that clouds and haze are less prevalent, or less dense, or their power of absorbing the heating rays of the sun less active, in the spring and autumn than in the winter and summer months; and in connection with this it may be remarked that the rate of change in the difference between the temperature of the air and the temperature of evaporation is greatest in the months of April and September.

Since the first part of this paper was printed in the Society's Proceedings, Mr. MACKERETH, F.R.A.S., has communicated to the Physical and Mathematical Section the monthly results of his Solar Radiation Observations made at Eccles during the five years 1862–66. As these observations were made with a black-bulb thermometer placed somewhat similarly to that used at Oxford, and as the series extend two years beyond the Oxford series, it was evidently very desirable to examine how far they confirmed the conclusions derived from the Oxford observations. The mean annual values of solar radiation having been deduced from the monthly means, and Mr. Mackereth having kindly supplied me with the annual mean temperatures of the air and of the dew-point, they were treated on the plan employed in discussing the Oxford observations. These data and the results thus derived from them are shown in the following table:—

| | Mean Temperature of Air. | Mean Temperature of Dew Point. | Difference. | Mean Amount of Solar Radiation. | Ratio. |
|---|---|---|---|---|---|
| | ° | ° | ° | | |
| 1862 ………… | 47·3 | 42·3 | 5·0 | 10·90 | 2·18 |
| 1863 ………… | 48·3 | 42·3 | 6·0 | 11·47 | 1·91 |
| 1864 ………… | 47·0 | 40·5 | 6·5 | 11·50 | 1·77 |
| 1865 ………… | 48·6 | 43·3 | 5·3 | 9·72 | 1·83 |
| 1866 ………… | 48·3 | 42·3 | 6·0 | 8·25 | 1·37 |

The mean ratio for the two years 1865–6 is 1·60, while that for the years 1862–4 amounts to 1·95. It appears therefore that the calorific intensity of the sun's rays continued to diminish for two years after the termination of the Oxford series, and as the observations of Schwabe, Wolf, Balfour Stewart, and others, have shown that the frequency of solar spots also diminished during these two years the probability that a close connexion exists between the two phenomena is considerably increased by the results of Mr. Mackereth's short but valuable series of observations.

On comparing the Oxford and the Eccles series of results it will be seen that the values for the years which are common to both (1862–4) are greater at Eccles than at Oxford in the ratio of 1·17 to 1·00, thus indicating that the blackened bulb of the thermometer at Eccles absorbs radiant heat more readily than that at Oxford. Dividing the Eccles mean annual values by 1·17 in order to reduce them to the Oxford scale and recalculating the ratios we have

| | Reduced Values of Solar Radiation. | Ratios. |
|---|---|---|
| 1862 .............. | 9·33 | 1·82 |
| 1863 .............. | 9·82 | 1·68 |
| 1864 .............. | 9·84 | 1·51 |
| 1865 .............. | 8·32 | 1·57 |
| 1866 .............. | 7·06 | 1·17 |

Combining now the two series of ratios, and taking the means of the two values given for each of the three years 1862-4, we have finally, the following series of corrected ratios all referred to the same scale, and therefore strictly comparable with each other. I have added for comparison the number of groups of solar spots observed in each year by Schwabe:—

| | Ratios. | Number of Groups of Solar Spots. |
|---|---|---|
| 1859 .............. | 2·33 | 205 |
| 1860 .............. | 2·57 | 211 |
| 1861 .............. | 2·17 | 204 |
| 1862 .............. | 1·74 | 160 |
| 1863 .............. | 1·64 | 124 |
| 1864 .............. | 1·61 | 130 |
| 1865 .............. | 1·57 | 93 |
| 1866 .............. | 1·17 | 45 |

The curves laid down from these two series of numbers present so close a resemblance in their general form, that it seems impossible to resist the conclusion that the causes which influence the frequency of solar spots also produce corresponding changes in the calorific intensity of the sun's rays.

In addition to the conclusions drawn from the discussion in the first part of this paper we have now the following:—

1. The power of the atmosphere to absorb the heating rays of the sun is much greater in the summer than in the winter months, and depends apparently upon the amount of aqueous vapour which it contains.

2. Clouds and haze are less prevalent during the day, or their power to intercept the heating rays of the sun is less active in the spring and autumn than in the winter and summer months.

3. Observations of solar radiation made with a black bulb thermometer, to be of value, ought to be taken with the radiation thermometer placed at the same height above the ground as the shaded maximum thermometer, with

which it is compared; but while freely exposed at all times to direct sun-light, it ought to be protected as much as possible from disturbing influences.

4. Solar radiation observations made on a plan similar to that adopted at Oxford, show that the calorific intensity of the sun's light continued to diminish during the years 1865-6, when the frequency of solar spots was also diminishing, thus giving additional weight to the probability that changes in the heating power of the sun's rays are intimately connected with variations in solar spot frequency.

## Ordinary Meeting, February 18th, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Dr. H. E. ROSCOE, F.R.S., communicated some of the results of his recent researches "On the Properties of Vanadium," and exhibited specimens of some of the more interesting of its compounds.

## Ordinary Meeting, March 3rd, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"On the Lunar Crater Linné," by Herr J. F. J. SCHMIDT, Director of the Observatory at Athens. Extract from a letter dated February 6th, 1868, addressed to Mr. Reimers, of Manchester. Communicated by J. B. DANCER, F.R.A.S.

"When I received your letter I had just returned from the Sartorin Islands. As regards the Linné crater, I find that it still presents the same appearance, viz., as a white cloud; but when the light is very sharp, I see more frequently than before a small opening or hole about one-tenth the diameter of the former crater.

"After the publication, by Baill of Leipsic, of my great Catalogue of the Ravines or Crevasses of the Moon, I discovered many other remarkable ones—an account of which I shall send soon to the Lunar Committee in London. The most complete account of my observations on Linné you will find in my general report to the Academy of Vienna, published by Gerold.

"No eruption of a telluric nature can be the cause of a change in Linné, but a filling up of the crater by masses of mud or dust; otherwise the shadow of the smoke, as in an ordinary eruption such as I observed at Vesuvius and at the Sartorin Islands, would not have escaped my observation."

"Brief Notes on the Laws of Physical Force," by J. C. DYER, Esq., V.P.

The wide acceptance of the new doctrine of the conservation of physical energy, or force, has led me to submit a few remarks thereon.

In the first place, let us keep in mind that there are five kinds of natural forces continually exerted by material bodies. Three of these are mechanical forces, and, as such, are measurable by the known laws of physics—viz., the force of gravity, of inertia, and of elasticity, each being co-extensive with, and inherent properties of all tangible bodies. The actual exertion of these, by their equal action and reaction, serves to sustain the positions and forms (whether in motion or at rest) of the entire material universe; and it is only by the observed action of such balanced forces that we are assured of the existence of material bodies.

Many theories have been propounded to account for the presence of these several forces in matter; yet no real light has been shed upon the questions involved in such inquiries beyond the fact that all these forces are found to be essential forces of the bodies exerting them. The two first are directly as the quantity of matter, and the third or elastic force varying in degrees of action according to the condition of bodies as solids, liquids, gases, or vapours. I have elsewhere aimed to show that a pervading "calorific element" constitutes the essence, or is the source, of the elastic forces in all ponderable bodies — a question here passed by.

Besides the above mechanical forces, we have to consider the two other forces, namely, the chemical and vital forces. The affinities and repulsions of the component parts of matter, by which its mutations are affected, constitute the chemical forces. These forces are of varying and complex intensities, and are still but vaguely understood even by our most able practical chemists; yet the reality of such forces is extensively evidenced to our senses, though they are seldom such as can be distinctly measured like the mechanical forces.

With respect to the vital forces, they are of a still more complex and recondite nature, and we must be content in our present state of knowledge to place these forces among the many other sublime and mysterious laws impressed upon organic matter by an all-wise Providence, and which are not yet placed within the range of man's mental vision. We simply know that the vital forces are, in part, controlled by the will, and a greater portion of them are called into action by organic stimulants; and since both kinds of action cease with death, they are properly treated as vital forces. Whilst we know not how the vital forces control the mechanical and chemical forces exerted in and by living beings, there is no lack of proof that they do in fact command the other forces exerted through organic nature.

I shall here cite an eloquent passage from an essay (given in a popular journal, *Once a Week,* for 13th October, 1865) by one of the most able exponents of what is called the "New Philosophy," as follows, namely:—"The great philosophical doctrine of the present era of science, as the conservation of energy has been worthily styled—teaches us that the activity which we see manifested in all the natural forces is a constant quantity, or in other words, that there is a definite amount of force distributed through nature, which is invariable in amount, and which we can neither add to nor take from, but whilst the force in one form disappears, it reappears in another form. But what do we mean by the term force? The simplest definition of the term is that which describes it as something which produces or resists motion. When we think of force we think of something moving or moved."

This definition of the term force would be unexceptionable, if what is called force, be held simply as indicating the properties of the bodies in which it acts, and not as a something, or energy, existing by itself, as an entity, apart from matter, but as a part of it. Were it merely intended to declare, that the sum of the mechanical forces, as gravity, inertia, and elasticity, is a constant quantity, just as is that of the bodies in which these forces are inherent properties, then there would be no grounds for asserting this fact, as disclosing a "great new doctrine;" for the indestructible nature of matter, and of its properties, were physical truths known and taught in our childhood.

The relations of matter and force—at least as regards the mechanical forces—have been set forth in such ample detail, comprising all known facts, in the elements of physical science, that it would seem needless to repeat the facts, of the unchangeable nature of the material universe, both as to the amount of, and the kind of forces, pertaining to matter, but for the enunciation of theories implying such changes, wherefore to treat of the conservation of the natural forces, and of the disappearance of "one kind of force and reappearance of another kind," must be held as employing unmeaning, or misleading terms. It is not that any of the forces themselves can disappear, or be changed from one to another kind of force; but that the motions generated by them may cease, or be rendered quite uniform when the moving forces are exactly balanced, by their action in opposite directions.

In short, the term force, used in an abstract sense, and without defining the kind of force intended, is worse than idle or unmeaning, because when so used by eminent physicists, it cannot fail to puzzle and mislead the younger students in the sciences. The author quoted frankly states that the dogmas he expounds—"seem not quite so clear to the unscientific mind."

Considering that this city is so widely distinguished, for its practical application of the mechanical and chemical sciences, it would reflect much discredit on the community, for us to be unable to comprehend the simple elements of the natural forces, so largely guided by our hands. Hence, a few words seem called for, to distinguish the several forces, acting conjointly, or separately; and to protest against their being confounded together, as an abstract entity to be conservated.

There seems no ground for asserting any new discoveries relating to the natural forces; yet a boundless field lies before us, for new applications of those forces, to the ever expanding and varying uses of man.

In addition to the before-mentioned fallacies, of including all physical forces under one head, and treating them as an abstract entity, it is assumed by the new philosophy, that vast mechanical forces, are continually exerted by special natural agencies, which hitherto have not been known to exert, or to possess this kind of force in any degree. To sustain this assumption, its authors have not been able to adduce any facts, or even analogies, in known phenomena, to prove or render probable the exertion of such forces, as, for instance, the alleged mechanical forces of the solar rays, when they are intercepted by aqueous vapour in the air, and the like forces assumed to be in continuous action, by and among the agitated atoms and molecules, or the ultimate particles of tangible bodies, and thus by their internal motions, tending to change, or to restore their sizes and forms, when the bodies are altered in either by external forces. Now, considering that such ultimate particles, called "atoms" and "molecules" are not discoverable by any analysis of, or known mutations of matter, to assert the exertion of such invisible moving forces, as apart from the tangible matter exerting those forces, is clearly a gratuitous assumption, unwarranted by any known properties of material bodies. With respect to the solar rays, we have many striking evidences of their chemical action in terrestrial phenomena; but no proof whatever has yet been adduced, to show their mechanical force or action upon tangible bodies, unless their impinging on the optic nerves, giving the sensation of light, may be held to be a mechanical action; but even if this be so, the force exerted can be but slight. Wherefore these newly discovered forces must "vanish into air, into thin air."

From the same high authority (before named) after giving a brilliant exposition of "the source of the solar rays," or as it is termed, "the origin and sustentation of the heat of the solar furnace"—and showing how the growth of vegetables depends on the solar influence, and that our stores of coal come from plants, also that by the combustion of coal, steam-power is obtained and machinery driven, &c. &c. The author then adduces the following case, namely—

"From machine power, we turn to muscular power. Between the steam-engine and living bodies there is the closest analogy. The nutritive materials upon which life depends, are no more nor less than combustible substances, which actually undergo a slow combustion. The conversion of food into work done, is effected by the same process as that which turns coal, or wood into motive force."

That we must eat to live, and the steam-engine must have steam to work it, are simple facts that require no comments; but the forces exerted to sustain the movements of a metallic engine, and the living organisms, are so widely different in the two cases, that one could hardly expect to see their close analogy, asserted by any writer on the laws of physics, except under strange illusions, concerning the kinds of forces employed in the economy of nature.

It is an obvious fallacy to assert such close analogy, to exist between the functional energies of living beings, and the force of steam in an engine. The distinction in the two cases, is plainly seen by reference to the two kinds of force, called into action by each of them; the first being a series of organic energies, that connect and sustain the necessive motions, through the complex structure of the said beings, the other being simply the elastic force that drives the engine. To support this alleged analogy, its authors should be enabled to bring some sort of proofs, that the vital forces do result solely, from the chemical forces evolved in the living system; but which is not sustained by any known facts, or deductions from facts. No "new philosophy" is required, to teach us that all organic beings, depend for their existence on solar heat, as also on the due supply of proper food. We also know that "the nutritive materials for sustaining life," contain a large portion of carbon, in the forms of gelatine, fibrine, saccharine, starch, adepose and other analagous bodies in common food; but these facts afford no explanation as to the origin, development and final decay of "nature's living progeny."

The vital forces, from the first germ to the close of life, exhibit in a clear plan, a train of connected and harmonious action, in guiding and controlling the chemical and mechanical forces, that are exerted in carrying on the functional powers of life—showing that the vital force was adapted to and designed to control the others.

The marvellous energy of the heart, far exceeds the force of any hydraulic engine of the like size; the forces exerted by the peristaltic, and other internal muscles, as also the chemical forces acting through the lungs, stomach and general circulations are alike inexplicable, upon any theory however ingenious, of slow combustion, or of machine power.

Although the term vital force may be somewhat vague, it seems to point out the special energies, that originate organic forms, and to multiply and expand them by a series of spontaneous actions, from the seeds and ova, to build up the living structures of plants and animals, and upon the attainment of their maturity, the subsidence of those energies commences, nor can any intrinsic aid, or stimuli prevent their extinction at the appointed time.

If the term force be made to include those exerted by the vital organs, and if the conservation of such forces be a practical reality, then the living energies need never to "fade away and expire," except through the wilful neglect of the conservative doctors.

I will here add, in the words of a profound thinker, that, in organic life, "every train of development exhibits in its course an adherence to plan, which can only have its ground in an internal vital destination. It exhibits at the same time an independence of all external influences, which testifies to the internally given force of vitality."*

It must be observed that these comments on vital forces relate solely to those functional energies that produce physical action, and which are quite apart from the "moral and intellectual forces" exerted externally by sentient beings upon each other — a subject of wider and far higher nature than any of mere physical action.

I venture to recommend a careful perusal of the learned and eloquent essay (in *Once a Week*) before quoted, in order to a clear and full comprehension of what is called "the great philosophical doctrine of the present era of science."

All know that in the act of breathing, the carbon in the blood is converted into its acid form, whereby the heat of warm-blooded animals is mostly kept up. This process is properly enough termed combustion. It is alike known that in the act of digestion, all sorts of food taken into the stomach are decomposed and converted into one uniform fluid (chyle), which process bears no resemblance whatever to that of combustion ; yet it is equally certain that animal life depends as strictly upon the digestion of food as upon healthful respiration. Indeed those chemical powers of the stomach are even more hidden and wonderful than the mechanical energy of the heart before noticed.

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* Ray Society's Bot. and Phys. Memoirs, 1863.

By the experiments of Dr. Beaumont, in the well-known case of the Canadian soldier's (St. André's) stomach, it was clearly proved that the many kinds of animal and vegetable food taken were dissolved and converted into a semi-fluid of one uniform substance in the course of about two to four hours, by the juice or fluid secreted by the epigastrium, the secretions following in succession and laying hold of the morsels of food as they were swallowed.

The secreted liquor (gastric juice) has never been formed in any other than "Nature's laboratory," nor has the chemist ever been able to discover the nature or constituents of this general solvent. How puerile, then, to talk of the analogy of these vital forces with those of slow combustion.

In conclusion let me add, that I make no pretence to any novelty in what is above said relating to the laws of force, but merely to have noticed some of the new principles and views that appear to be wholly untenable under every known law. To treat fully the many physical questions connected with the natural forces, and especially those of the vital forces, would demand (in place of a few pages) a work of ample size and labour, without exhausting the inquiries concerning obscure phenomena.

## MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 6th, 1868.

J. B. **Dancer**, F.R.A.S., President of the Section, in the Chair.

Mr. **Sidebotham** exhibited a new revolving table for Microscopical purposes, made to his order by Mr. Rogerson; also a large Duck's egg, containing a second egg with a perfect shell.

Specimens of the following plants were offered for distribution among the members by Mr. **Notcutt**: Iberis amara, Hierochloë borealis, Caucalis daucoides.

Mr. **Dancer** read a Paper entitled "Some Remarks on Crystals containing Fluid."

Mr. **Sidebotham** also read a Paper on "Some of the Rarer Plants found near Llandudno."

[Both these Papers were afterwards read at the Ordinary Meeting of the Society, held January 21st, 1868, see pages 83—90.]

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January 27th, 1868.

J. B. **Dancer**, F.R.A.S., President of the Section, in the Chair.

The **President** announced the death of J. Hepworth, Esq., Surgeon, who had occasionally contributed papers to the Section. Mr. Hepworth's industry and success in the preparation of injected objects for the microscope, and his liberality in placing them at the disposal of microscopists, was well known and appreciated by the members of the Section.

Mr. LINTON exhibited some neatly executed drawings and mounted slides of a Coccus found on an orange sent to him by Mr. Heys. The late Mr. R. Beck discovered this Coccus to be of the same species as those found on a Cotoneaster and Rosebush in his garden.

The PRESIDENT exhibited two erecting eyepieces for the microscope, by Nachet of Paris. Instead of placing the erector between the eyepiece and object glass, as is usual in the English instruments, Nachet combines the erector and eyepiece together. In one form the prism is placed between the eye lens and the field lens of the eyepiece; in the second form the prism is placed over the eye lens of the eyepiece.

He also exhibited a very portable compound microscope for travellers' use.

Mr. LINTON exhibited some live Diatoms from Southport.

## PHYSICAL AND MATHEMATICAL SECTION.

February 25th, 1868.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section, in the Chair.

"On the Aurora Borealis as a Weather Prognostic," by THOMAS OGILVY, Esq. Communicated by MURRAY GLADSTONE, F.R.A.S.

It is upwards of 36 years since my attention was directed to the aurora borealis as a weather prognostic.

I was then residing in Cheshire, having frequently to cross the river Mersey, and ride or drive several miles after dusk, which gave much opportunity of observing the aurora, and of remarking the nature of the weather which accompanied and followed its appearance.

I think it was generally seen when the wind, if any, was light; and in Winter, inclined to be northerly, with a tendency to frost; and when the corruscations were vivid, and particularly if extending towards the zenith, or showing much motion, I remarked that the aurora was *almost invariably* followed by a gale of wind, with rain, from S.W.—within from forty-eight hours, to four days.

The more brilliant and lively the appearance and motion of the aurora, the earlier the gale which followed took place, and the greater was its severity. Slighter manifestations of the northern lights were not followed by any appreciable change of weather.

One of the most striking appearances of the aurora I ever witnessed was at Corrimony, on the 18th November, 1848. It was of a deep fiery red colour, and extended all over the heavens; even from the zenith down to the summits of the southern hills; and within two days it was succeeded by one of the most violent storms of wind and rain from the S.W., I ever witnessed in that part of the kingdom.

It was only within the last four or five years that I met with an extract from a newspaper; published, at the time, in one of the midland counties of England; in which reference was made to the same appearance of the aurora borealis; on the same date, and about the same hour in the evening, eight or nine o'clock. It gave a similar description as to colour, extent over the visible horizon, &c., but added that it was not followed, "as usual," by westerly wind and rain. I would call attention to this fact, as it serves to illustrate a theory I would offer in explanation of the phenomenon generally.

I would only further notice an observation somewhat different in kind, and made during the day; it was mentioned by my grieve, soon after I went to reside in Elen Urquhart, 26 years ago; and he was one of the most observant, and intelligent men of his class I ever met with.

On a brilliant afternoon towards the end of Summer, I happened to remark to him, how very fine the weather had been and was, when, shading his eyes from the Sun, and looking up over head, he replied that, notwithstanding, there would be rain in three days, "for he saw it in the sky;" adding, he found this almost always right. On inquiry, and looking up in a similar way, I discovered that what he saw was a faint streaky cloud, nearly in the zenith, with a shooting and sort of wavy motion towards its southern margin.

This prediction was verified in the instance here referred to; and as often, afterwards, as I happened to remark the same appearances, but my attention was never so much, nor from its nature, so often directed to this as to the nocturnal phenomenon; and I do not, from my own experience, speak of it with the same confidence—but I cannot resist the conclusion that it is the same meteor, only presenting a different appearance as seen at night, and in bright sunlight.

From the frequency with which the aurora was at one time brought before me, I was gradually led to form some theory explanatory of its nature, and of the weather changes I had observed to succeed it, which I here submit, with diffidence, for consideration.

I would first notice a remark made, if I recollect aright, by Sir John Ross, the Arctic navigator, that the aurora was confined to the lower (or *lowest*) regions of the atmosphere.

That this should be the case in very high northern latitudes, would be quite in accordance with the theory I have formed. That the reverse is the case in our own country may be inferred from the appearances in Inverness-shire, and in England in November, 1848;—and, I believe, from many similar phenomena being observed, at the same times, over wide ranges of the lower latitudes. But this, if correct, is equally in accordance with the theory.

In explanation, I will suppose it may be assumed that the air passes from the poles towards the Equator; where, being rarified, it rises, and, in considerable degree, at least, returns back to the poles, in the higher regions of the atmosphere.

This general circulation is, no doubt, much modified nearer the surface of the globe, by large islands, chains of mountains, and more especially by extensive continents, as they are heated or cooled down by the comparative presence or absence of the sun's rays. But where not influenced by such cases, and in those latitudes where they have room to expand and find uninterrupted course over a wide and unbroken extent of ocean, the unvarying direction and perennial nature of the N.E. and S.E. trade winds appears to prove the fact of a constant flow of air from the poles towards the equator. On their approaching the equator, these winds, as might be anticipated, enter a zone of frequent calms, and are lost. This constant supply of air, poured from both poles towards the equator, must find vent in some way; and while running down the N.E. trade winds on a voyage to India, I have repeatedly observed, through the intervals of the lower clouds, detached thin ones moving at a great elevation towards the north, and directly against the course of the trade winds and of the much lower but heavier stratum of vapour carried along with them.

At the changes of monsoons, the opposite winds are found charged, respectively, with negative and positive electricity, and heavy storms of thunder and lightning result on their meeting; and the same is observed in a minor degree when easterly and westerly winds meet. Magnetism, so powerful in the higher latitudes, is weakened towards the equator; and if it may be inferred that, in like manner, the positive electricity brought by the atmosphere from the poles is discharged on approaching the equator and becomes negative, an explanation would be afforded of the cause and nature of the aurora borealis.

This is distinctly given as his opinion by the late Admiral Fitzroy, in his report of 1864, on the Polar and Equatorial Air Currents.*

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* "We have also found the telegraph wires much disturbed when these main (principal) currents were beginning to act positively in force. No great tempest—no heavy polar or tropical gale has occurred here since 1860 without more or less '*wire disturbance*' (as the telegraphists say); and I am led by these, with other concomitant facts, and examination of numerous sea records, as well as registers on land, to believe that such electric effects are caused by atmospheric collision, and that '*magnetic storms*,' sedulously watched now at many observatories, are *results* of atmospheric storms,

When a larger body than usual of light air from the south begins to descend upon the upper surface of the stream from the north, as those opposite currents in the atmosphere come into close proximity, their negative and positive electricities produce corruscations.

The rarity of the atmosphere and the great elevation probably prevent (at least for the most part) any sound or thunder being heard; and the former cause, joined with the manner in which the currents approach each other, may probably occasion the shooting, flickering movements of the aurora, and of the clouds formerly mentioned as being seen by day.

The arches of boreal light frequently seen stretching from E. to W., may be produced by large masses of air, charged with opposite electricities, meeting each other and feeding the flame, quietly and continuously, on an extended front; while the movements of light occasionally occurring throughout the length of these arches, may arise from the masses of vapour coming more actively into contact at particular points, and lighting up a corruscation, which like a running fire, passes along the whole line.

When the corruscations are more than usually vivid, or violent in their motion, it would indicate a larger arrival than usual of negatively electric air from S. or S.W., which, *in a shorter or longer time, according to its strength,* first checks, and then overpowers the N. or N.E. wind generally blowing when the aurora is seen. The lower temperature of the atmosphere, cooled down by the recent northerly wind, condenses the moisture borne by that from the warm

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somewhere in one hemisphere or other, instantaneously felt through the air, however remote—or by conduction, or by vibration—as an earthquake is felt on board ship in the middle of a deep ocean, even the South Pacific."

The Rev. P. Secchi, Director of the Roman Observatory, in writing of magnetic currents in a wire in the magnetic meridian, states that disturbances in it indicate the approach of storms, as well as the barometer.

south, and precipitates it in showers; or, in more extreme cases, in storms of rain.

If I do not mistake, Sir John Ross describes the aurora in high northern latitudes, in winter, as being frequent, almost constant, as well as *very* near the Earth.

The cause of this seems to be the opposing currents being then close to the point where they must necessarily come into contact, and where even the smallest portion remaining of the opposite electricity must be discharged.

Sir John also speaks of the waving motion being accompanied by a very perceptibly audible sound. This may be aided by the known property of highly condensed air to transmit sound to unusual distances; and I have a strong impression of having myself heard a sound similar to that produced by the waving of a flag, when the aurora was playing actively.

As far as it is known, the identity of the light flickering clouds, above described as seen by day, and of the aurora as visible at night, is a *new* observation; and the following opinions, expressed by men of great eminence in meteorological science, while in no instance identical with the theory here proposed, may it appears, be not unfairly adduced in its support. Beccaria, in the last century, offered a suggestion that there was a constant circulation of the electric fluid between north and south; but he gave an explanation different from the foregoing of the manner in which he conceived it might take place. Herschel states his idea, in general terms, that the northern lights are produced by the electric fluid; and Admiral Fitzroy, in his report referred to above, "On the Polar and Equatorial Air Currents," has the following passage:—"We find here that the main currents, polar and equatorial, have very different electrical characteristics—one, the polar, being always plus; the other, always minus;—if pure, unmixed with the polar."

There is, besides, the well-known experiment of passing an electric spark, in vacuo, through a glass tube, when it presents the appearance of the aurora borealis, even with its play.

I would only further advert to the fact, mentioned above, that the remarkable aurora in November, 1848, was not followed by rain, where seen in England; while within two days after its occurrence, there was a perfect deluge of rain, and almost a hurricane, in Inverness-shire.

If, on reaching a few degrees further north, the column of air from the south had come into contact with that from the north in sufficient force to drive it back on the surface of the earth; in other words, had proceeded so much farther downwards on its course to its ultimate depression near the pole, as thereby (in consequence of its necessary contact with the colder northern atmosphere) to condense and discharge the vapour with which it was itself loaded, but which it was too high up to get rid of, when seen in the centre of England; if this were the case with that remarkable aurora borealis, may it not likewise, without unfairness be deemed a corroboration of the theory suggested?

"On the Supposed Influence of the Moon on the Temperature of the Atmosphere near the Surface of the Earth," by JOSEPH BAXENDELL, F.R.A.S.

The January number of the "Monthly Notices" of the Royal Astronomical Society contains a paper entitled "Inductive Proof of the Moon's Insolation," by Mr. J. Park Harrison, M.A., in which the Author points out that when the mean daily temperatures at Berlin for the years 1820—1835; at Oxford for 1856—64; and at Greenwich for 1841—47 and 1856—64, are arranged in tables according to the age of the moon, they show that a maximum mean temperature occurs on the average, at each of the stations, upon the 6th and 7th day of the lunation, when the moon's crust turned towards the earth is coldest; and a minimum mean temperature some time after full moon, when the same crust has been exposed for several days to intense radiation from the sun. It does not appear that any other period than that of the moon's synodical revolution was tried; nor is any allusion made to the temperature period which, as Nervander and Buys Ballot showed many years ago, depends apparently upon the sun's geocentric rotation on his axis. The existence of both these periods has however been questioned. Thus Buys Ballot on tabulating a series of 70 years' mean daily temperatures according to the moon's age found that the highest temperatures occurred during the seven days after full moon, or precisely during that part of the lunar period in which, according to Mr. Harrison's results, the temperature is at a minimum; and the Astronomer Royal discussing the observations of temperature made at Greenwich from the 2nd of June, 1848, to the end of 1853, with reference to the period of the sun's geocentric rotation on his axis, obtained a series of numbers "which did not appear to support the hypothesis that there is any certain inequality of temperature depending on the meridian of the sun which is turned towards the earth." In an inquiry of this nature it is, however, manifestly very undesirable to rely upon observations taken during the same series of years at places so near each other as Greenwich and Oxford, or upon those taken at places far apart, but in different years. The general agreement between the results of the Berlin observations in 1820 to 1835, and those at Oxford in 1856 to 1864, as given by Mr. Harrison is, of itself, no proof that the temperature of the earth's atmosphere is sensibly affected by the moon. This agreement may, and indeed does, arise from causes altogether independent of any influence of the moon. In a paper which I communicated to this Society in March, 1864, "On Periodic Changes in the

Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface."* I showed that different years have temperature periods of different lengths ranging from about 23½ to 31 days, the minimum values occurring in years of minimum solar spot frequency, and the maximum in years when the solar spots are most numerous; the approximate mean value as derived from observations made at various stations in Europe, Asia, and North America, during the years 1833 to 1859, being 27·4 days; and it was also shown that although in any particular year observations at places scattered widely over the three Continents, gave the same value of the temperature period yet the epochs of maximum and minimum often differed very considerably, thus indicating that the variation was not due to any direct action of the sun, the effects of which would have occurred simultaneously at all the stations. The adoption of any fixed value of a temperature period whether that of the sun's geocentric rotation on his axis, or of the moon's synodical revolution round the earth, or any assumed period, did not satisfactorily represent observations made in different years, and gave much greater probable errors than a variable temperature period whose changing values followed the course of the curve of sun spot frequency; and thus while the results given by the Astronomer Royal in the "Astronomische Nachrichten," No. 934, do not support the hypothesis of a temperature period depending on the sun's rotation on his axis, they will, nevertheless, on careful examination be found to indicate changes of temperature occurring in a period which was gradually diminishing from the commencement of the series of observations in 1848 to its termination in 1853. On the other hand Mr. Harrison's results appear to support very strongly the hypothesis of a lunar period; but it is easy to show that they form no exception to the same general law. For this purpose I have made use of the St. Petersburg observations for 1856—64, being the years comprised in Mr. Harrison's Oxford series; and as the diagrams which accompany Mr. Harrison's paper show a maximum of mean daily temperature on the 7th day after new moon, and a minimum on the 5th day after full moon, I have arranged the St. Petersburg mean daily temperatures for the five days from the 5th to the 9th day after new moon, and the five days from the 3rd to the 7th after full moon in two tables. There were 111 lunations during the nine years, and the mean temperatures of the selected days were found to be as follows:—

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* Proceedings, Vol. III., p. 251—260.

| Days after New Moon. | Mean Tem. | Days after Full Moon. | Mean Tem. |
|---|---|---|---|
| 5th day......... | 38·32° F. | 3rd day ......... | 38·88° F. |
| 6th „ ......... | 38·30 | 4th „ ......... | 38·79 |
| 7th „ ......... | 38·57 | 5th „ ......... | 39·29 |
| 8th „ ......... | 38·16 | 6th „ ......... | 39·62 |
| 9th „ ......... | 37·65 | 7th „ ......... | 39·96 |
| Mean...... | 38·20 | Mean...... | 39·31 |

The temperatures of the days in the group after new moon, are all below the lowest in the group after full moon; and the mean of the first group is 1·11° below that of the second; but at Oxford, during the same series of years, the highest temperatures occurred on the days comprised in the first group, and the lowest on those in the second. It is evident, therefore, that Mr. Harrison's results cannot be regarded as a proof of the moon's insolation; or that it has any sensible effect upon the temperature of the atmosphere near the surface of the earth.

I may here remark that the results given by Mr. Glaisher, F.R.S., in his paper "On the Influence of the Moon on the Direction of the Wind" (Proc. Met. Soc., March, 1867), are also dependent upon the general law above referred to, and cannot fairly be regarded as proving that the moon has really any influence upon the direction of the wind, since in most years, other periods than that of the moon's synodical revolution give greater differences in the distribution of the winds than can be obtained by employing the lunar period.

"On the Rainfall at Old Trafford, Manchester, during the year 1867," by G. V. VERNON, F.R.A.S., F.M.S.

The rainfall for 1867 was 0·858 inches below the average of the last 74 years, and 8·590 below the fall for 1866. The fall occurred on 188 days in 1867, as against 214 days in 1866.

During the first three months of the year the fall somewhat exceeded the average. In the second three months the fall was above the average, but this was caused by a very wet April, the fall in this month being more than double the average, whilst in the other two months the fall was below the average, especially in June. In the three months ending September, the fall was considerably below the average. July was wet, but August and September were below the average, August not having one-half the usual fall.

In the last three months of the year, the fall was slightly below the average; October exceeded the average slightly, November was considerably below the average, and not half the amount that fell in November, 1866, whilst December exceeded the average. The daily falls exceeding half an inch were:—

| 1867. | Inches. |
|---|---|
| January 7 | 0·500 |
| April 20 | 0·630 |
| July 15 | 0·770 |
| July 16 | 1·110 |
| September 1 | 0·620 |
| October 7 | 0·520 |
| October 27 | 0·750 |
| November 14 | 0·510 |
| November 30 | 1·500 |
| December 1 | 0·500 |
| December 15 | 0·800 |
| December 20 | 0·540 |

In 1867 we had only two days upon which the daily amount exceeded an inch, whilst in 1866 the daily fall exceeded an inch upon five days, and upon one day, viz.:— 28th July, 1866, it was 1·985 in.

It would seem that in our district we can safely reckon upon having rain on an average of about 180 days in the year, or that rain falls on about half the days of the year. Of course this includes every day upon which a measurable amount of rain falls, and it is only right that every day on which rain falls, and can be measured, ought to be included, notwithstanding Mr. Symons' limiting the amount to days only on which the fall reaches 0·010 inch. In 1867, there were no less than 16 days on which rain fell less than 0·010 inch in amount, and to leave these out must vitiate the result. Moreover, in places where the rainfall is much less than here, there will probably be a much greater number of days upon which a daily amount of 0·010 inch falls. Every day upon which a measurable amount of rain falls ought to be given; and if for any other purpose a limit in amount is wanted, it ought to be tabulated separately.

### OLD TRAFFORD, MANCHESTER.

Rain Gauge 3 feet above the ground, and 106 feet above mean sea level.

| Quarterly Periods. | | 1867 | Fall in Inches. | Average of 74 Yrs. | Differences. | No. of Days' Rainfall in 1867. | Quarterly Periods. | | Quarterly Periods for 74 Yrs. |
|---|---|---|---|---|---|---|---|---|---|
| 1866 | 1867 | | | | | | 1867 | 1866 | |
| | | | In. | In. | In. | | In. | In. | In. |
| 59 | 42 | Jan...... | 3·270 | 2·490 | +0·780 | 15 | 7·646 | 8·403 | 7·171 |
| | | Feb...... | 2·930 | 2·394 | +0·536 | 12 | | | |
| | | March... | 1·446 | 2·237 | —0·841 | 15 | | | |
| 32 | 47 | April ... | 4·323 | 2·032 | +2·291 | 25 | 7·864 | 5·814 | 7·266 |
| | | May ... | 1·950 | 2·351 | —0·401 | 9 | | | |
| | | June ... | 1·591 | 2·883 | —1·292 | 13 | | | |
| 67 | 50 | July ... | 4·284 | 3·625 | +0·659 | 15 | 8·684 | 16·556 | 10·461 |
| | | August.. | 1·410 | 3·586 | —2·176 | 16 | | | |
| | | Sept. ... | 2·990 | 3·250 | —0·260 | 19 | | | |
| 56 | 49 | Oct...... | 3·979 | 3·811 | +0·168 | 22 | 10·385 | 12·396 | 10·539 |
| | | Nov. ... | 2·431 | 3·480 | —1·049 | 8 | | | |
| | | Dec...... | 3·975 | 3·248 | +0·727 | 19 | | | |
| 214 | 188 | | 34·579 | 35·437 | —0·858 | 188 | 34·579 | 43·169 | 35·437 |

## Ordinary Meeting, March 17th, 1868.

EDW. SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

E. W. BINNEY, F.R.S., F.G.S., exhibited to the members an original letter of the late Sir Walter Scott, written many years ago to a gentleman residing in the North Riding of Yorkshire, on the subject of an old ballad entitled "*Jock o' Milk*" :—

Sir,

You have doubtless by this time set me down as guilty of great ingratitude and unworthy of your farther correspondence for so long and unjustifiable a delay in answering your letter enclosing *Jock o' Milk.* The truth is I have been absent from Edinburgh for some weeks, and since my return my professional engagements have obliged me to leave the tales of the East, West, and Middle Marches as quiet in my desk as the bodies of their quondam heroes rest in their graves. At length I have an opportunity to acknowledge your obliging favour. My Incredulity with regard to the Ballad you have been so good as to send me is not yet entirely obviated. If it is not entirely and radically a modern fabrication, the ancient verses are what the ffrench call *beaucoup brodeés.* "Virtue is its own reward," trite as the sentiment is, can hardly be supposed quite so old as the reign of Dav$^{d}$ II. The Title of Duke was first introduced into Scotland in the Reign of Rob$^{t}$ 3$^{d}$, and was only conferred upon immediate relations of the Royal family till at a very late period the Hamilton family got that title.

There never was, as far as I can learn, a peer, whether Duke, Earl, or Baron, of the name of Irving, and although there were many landholders of the name in the south-west of Scotland, the principal seat of their Chieftain was *Drum*, in Aberdeenshire. So far with regard to historical fact—but a ballad maker is entitled to use great latitudes in that respect, and accordingly it is not upon the anachronisms that I chiefly found my disbelief in the antiquity of the Poem. It is rather upon the mixture of ancient and modern phraseology, and especially upon the different attempts at sentiment and pathos, inconsistent with the simplicity of the Minstrel style, that I ground my opinion, which will always, however, be subject to alteration upon reasonable and convincing evidence. The copy you have been so good as to send me is nearly the same with one which I found in Glenriddell's MS. collection of ballads, and with another procured from Mr. David Herd, of this place. The last copy has this memorandum—"This fragment was taken down from the recitation of some of the country people in Annandale, by William Bell, a writer there, who communicated it to D. H., but in a very bad case, about the year 1776, and he was afterwards informed that Dr. Clapperton, a surgeon, in Lochmaben, was in possession of a compleat copy of the Ballad, which never could be got, the Dr. intending, as was said, its publication along with several other curious ancient songs." As this account in a great measure tallies with that with which you have favoured me, I hope it may be yet possible to recover some account of the original copy of this curious Ballad, by which means we may perhaps be able to determine what parts are modern and what really ancient. I shall wait with impatience the result of your inquiries at your friend Mr. Lidderdale. The battle in question, if such there was, must have been fought in the course of the four years intervening betwixt 1342, the date of David's return from France, and 1346, when the fatal battle of Durham was fought, in which Randolph E. of Murray was slain, and the King himself led into captivity.—Believe me, Sir, with many thanks for your obliging communication, your faithful humble servt.,

Edin., 4 June, 1802. WALTER SCOTT.

"Note on 'An Essay on the Resolution of Algebraic Equations, by the late Judge Hargreave, LL.D., F.R.S.,'" by the Rev. THOS. P. KIRKMAN, M.A., F.R.S.

The following appears to me to be a demonstration, that Dr. Hargreave's very skilful attempt to solve the quintic is, like all that have preceded it, a failure:

One of the final equations at p. 94 is—

$$y_1^{\frac{1}{5}}+y_2^{\frac{1}{5}}=A_1(z_1-b_1)+A_2(z_2-b_1)+A_3(z_3-b_1)+A_4(z_4-b_1)+A_5(z_5-b_1)$$
$$=\Sigma(At).$$

If so, it is possible to find $a_1+a_1=A_1$, $a_2+a_2=A_2$, &c., such that

$$y_1^{\frac{1}{5}}+y_2^{\frac{1}{5}}=\left((\Sigma at)^5\right)^{\frac{1}{5}}+\left((\Sigma at)^{\frac{1}{5}}\right)=\left(P^5\right)^{\frac{1}{5}}+\left(Q^5\right)^{\frac{1}{5}},\text{ where}$$
$$P^5=y_1\text{ and }Q^5=y_2.$$

As $y_1+y_2=2(51)$, p. 85, is a rational and symmetrical function of the $z$'s, $P^5+Q^5$ is one also; and this is plainly impossible, unless $P+Q=\Sigma At$ is a symmetrical function of the $z$'s, That is

$$y_1^{\frac{1}{5}}+y_2^{\frac{1}{5}}=\Sigma(At)=A\Sigma t=A\Sigma(z-b_1)=0;\text{ and }y_1=-y_2;$$
$$\text{or }y_1+y_2=2(51)=0,$$

This destroys the form of the conditioned quintic in $z$, at p 82; for $(51)=0$ gives, as $(21)$ and $(31)$ are each $=0$, (p. 78), (p. 7), $(51)=4b_1^5-5b_1b_4+b_5=0$; or $b_5$ is expressible in $b_1$ and $b_4$.

I suspect that it follows in a similar way from the equation in p. 54, that $(41)=0$, in the quartic in $z$ in p. 53.

It is evident from consideration of the known solutions of the quartic, the cubic, and the quadratic, that every solution of the general quintic $F=0$, whose roots are $x_0\ x_1\ x_2\ x_3\ x_4$, must be a transformation, by algebraic artifice, of the expression

$$5x_0=\Sigma x+4x_0+(a+a^2+a^3+a^4)(x_1+x_2+x_3+x_4);$$

where $a$ is an imaginary fifth root of unity; and any transformation, by which the right side of this expression can be made to contain only symmetrical functions of $x_0\ x_1\ x_2\ x_3\ x_4$, will be, by proper reading of roots, a correct solution of F=0, as $x_0$ is any one of the five. Let us try to effect this. We can write

$$\begin{aligned}x_0=\frac{1}{5}\Sigma x&+\frac{1}{5}\Big((x_0+a_1x_1+a_2x_2+a_3x_3+a_4x_4)^5\Big)^{\frac{1}{5}}\\&+\frac{1}{5}\Big((x_0+a_1x_2+a_2x_3+a_3x_4+a_4x_1)^5\Big)^{\frac{1}{5}}\\&+\frac{1}{5}\Big((x_0+a_1x_3+a_2x_4+a_3x_1+a_4x_2)^5\Big)^{\frac{1}{5}}\\&+\frac{1}{5}\Big((x_0+a_1x_4+a_2x_1+a_3x_2+a_4x_3)^5\Big)^{\frac{1}{5}},\\=\frac{1}{5}\Sigma x&+\frac{1}{5}\Big(\mathrm{P}_1^5\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(\mathrm{P}_2^5\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(\mathrm{P}_3^5\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(\mathrm{P}_4^5\Big)^{\frac{1}{5}}\\=\frac{1}{5}\Sigma x&+\frac{1}{5}\Big(\mathrm{S}+\mathrm{H}_1\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(\mathrm{S}+\mathrm{H}_2\Big)^{\frac{1}{5}}\\&+\frac{1}{5}\Big(\mathrm{S}+\mathrm{H}_3\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(\mathrm{S}+\mathrm{H}_4\Big)^{\frac{1}{5}};\end{aligned}$$

where S is a symmetric function of the roots of F=0, and $\mathrm{H}_1\ \mathrm{H}_2\ \mathrm{H}_3\ \mathrm{H}_4$ are four values of a 24-valued function of these roots. $\mathrm{H}_2$ is formed from $\mathrm{H}_1$ by the substitution $\begin{smallmatrix}0&2&3&4&1\\0&1&2&3&4\end{smallmatrix}$, effected on the subindices of the $x$'s, leaving undisturbed the $a\ a^2$, &c.; $\mathrm{H}_3$ from $\mathrm{H}_1$ by the substitution $\begin{smallmatrix}0&3&4&1&2\\0&1&2&3&4\end{smallmatrix}$, and $\mathrm{H}_4$ from $\mathrm{H}_1$ by $\begin{smallmatrix}0&4&1&2&3\\0&1&2&3&4\end{smallmatrix}$. $\mathrm{H}_1$ is unaltered by the cyclical permutation of $x_0\ x_1\ x_2\ x_3\ x_4$.

Another set of four values, $\mathrm{H}_5\ \mathrm{H}_6\ \mathrm{H}_7\ \mathrm{H}_8$, can be formed by operating on $\mathrm{H}_1\ \mathrm{H}_2\ \mathrm{H}_3\ \mathrm{H}_4$ by the substitution $\begin{smallmatrix}0&1&3&4&2\\0&1&2&3&4\end{smallmatrix}$, or, which is the same thing, by operating on $\mathrm{H}_1$ only, by the four substitutions 01342, 03421, 04213, 02134, omitting the suffixed 01234. And if we write in the above expression for $x_0$, $\mathrm{H}_5\ \mathrm{H}_6\ \mathrm{H}_7\ \mathrm{H}_8$ in the places of $\mathrm{H}_1\ \mathrm{H}_2\ \mathrm{H}_3\ \mathrm{H}_4$, we shall have still a correct expression of the value of $x_0$. Is not this a sufficient and a new demonstration of the absurdity of supposing a solution of the quintic?

Four more sets each of four values of $\mathrm{H}_1$ can be made by operating on $\mathrm{H}_1\ \mathrm{H}_2\ \mathrm{H}_3\ \mathrm{H}_4$, by the four substitutions, 01423, 01243, 01432, 01324; and the sum of the 24 values of $\mathrm{H}_1$, thus obtained in sets of four, will be that of all the values that $H_1$ can have by permutation of the $x$'s, leaving untouched the $\alpha\,\alpha^2$, &c. We thus obtain six distinct expressions for $x_0$, of which no two contain a common value of the 24-valued $H_1$. All this is true if $x_0=\phi t_0$, $x_1=\phi t_1$, $x_2=\phi t_2$, &c., $H_1$ being a function of the $\phi t$'s, and S also.

It is plain that we can form the equation

$$y^{24}+ay^{23}+by^{22}+\text{ \&c.} \qquad =0=U,$$

whose roots are these 24 values of $H_1$, and that $a$, $b$, &c., in this equation will be rational functions of the coefficients of $F=0$. If we knew how to solve this equation, $U=0$, we should have the correct solution of the quintic $F=0$, by writing, instead of $H_1\,H_2\,H_3\,H_4$, the algebraic root of $U=0$, under each of the four quintic radical signs. The ambiguity arising from the different roots of unity read with all the radicals would of course remain, as in the lower equations.

I see but two conceivable chances of success, either that $U=0$ should break up readily into six biquadratics, or that it should be somehow reducible to the form

$$y^{24}+Ky^{18}+Ly^{12}+My^{6}+N=0=U^1,$$

or to some other biquadratic in $z$, $z$ being a cubic in $y^2$.

Such reduction would be possible if we had a sufficient number of constants at our disposal in $x_0\,x_1\,x_2\,x_3\,x_4$. Let us then suppose that, instead of the general quintic $F=0$, we have a quintic $F^1=0$ to solve, whose roots are $\phi x_0$, $\phi x_1$, $\phi x_2$, $\phi x_3$, $\phi x_4$, $x_0\,x_1$ &c. being the roots of $F=0$, and

$$\phi x=hx^4+kx^3+lx^2+mx+n,$$

$h\;k\;l\;m\;n$ being undetermined quantities, surd or rational. We can write

$$\phi x_0=\frac{1}{5}\Sigma\phi x+\frac{1}{5}\Big(S+H_1\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(S+H_2\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(S+H_3\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(S+H_4\Big)^{\frac{1}{5}}$$

where S is a symmetrical, and $H_1\ H_2\ H_3\ H_4$ are values of a 24-valued, function of the $\phi x$'s. We can form, as before, the equation $U=0$, whose roots are the 24 values of this $H_1$ and in all whose coefficients the constants $h\;k\;l\;m\;n$ are at our disposal. If by solutions of equations among these quantities, of degrees below the fifth, we can reduce U to the form $U^1$, we have $\phi x_0$ by putting for $H_1$ $H_2$ $H_3$ $H_4$ the given algebraic expressions

$$Y_1^{\frac{1}{6}},\ Y_2^{\frac{1}{6}},\ Y_3^{\frac{1}{6}},\ Y_4^{\frac{1}{6}};$$

$Y_1$, &c., being the four roots of $U^1=0$. The roots of $F=0$ would be then given by five biquadratics, *h k l m n* being now determined functions of the coefficients of $F=0$ through those of $F^1=0$,

$$\phi x_0=C_0,\ \phi x_1=C_1,\ \phi x_2=C_2,\ \phi x_3=C_3,\ \phi x_4=C_4;$$

and Dr. Hargreave's device would bear the coveted fruit.

Without expressing any opinion about the possibility of this transformation, I can shew that the equation $U=0$ is farther conditioned beyond what has been seen concerning the arrangement of its roots in sets of four. We formed at first $H_1$ $H_2$ $H_3$ $H_4$ on the first of the following groups:—

| | | | |
|---|---|---|---|
| 1234 | 1234 | 1234 | 1234 |
| 2341 | 3142 | 4312 | 3412 |
| 3412 | 4321 | 2143 | 4321 |
| 4123 | 2413 | 3421 | 2143 |

If we had used the second instead of the first group, we should have formed $H_1$ $H_2^1$ $H_3^1$ $H_4^1$, of which $H_1$ only is in the former set of four: if we had used the third, we should have formed $H_1$ $H_2^{11}$ $H_3^{11}$ $H_4^{11}$, all fresh values, except $H_1$: if we had employed the fourth, we should have formed $H_1$ $H_4$ $H_3^1$ $H_3^{11}$, all before written. Thus we see that $U=0$ is something very different from the general equation of its degree. It may possibly break up into quadratics, by introducing surds into the coefficients.

Whichever group we employ, its five derivates give us five more expressions of $x_0$; that is, there are 24 ways of expressing $x_0$, exhibiting 24 different quadruplets from the 24 values of $H_1$.

I fear much that all this is rather a proof of the absurdity of attempting, than a contribution towards effecting, the algebraic resolution of the quintic. But it may be worth the while of some one, endued with more faith, to form and to study this $U=0$ of the 24th degree. He would deserve well of science who could shew us how to form it by a compendious method.

When there are only four roots, $x_0$ $x_1$ $x_2$ $x_3$, we obtain, if we choose needlessly to introduce three biquadratic instead of the usual three quadratic radicals free from $\sqrt{-1}$, after $\Sigma x$,

$$4x_0 = \Sigma x + \left(S + H_1\right)^{\frac{1}{4}} + \left(S + H_2\right)^{\frac{1}{4}} + \left(S + H_3\right)^{\frac{1}{4}},$$

where $S = \Sigma x^4 + 24 \cdot x_0 x_1 x_2 x_3 - 6 \Sigma x^2 x^2$, and

$$\begin{aligned} H_1 &= 12(x_0^2 x_2^2 + x_1^2 x_3^2) - 4\left(x_0 x_2(x_0^2 + x_2^2) + x_1 x_3(x_1^2 + x_3^2)\right) \\ &\quad + 4\sqrt{-1}\left(x_0^3(x_1 - x_3) + x_1^3(x_2 - x_0) + x_2^3(x_3 - x_1) + x_3^3(x_0 - x_2)\right), \\ &= h + 4k, \end{aligned}$$

$h$ being a three-valued, and $k$ a six-valued function, whose square $k^2$ is three-valued. We can form with the coefficients of $F=0$ the two cubics whose roots are the values of $h$ and the values of $k^2$. If their algebraic roots be $Y_1$ $Y_2$ $Y_3$, and $Z_1$ $Z_2$ $Z_3$, we have, for the root of the biquadratic

$$F = (x - x_0)(x - x_1)(x - x_2)(x - x_3) = 0,$$

$$x_0 = \frac{1}{4}\Sigma x + \frac{1}{4}\left(S + Y_1 + 4Z_1^{\frac{1}{2}}\right)^{\frac{1}{4}} + \frac{1}{4}\left(S + Y_2 + 4Z_2^{\frac{1}{2}}\right)^{\frac{1}{4}} + \frac{1}{4}\left(S + Y_3 + 4Z_3^{\frac{1}{2}}\right)^{\frac{1}{4}}.$$

To transform into this shape the ordinary solution does not seem easy.

It is obvious that $x_0$ can be represented in like manner in terms of $x_0$ $x_1$ $x_2 \cdot\cdot$ $x_{n-1}$ by an expression which is true when for $x_0$ is put any one of the $n-1$ others. Nothing more is required for the algebraic solution of the general equation of the $n$th degree, than to break up the given equation $U=0$ of the degree $(n-1)\cdot(n-2)\cdot\cdot 2 \cdot 1$ into factors of the $n-1$th degree. But the inexorable theory of groups forbids me to hope for this reduction of $U=0$, when $n$, the number of roots, exceeds four.

Has it been before pointed out, that the problem of algebraic resolution of equations is merely that of elimination of imaginaries by involution into symmetrical functions under radical signs?

## "On the Origin of Colour and the Theory of Light," Part II., by J. SMITH, M.A., Perth Academy.

On a former occasion the Author proved experimentally that colour can be produced by the mere motion of a white ray without the aid of refraction; his object now was to explain those physical laws by which colour is produced from a homogeneous substance.

He argued that as we hear through the medium of the air—a homogeneous substance—and the variety of sounds is equal to the variety of colours, we may have as great a variety of colour from the ether considered as homogeneous as of sounds from the air so considered.

The difficulty was to prove by what process nature modifies so wonderfully the motion of a homogeneous substance. Many years ago he found that by pressing a piece of glass with a point he obtained colours. If the glass pressed were laid on an irregular surface, as a piece of ivory or steel, the colours were in irregular patches; but if the glass were laid on another piece of glass, the circles were complete.

This proved that Newton's rings, as they are called, were produced by undulations or waves in the glass, that these waves are virtually small prisms, and that they are stationary as long as the pressure is stationary.

By and by he found that similar waves or bands exhibited themselves in a film of soap when made to cover the mouth of a tumbler, and that they were particularly interesting and instructive when the tumbler was held at an angle to the horizon. He also found that by blowing through a tube on a plate of glass, a converse series of rings was obtained. And as a proof that these rings were formed of waves, we had merely to examine their motions—the motion of the one series was the converse of the other.

Again, he took a thin disc of glass and laid it on another piece of glass of any thickness, and subjecting it to pressure and then cementing the pieces together at the edges, the rings were made permanent.

He also showed how the parallel bands of colour could be imitated by prismatic refraction of white bands of the same ratio as to breadth.

The author then applied these laws to explaining the phenomena of the prism.

He argued that all matter was put in motion by light, just as we are sensible that every atom of a body is moved by heat. He showed, not only by diagrams but mechanically, the physical laws by which light was refracted and reflected, and expounded, as he considered, the true theory of prismatic refraction.

As — how a ray of light was always refracted towards the base of a prism, and how it could not be physically otherwise. Then from these laws he concluded — Given the molecular vibration of matter, given the dispersion of light.

He consequently arrived at the conclusion that light no more passed through glass than does sound. If we could withdraw the ether from a glass globe, we could no more see through the glass than we could hear through it if the air were removed.

As to the ether being heterogeneous, we do not say the atmosphere is heterogeneous because one sound differs from another.

Why should light, then, differ from sound? Why compound, when the same uncompounded air can convey as great a variety of notes as there are shades or varieties of colours? A prism no more analyses light or the ether in motion, than an Æolian harp analyses sound, or the air in motion, for light and sound are both merely modes of motion of two different elements.

As a magnet conveys motions of one kind, so glass conveys motions of another, the one not more wonderful than the other.

We surely consider the molecules of the magnet to vibrate. Why not the molecules of glass, and consequently of all matter refracting light?

A telescope is but another kind of telegraph, only much more comprehensive in its range.

## Ordinary Meeting, March 31st, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

### "On the Solution of Algebraic Equations," by the Rev. THOS. P. KIRKMAN, M.A., F.R.S.

I showed in the preceding number of these Proceedings that if $x_0$ $x_1$ $x_2$ $x_3$ $x_4$ are the roots of the quintic $F=0$, we can write,

$$x_0=\frac{1}{5}\Sigma x+\frac{1}{5}\Big(S+H_1\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(S+H_2\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(S+H_3\Big)^{\frac{1}{5}}+\frac{1}{5}\Big(S+H_4\Big)^{\frac{1}{5}},$$

where S is a symmetrical function of the roots, and $H_1$ $H_2$ $H_3$ $H_4$ are four values of a 24-valued function of those roots. I can now show how to form these four values with the coefficients of $F=0$; *i.e.* how to solve the famous quintic by a method which leads directly to the solution of all algebraic equations. We write the complete group of 24 substitutions among four elements thus:

| | | | | | |
|---|---|---|---|---|---|
| 1234 | 1342 | 1423 | 1243 | 1324 | 1432 |
| 2143 | 3124 | 4123 | 2134 | 3142 | 4123 |
| 3412 | 4213 | 2314 | 4312 | 2413 | 3214 |
| 4321 | 2431 | 3241 | 3421 | 4231 | 2341 |
| A | B | C | D | E | F |

The 12 substitutions over A B C are all what we call *positive*, that is, they are made by an even number of transpositions; the 12 over D E F are all *negative*, that is, made by an odd number of transpositions. Thus 1243 is made by the transposition of 4 and 3; 2341 by the three transpositions 21, 31, 41. The systems (A+B+C) and (D+E+F) are changed one into the other by any odd number, and remain both unchanged by any even number, of transpositions.

The entire number of substitutions possible with *r* elements can be written down in like manner in a sum of two systems, which both remain invariable by any even number, and are changed the one into the other by any odd number, of transpositions.

$H_1$ in the above expression for $x_0$ is invariable by the cyclical permutation of $x_0\ x_1\ x_2\ x_3\ x_4$, and therefore receives all its possible values by permutations of $x_1\ x_2\ x_3\ x_4$. $H_2$ is formed by effecting on $H_1$ the substitution 2143, $H_3$ by 3412, and $H_4$ by 4321, performed on the subindices of $x_1\ x_2\ x_3\ x_4$ in $H_1$.

Our object is to construct the values of $H_1\ H_2\ H_3\ H_4$ in terms of the coefficients of $F=0$.

The functions $H_1^i\ H_2^i\ H_3^i\ H_4^i$ can all be formed for any integer index $i$. Let

$$
\begin{array}{c}
H_1^i + H_2^i + H_3^i + H_4^i = (A)_i \\
H_5^i + H_6^i + H_7^i + H_8^i = (B)_i \\
\cdot \qquad\qquad\qquad\qquad \cdot \\
\cdot \qquad\qquad\qquad\qquad \cdot \\
\cdot \qquad\qquad\qquad\qquad \cdot \\
H_{21}^i + H_{22}^i + H_{23}^i + H_{24}^i = (F)_i
\end{array}
$$

where the four functions $(B)_i$ are formed by effecting on $H_1^i$ the four substitutions written over B, &c.; or, which is the same thing, by raising to the $i$th power $H_5\ H_6\ H_7$ and $H_8$, made from $H_1$ by the four substitutions over B, &c.

It is plain that

$$(A)_i+(B)_i+(C)_i+(D)_i+(E)_i+(F)_i \quad = M_i$$

and

$$\Big((A)_i+(B)_i+(C)_i\Big)\Big((D)_i+(E)_i+(F)_i\Big)=N_i$$

are both given symmetrical functions of the roots, and rational in the coefficients of the quintic $F=0$; for the former is the sum of all the 24 values of $H_1^i$, and the latter is the product of two factors, either of which becomes the other by any odd number of transpositions of $x_1\ x_2\ x_3\ x_4$, and neither of which is changed by any even number of such transpositions.

Hence $(A)_i+(B)_i+(C)_i$ is the root of a given quadratic, whose coefficients are rational functions of those of the given quintic.

Next, the functions

$$\left\{(A)_i(B)_i+(A)_i(C)_i+(B)_i(C)_i\right\}+\left\{(D)_i(E)_i+(D)_i(F)_i+(E)_i(F)_i\right\}=P_i,$$

$$\left\{(A)_i(B)_i+(A)_i(C)_i+(B)_i(C)_i\right\}\left\{(D)_i(E)_i+(D)_i(F)_i+(E)_i(F)_i\right\}\quad=Q_i,$$

are rational functions of the coefficients of the quintic $F=0$; for the former is the sum, and the latter is the product, of two functions of $x_1\,x_2\,x_3\,x_4$, of which either is changed into the other by any odd number of transpositions of those variables, while neither is changed by any even number of such transpositions.

Wherefore

$$(A)_i(B)_i+(A)_i(C)_i+(B)_i(C)_i$$

is the root of a given quadratic, whose coefficients are rational functions of those of the given quintic.

Finally,

$$(A)_i(B)_i(C)_i+(D)_i(E)_i(F)_i\quad=R_i \text{ and}$$

$$\left\{(A)_i(B)_i(C_i)\right\}\left\{(D)_i(E)_i(F)_i\right\}=S_i$$

are both rational functions of the coefficients of the quintic $F=0$, for the reasons above given, and $(A)_i(B)_i(C)_i$ is again the root of a given quadratic.

Hence we can form the cubic whose roots are $(A)_i$, $(B)_i$, and $(C)_i$, whatever be $i$; for its coefficients are given irrational functions of the coefficients of $F=0$ with ambiguous radicals.

Forming, then, and solving these cubics, for $i=1$, $i=2$, $i=3$, $i=4$, we obtain $(A)_i=V_i$, $V_i$ being a given irrational function of the coefficients with ambiguous radicals. That is, we have

$$H_1+H_2+H_3+H_4=V_1,$$
$$H_1^2+H_2^2+H_3^2+H_4^2=V_2,$$
$$H_1^3+H_2^3+H_3^3+H_4^3=V_3,$$
$$H_1^4+H_2^4+H_3^4+H_4^4=V_4,$$

By the aid of these equations we can form the quartic whose roots are $H_1$, $H_2$, $H_3$, and $H_4$, by a well-known method; and, as we can also solve this quartic, we can write its four roots $(H_1)$, $(H_2)$, $(H_3)$, $(H_4)$, all given irrational functions of the co-efficients of $F=0$, in the above expression for $x_0$, in the places of $H_1$, $H_2$, $H_3$, and $H_4$; and thus we have the long-sought algebraic root of the given quintic $F=0$.

We cannot always form a positive and transitive group of $n-1$, corresponding to our group over A; but the preceding reasoning applies with little change to the solution of the general equation of the $n$th degree. When $n$ is a power of a prime number, the process will be somewhat abridged. Nothing prevents us from breaking up the function $H_1$ above handled into a number of separate functions, which will each contribute a quartic root to the solution, and thus most probably simplify the computation.

To solve the general sextic we proceed thus:—Let $x_0\,x_1\,x_2\,x_3\,x_4\,x_5$ be the roots of the sextic, G=0. We can write

$$x_0=\frac{1}{6}\Sigma x+\frac{1}{6}\left(\{x_0-x_3+a(x_1-x_4)+a^2(x_2-x_5\}^6\right)^{\frac{1}{6}}$$

$$+\frac{1}{6}\left(\{x_0-x_4+a(x_2-x_5)+a^2(x_3-x_1\}^6\right)^{\frac{1}{6}}$$

$$+\frac{1}{6}\left(\{x_0-x_5+a(x_3-x_1)+a^2(x_4-x_2)\}^6\right)^{\frac{1}{6}}$$

$$+\frac{1}{6}\left(\{x_0-x_1+a(x_5-x_2)+a^2(x_5-x_3)\}^6\right)^{\frac{1}{6}}$$

$$+\frac{1}{6}\left(\{x_6-x_2+a(x_6-x_3(+a_2(x_1-x_4)\}^6\right)^{\frac{1}{6}}$$

$$=\frac{1}{6}\Sigma x+\frac{1}{6}\left(S+H_1\right)^{\frac{1}{6}}+\frac{1}{6}\left(S+H_2\right)^{\frac{1}{6}}+\frac{1}{6}\left(S+H_3\right)^{\frac{1}{6}}$$

$$+\frac{1}{6}\left(S+H_4\right)^{\frac{1}{6}}+\left(S+H_5\right)^{\frac{1}{6}},$$

where $a$ is an imaginary cube root of unity, and S is symmetrical in the roots and rational in the coefficients of G=0, and where $H_1\ H_2\ H_3\ H_4\ H_5$ are five values of a 120-valued function of those roots. Our object is to transform $H_1$, &c., into irrational functions of the coefficients of G=0.

Since $H_1\ H_2$, &c., *after* involution, are invariable by the cyclical permutations of $x_0\,x_1\,x_2\,x_3\,x_4\,x_5$, they receive all their possible values by the permutations of $x_1\ x_2\ x_3\ x_4\ x_5$.

The positive substitutions composed of five elements are as follows:—

$$\left\{\begin{array}{cccc} j_1 & k_1 & l_1 & m_1 \\ 12345 & 21435 & 34125 & 43215 \\ 23451 & 14352 & 41253 & 32154 \\ 34512 & 43521 & 12534 & 21543 \\ 45123 & 35214 & 25341 & 15432 \\ 51234 & 52143 & 53412 & 54321 \end{array}\right\} x_1,$$

$$\left\{\begin{array}{cccc} j_2 & k_2 & l_2 & m_2 \\ 23145 & 32415 & 14235 & 41325 \\ 31452 & 24153 & 42351 & 13254 \\ 14523 & 41532 & 23514 & 32541 \\ 45231 & 15324 & 35142 & 25413 \\ 52314 & 53241 & 51423 & 54132 \end{array}\right\} x_2,$$

$$\left\{\begin{array}{cccc} j_3 & k_3 & l_3 & m_3 \\ 31245 & 13425 & 24315 & 42135 \\ 12453 & 34251 & 43152 & 21354 \\ 24531 & 42513 & 31524 & 13542 \\ 45312 & 25134 & 15243 & 35421 \\ 53124 & 51342 & 52431 & 54213 \end{array}\right\} x_3.$$

The 60 negative substitutions are obtained by making the transposition 21 for 12 in all the preceding.

By

$$x_1 = j_1 + k_1 + l_1 + m_1$$
$$x_2 = j_2 + k_2 + l_2 + m_2$$
$$x_3 = j_3 + k_3 + l_3 + m_3$$

we mean to say that $x_1$ is the sum of the four subgroups written under $j_1$ $k_1$ $l_1$ and $m_1$, with a like signification for $x_2$ and $x_3$. And by $-x_1 = -j_1 + (-k_1) + (-l_1) + (-m_1)$, &c., we mean that $-x_1$, $-x_2$. $-x_3$, and $-j_1$, &c., are what $x_1$, $x_2$, $x_3$, and $j_1$, &c., become by the transposition 21 for 12 made alike through their sums of substitutions.

By

$$H_1^i + H_2^i + H_3^i + H_4^i + H_5^i = {}_1J_i$$
$$H_6^i + H_7^i + H_8^i + H_9^i + H_{10}^i = {}_1K_i$$
$$H_{11}^i + H_{12}^i + H_{13}^i + H_{14}^i + H_{15}^i = {}_1L_i$$
$$H_{16}^i + H_{17}^i + H_{18}^i + H_{19}^i + H_{20}^i = {}_1M_i,$$
$$H_{21}^i + H_{22}^i + H_{23}^i + H_{24}^i + H_{25}^i = {}_2J_i$$
$$\vdots \qquad\qquad\qquad\qquad \vdots$$

$$
\begin{array}{l}
H^i_{36} + \quad . \quad . \quad . \quad . \quad . = {}_2M_i \\
. \qquad\qquad\qquad\qquad\quad . \\
. \qquad\qquad\qquad\qquad\quad . \\
. \qquad\qquad\qquad\qquad\quad . \\
. \qquad\qquad\qquad\qquad\quad . \\
H^i_{56} + \quad . \quad . \quad . \quad . \quad + H^i_{60} = {}_3M_i ,
\end{array}
$$

I mean that ${}_1J_i$ is the sum of the *i*th powers of the functions obtained by effecting, on the subindices of $x_1 x_2 x_3 x_4 x_5$ in $H_1$, the substitutions written under $j_1$, ${}_2J_i$ being the sum of the five functions made by operating on $H^i_1$ by the substitutions under $j_2$, &c. We form thus 60 values of $H^i_1$ with the positive, and 60 more with the negative, substitutions.

By
$$
\begin{array}{l}
{}_1J^h_i + {}_1K^h_i + {}_1L^h_i + {}_1M^h_i = {}_1X_{hi} \\
{}_2J^h_i + {}_2K^h_i + {}_2L^h_i + {}_2M^h_i = {}_2X_{hi} \\
{}_3J^h_i + {}_3K^h_i + {}_3L^h_i + {}_3M^h_i = {}_3X_{hi}
\end{array}
$$

I mean to say that ${}_1X_{hi}$ is the sum of the *h*th powers of the four functions ${}_1J_i$ &c.

It is evident that $H^i_1$, $H^i_2$, &c., can be formed for any integer index *i*, and as well as ${}_1J^h_i$, &c., for any integer *h*.

I denote by $H^i_{-1}$, $H^i_{-2}$, &c., and by ${}_{-1}J^h_i$, on ${}_{-1}K^h_i$, &c., and by ${}_{-1}X_{hi}$, &c., the results of operating on $H^i_1$, $H^i_2$, &c., on ${}_1J^h_i$, &c., and on ${}_1X_{hi}$, &c., by the substitution 2134, effected on the subindices of $x_1 x_2 x_3 x_4 x_5$ throughout the function considered.

It is evident that

$$({}_1X_{hi} + {}_2X_{hi} + {}_3X_{hi}) + ({}_{-1}X_{hi} + {}_{-2}X_{hi} + {}_{-3}X_{hi})$$

and
$$({}_1X_{hi} + {}_2X_{hi} + {}_3X_{hi})({}_{-1}X_{hi} + {}_{-2}X_{hi} + {}_{-3}X_{hi})$$

are both symmetrical functions of the roots, and rational functions of the coefficients, of $G=0$; as may be readily seen by making $h=i=1$, and observing that the involutions cannot destroy the symmetry in the roots, or the rationality in the coefficients of $G=0$; for the former is the sum, and the latter the product of two functions, either of which becomes the other by any odd number of transpositions of the subindices of $x_1 x_2 x_3 x_4 x_5$, while neither is altered by any even number of such transpositions Wherefore

$${}_1X_{hi} + {}_2X_{hi} + {}_3X_{hi}$$

is the root of a given quadratic, whose coefficients are rational functions of those of $G=0$.

For the very same reason
$({}_1X_{hi}\cdot{}_2X_{hi}+{}_1X_{hi}\cdot{}_3X_{hi}+{}_2X_{hi}\cdot{}_3X_{hi})+({}_{-1}X_{hi}\cdot{}_{-2}X_{hi}+{}_{-1}X_{hi}\cdot{}_{-3}X_{hi}+{}_{-2}X_{hi}\cdot{}_{-3}X_{hi})$
and
$({}_1X_{hi}\cdot{}_2X_{hi}+{}_1X_{hi}\cdot{}_3X_{hi}+{}_2X_{hi}\cdot{}_3X_{hi})({}_{-1}X_{hi}\cdot{}_{-2}X_{hi}+{}_{-1}X_{hi}\cdot{}_{-3}X_{hi}+{}_{-2}X_{hi}\cdot{}_{-3}X_{hi})$
are rational functions of the coefficients of G=0, and

$${}_1X_{hi}\cdot{}_2X_{hi}+{}_1X_{hi}\cdot{}_3X_{hi}+{}_2X_{hi}\cdot{}_3X_{hi}$$

is the root of such another given quadratic. Also

$${}_1X_{hi}\cdot{}_2X_{hi}\cdot{}_3X_{hi}+{}_{-1}X_{hi}\cdot{}_{-1}X_{hi}\cdot{}_{-3}X_{hi},\text{ and}$$

$${}_1X_{hi}\cdot{}_2X_{hi}\cdot{}_3X_{hi}\cdot{}_{-1}X_{hi}\cdot{}_{-2}X_{hi}\cdot{}_{-3}X_{hi}$$

are rational in the coefficients of G=0, for the same reason; and

$${}_1X_{hi}\cdot{}_2X_{hi}\cdot{}_3X_{hi}$$

is the root of such another given quadratic,

We can now form the cubic whose roots are ${}_1X_{hi}$, ${}_2X_{hi}$ and ${}_3X_{hi}$, an equation having for coefficients given irrational functions of the coefficients of G=0, in which every radical has a determined sign. Let $({}_1X_{hi})$ , $({}_2X_{hi})$ $({}_3X_{hi})$ be the algebraic roots of this cubic.

By forming and solving this cubic for the values $h=1$, $h=2$, $h=3$, $h=4$, we get, and we are content to get, from each of the four cubics, its one algebraic root $({}_1X_{hi})$, in which there is no ambiguity in the radicals. That is, we obtain

$$({}_1X_i)={}_1J_i+{}_1K_i+{}_1L_i+{}_1M_i$$
$$({}_1X_{2i})={}_1J_i^2+{}_1K_i^2+{}_1L_i^2+{}_1M_i^2$$
$$({}_1X_{3i})={}_1J_i^3+{}_1K_i^3+{}_1L_i^3+{}_1M_i^3$$
$$({}_1X_{4i})={}_1J_i^4+{}_1K_i^4+{}_1L_i^4+{}_1M_i^4,$$

where the left members are all given irrational functions of the coefficients of G=0, in which every radical has a determined sign.

From these equations given for the five values (1, 2, 3, 4, 5) of $i$, we form easily for every such value the quartic whose roots are ${}_1J_i$, ${}_1K_i$, ${}_1L_i$, and ${}_1M_i$. This quartic will have for coefficients given irrational functions of the coefficients of G=0, every radical having a determinate sign.

Forming now and solving these quartics for the first five values of $i$, we obtain from each for the one root ${}_1J_i$ an irrational function $({}_1J_i)$ of the given coefficients of the sextic G=0, in which every radical has a determined sign. That is, we obtain

$$({}_1J_1)=H_1+H_2+H_3+H_4+H_5,$$
$$({}_1J_2)=H_1^2+H_2^2+H_3^2+H_4^2+H_5^2,$$
$$({}_1J_3)=H_1^3+H_2^3+H_3^3+H_4^3+H_5^3,$$
$$({}_1J_4)=H_1^4+H_2^4+H_3^4+H_4^4+H_5^4,$$
$$({}_1J_5)=H_1^5+H_2^5+H_3^5+H_4^5+H_5^5,$$

With these data we can form the quintic whose roots are $H_1$ $H_2$ $H_3$ $H_4$ and $H_5$. Its coefficients will be given irrational functions of the coefficients of $G=0$ formed from the definite five $({}_1J_i)$, in which every radical will have a determined sign.

Solving this quintic, and substituting its five roots $(H_1)$, $(H_2)$, &c., for $H_1$ $H_2$ $H_3$ $H_4$, $H_5$ in our expression for $x_0$, we have the algebraical solution of the general sextic $G=0$: and every one of $x_0$ $x_1$ $x_2$ $x_3$ $x_4$ $x_5$ is definitely given by the proper reading of the cube roots of unity.

By a like process can every algebraic equation be solved, the $n$th degree after the $(n-1)$th.

"Description of a Dolerite at Gleaston, in Low Furness," by E. W. BINNEY, F.R.S., F.G.S.

During the last 30 years the tract of land known as the Hundred of Low Furness has been investigated and described by several geologists. It was one of the earliest fields investigated by the venerable Sedgwick, who has left us a most valuable memoir of his labours in that district. Since then, Mr. Jopling, myself, and Sir R. I. Murchison, and Professor Harkness have published descriptions of the silurian mountain limestone and permian formations of the country. Miss E. Hodgson has also given us information as to the drift deposits overlying the palœozoic strata. Still, notwithstanding what has been done, it may confidently be asserted that the peninsula comprising the southern part of the Hundred of Low Furness has yet to be carefully examined before its geology can be said to be thoroughly known.

None of the above-named persons appear to have been aware of the occurrence of any trap dykes in this district, judging from their published writings, with the exception of Mr. Jopling, who, in his sketch of the geology of Low Furness and Cartmel, comprehending the Hundred of Lonsdale north of the sands, published in 1843, when speaking of the geology of Gleaston, says:—"Carboniferous limestone abounds, and in the quarries near the castle are many fossils beautifully preserved in the shale beds between those of the limestone; there is also a vein of trap." At page 72, the same author says "there are also appearances of trap near Gleaston, associated with limestone breccia."

In the month of October last, Miss E. Hodgson was so kind as to send me some specimens of rocks from Gleaston, which puzzled her a good deal. Some of the parties to whom she had sent them called them dolomites, whilst others named them traps and greenstones. To the latter opinion Miss Hodgson, I believe, was inclined to add the weight of her sanction. Not having previously seen, or even heard of, the occurrence of any such rocks in the district where they were said to be met with, I went over to examine them, and having been furnished with information by Miss Hodgson, easily found the place where they are exposed at Gleaston Green. At that time Mr. Jopling's book had not been seen by me. The space occupied by these singular rocks, at least so far as at present exposed, is so limited that all that can be seen is very soon ascertained. Specimens were collected, and a few observations made. The former, by the kindness of my friend Professor Roscoe, F.R.S., were analyzed for me in the laboratory of Owens College. It is only by the labours of the chemist that geologists can with any certainty decide upon the age and origin of such rocks as those which are met with at Gleaston.

On approaching Gleaston Green from Scales, the mountain limestone appears to occupy the country so far as it can be seen. In a quarry below the old castle on the roadside, this rock in the northern part is very hard, and dips to the west at an angle of 25°, whilst in the southern part, where it is softer, it dips in the same direction at an angle of 16°. Owing to the covering of drift, the limestone is not seen nearer to the mill, but it probably extends further in that direction. At a short distance below the mill, dark-coloured laminated shales are seen in the bank on the roadside, dipping apparently at an angle to the N.N.W. We then come to the rocks at the end of the Green. They appear to run in an east and west direction, and are not now exposed for more than twenty yards. From north to south they may probably extend about forty yards, but certainly for more than half of that distance, towards the beck, they are not now seen until the land rises on the bluff south of the beck, where they reappear as a reddish and bedded trap ash, having an east and west direction, and dipping N.N.W. at an angle of about 60°. This ash is succeeded by a coarse breccia of a few yards in thickness, so far as exposed, which dips slightly north of west, at an angle of 25°, and then is covered up by grass so as not to be seen, but no doubt, from sections in the adjoining lane and borings made on the rise of the strata, dark-coloured shales occur again, and the dyke most probably intrudes through these shales, which are in every respect like limestone shales, but no organic remains were observed in them so as to make us certain of their geological age.

Returning to the north side of the beck, nothing is exposed of the district west of the hard rocks seen on the Green, owing to the thick covering of drift in that direction; but Mr. Hodgson has proved, by a series of bore-holes, the occurrence of upper permian sandstone, red shale, and limestone shale — the first to the S.W., the second to the west, and the third to the N.W. of Gleaston; and Mr. Ashburner has proved limestone shale and limestone in bores to the E. and N.E. of the locality where the rock is found.

In the bluff on the south side of the stream, as previously stated, the rock appears more like a trap ash of a reddish-brown colour, and exhibits traces of bedding and white lines like carbonate of lime. Immediately adjoining trap ash, and on its rise, occurs the coarse breccia composed of fine-grained siliceous rocks, cemented together with quartz, and like a permian breccia; but although the beds are near together, there was not evidence to show whether the trap ash gradually passed into the breccia or intruded through it, still the breccia appeared to dip in the same direction, but at a much less angle, namely, 25° to a little west of north. This is a very interesting fact to prove; for if the rock graduates into the breccia, it would appear to be of permian age, and most probably a melaphyr, but if it is intrusive, as the evidence on the whole appears to prove, all we can say is that it is of later date.

This breccia is composed of angular pieces of a fine siliceous stone, of a pink colour, more resembling quartzite than anything else cemented together by small quartz crystals, and containing minute quantities of protoxide of manganese. The form of the fragments is very like that of the rocks in the permian breccia of Rougham Point, near the mountain limestone of Humfray Head, and there consisting for the most part of the neighbouring mountain limestone, but no limestone has yet been met with in the Gleaston breccia as might have been reasonably expected; and the pink quartzite is a rock hitherto unknown in the district. The permian breccia so far as my experience goes, although sometimes containing volcanic ash are composed of the rocks now found in the neighbourhood where they occur, and nearly always vary with the older geological rocks of the district. The composition of the Gleaston breccia makes me hesitate in designating it as permian as it may be some rock altered by the dyke.

The rock in its best state of preservation is remarkably hard, of a reddish brown colour, has a moderately straight fracture, and a pinkish white streak, and its specific gravity is 2·92.

Three average samples of the rock were taken, two from the north and one from the south side of the beck. All of them were more or less decomposed by exposure to air and moisture, but No. 26 much less than Nos. 23 and 24. Professor Roscoe on analysis, found their present chemical composition to be as follows:—

| | South of Stream, No. 23. | North of Stream, No. 24. | North of Stream, No. 26. |
|---|---|---|---|
| Silica | 45·54 | 50·96 | 51·10 |
| Peroxide of Iron | 24·76 | 24·20 | 21·58 |
| Alumnia | 7·70 | 14·48 | 9·40 |
| Lime | 13·84 | 7·32 | 6·24 |
| Magnesia | 0·57 | 0·55 | 1·33 |
| Carbonic Acid | 2·78 | 1·90 | 2·70 |
| Alkalies, Water (by difference) | 4·82 | 0·59 | 7·65 |
| | 100·00 | 100·00 | 100·00 |

The only rock which I know of a similar composition, is a probable variety of green earth, resembling a decomposed pyroxene, described by Macfarlane, in the Canadian Naturalist, New Series, vol. iii., No. 1, page 5, in a paper on the cupriferous bed of Portage, Lake Michigan, which consists of—

| | |
|---|---|
| Silica | 46·48 |
| Alumnia | 17·71 |
| Protoxide of Iron | 21·17 |
| Lime | 9·89 |
| Magnesia trace | —— |
| Alkalies (by difference) | 1·97 |
| Water | 2·78 |

Mr. David Forbes, F.R.S., to whom were forwarded small specimens of the rocks and the above analyses, kindly informed me that the rocks were so much decomposed that it was difficult to pronounce with certainty as to what they were, but he was inclined to think that they were an intrusive dolerite of carboniferous age rather than a melaphyr. The iron had been changed from a protoxide into a peroxide, and the lime had resulted from the decomposition of a lime felspar. As Mr. Forbes had found the presence of titanium united with iron in all the carboniferous dolerites he had examined; I took several ounces of the three samples above given, and having heated them in a crucible so as to convert the iron from a per into a protoxide, and extracted it by a magnet. About half an ounce of this iron was very carefully examined by Mr. Thorpe in Dr. Roscoe's laboratory, especially for titanic acid, and no trace of that substance was found. He used the test with microcosmic salt, having separated iron and silica. The absence of titanium in the rock would lead us to believe that it was of later origin than the carboniferous age, but if traces of that metal had been found, it would not only have settled the question as to the age, but it would have shewn a connexion with the hæmatite iron ores of Whitehaven and Ulverston, all of which contain more or less of titanium as proved by the deposits of that metal found on the sides of old furnaces where hæmatite has been smelted. Is the rock of permian age? It is certainly not much unlike the melaphyr of the German Geologists, and the breccia near the dolerite is not greatly different from that of Ballochmyle, described by Mr. A. Gekie, F.R.S., in the Geological Magazine, for December, 1866, but we could not obtain direct evidence that the breccia gradually passed into the trap, the latter appeared to protrude through it, but certainly the trap and the breccia dipped in the same direction, the one at about 60° and the other at 25° a little West of North. This point can only be satisfactorily determined by cutting a trench and shewing the contact of the breccia with the trap. The extent of the dyke can only be traced for a few yards east and west as previously stated, and none of the hæmatite iron deposits, so far as known, have been found south of it. Its age also appears to be more recent, even supposing it to be permian, than those deposits, which for the most part must be considered of carboniferous age. The occurrence of this trap might have been considered to have some connection with the deposition of the iron had it been of carboniferous age, but it is evidently more recent, and therefore could have had nothing to do with it further than disturb or displace it.

"A Search for Solid Bodies in the Atmosphere," by R. ANGUS SMITH, Ph.D., F.R.S., &c.

I have so frequently for many years attempted to find, and have found, organic substances which have passed from the air into liquids in which they were collected, that perhaps the Society will scarcely attend to another attempt, although it indicates, I think, some progress. It was in the year 1847 that I first collected what I believe was matter from the respiration and perspiration, and found that as it was kept it grew into distinct confirmed forms.

Whilst examining some matters relating to the cattle plague I found one or two remarkable points. I had before that time used aspirators to pass the air through liquids, except in the oxidation experiments. At that time I used simply a bottle which contained a little water. The bottle was filled with the air of the place and the water shaken in it. The difference of air was remarkable. A very few repetitions would cause the liquid to be muddy, and the particles found in many places were distinctly organic.

Lately I tried the same plan on a larger scale. A bottle of the capacity of 4990 c.c. was filled with air and shaken with water. The bottle was again filled and shaken with the same water, and this was repeated 500 times, nearly equal to 2½ million cbc., or 2,495 litres. As this could not be done in a short time, there was considerable variety of weather, but chiefly dry, with a westerly wind. The operation was conducted behind my laboratory, in the neighbourhood of places not very clear, it is true, but from which the wind was blowing to all parts of the town. I did not observe any dust blowing, but if there were dust, it was such as we may be called on to breathe. The liquid was clouded, and the unaided eye could perceive that particles, very light, were floating. When examined by a microscope the scene was varied in a very high degree—there was evidently organic life. I thought it better to carry the whole to Mr. Dancer and to leave him to do the rest, as my knowledge of microscopic forms is so trifling compared to his. It may, however, interest the Society to hear of a few of these previous attempts, the latest made till recently. I shall therefore read from a report to be found in the appendix to that on the cattle plague.

Mr. Crookes also brought me some cotton through which air from an infected place had passed. It was examined at the same time. Taking cotton in the mass nothing decided was seen; but when it was washed some of the separate films were coated over with small nearly round bodies, presenting no structure, or at least only feeble traces of it, and perhaps to be called cells. I had not sent gun-cotton, as I intended, to Mr. Crookes, fearing the rules of the post; otherwise there would have been more certainty that the bodies spoken of did not exist previously on the cotton. However, Mr. Dancer, who has examined cotton with the microscope oftener than most persons, even of those experienced in the subject, had never observed a similar appearance.

The liquid had also a number of similar bodies floating in it.

It was then that Mr. Crookes sent a liquid which he had condensed from the air of an infected cowshed at a space a little above the head of a diseased cow. It was also examined, and it presented similar indications of very numerous small bodies. Not being a professed microscopist I shall not attempt a description, but add that they clearly belonged to the organic world, and were not in all cases mere debris. We found also one body a good deal larger than the rest; it resembled somewhat a paramecium, although clearly not one.

We found no motion whatever, and only this latter substance could be adduced as an absolute proof of anything organized being present. Next day I examined the same liquid; and, whether from the fact of time being given for development or from other causes, there was a very abundant motion. There were at least six specimens in the field at a time, of a body resembling the euglena, although smaller than I have seen it. When these minute bodies occur it is clear that more may exist, and germs in this early stage are too indefinite to be described. The existence of vital sparks in the organic substances in the air alluded to is all I wish to assert, confirming by a different method the observations of others. It might, of course, be said that since the bottle was opened at Mr. Dancer's the air at that place may have communicated them. I answer that, before it was opened, a good glass could detect floating matter, some of it, however, as in the microscope proved, indefinite enough.

Finding this, and fearing that the long time needful to collect liquid from the atmosphere might expose it also to much dust, I used a bottle of about 100 cubic inches dimensions, and putting with it a very little water, not above five cub. centimetres, I pumped out the air of the bottle, allowing the air of the place to enter. This was done six times for each sample, the water shaken each time, and the result examined. This was done with the same bottle that was used in my early experiments with permanganate, and by the same method, except that water instead of that salt was used. At first considerable numbers of moving particles were found; but it was needful to examine the water used, and here occurred a difficulty. It was not until we had carefully treated with chemicals and then distilled the water again and again that we could trust it. Particles seemed to rise with the vapour, and if so, why not with the evaporating water of impure places.

Having kept an assistant at the work for a week, and having myself examined the air of three cow-houses, I came to the conclusion that the air of cow-houses and stables is to be recognized as containing more particles than the air of the street in which my laboratory is, and of the room in which I sit, and that it contains minute bodies, which sometimes move, if not at first, yet after a time, even if the bottle has not been opened in the interval. There is found in reality a considerable mass of debris with hairs or fine fibres, which even the eye, or at least a good pocket lens, can detect. After making about two dozen trials, we have not been able to obtain it otherwise. Even in the quiet office at the laboratory there seemed some indications.

I found similar indications in a cow-house with healthy cows; so I do not pretend to have distinguished the poison of Cattle Plague in these forms; but it is clear that where these exist there may be room for any ferment or fomites of disease; and I do not doubt that one class is the poison itself in its earliest stage. It would be interesting to develope it farther.

I have recorded elsewhere that I condensed the liquid from the air of a flower garden, and found in it, or imagined I found, the smell of flowers. I do not remember that I looked much to the solid or floating particles, thinking them to be blown from the ground, but it does not affect the result, whether they be found constantly in the air or are raised by the action of currents.

"Microscopical Examination of the Solid Particles from the Air of Manchester," by J. B. DANCER, F.R.A.S.

The air had been washed in distilled water, and the solid matter which subsided was collected in a small stoppered bottle, and on the 13th of this month Dr. Smith requested me to examine the matter contained in this water. An illness prevented me from giving it so much attention as I could have wished.

The water containing this air-washing was first examined with a power of 50 diameters only, for the purpose of getting a general knowledge of its contents; afterwards magnifying powers varying from 120 to 1,600 diameters were employed.

During the first observations, few living organisms were noticed; but, as it afterwards proved, the germs of plant and animal life (probably in a dormant condition) were present.

I will now endeavour to describe the objects found in this matter, and begin in the order in which they appeared most abundant.

1st. *Fungoid Matter.*—Spores or sporidiæ appeared in numbers, and, to ascertain as nearly as possible the numerical proportion of these minute bodies in a single drop of the fluid, the contents of the bottle were well shaken, and then one drop was taken up with a pipette; this was spread out by compression to a circle ½ an inch in diameter. A magnifying power was then employed, which gave a field of view of an area exactly 100th of an inch in diameter, and it was found that more than 100 spores were contained in this space; consequently the average number of spores in a single drop would be 250,000. These spores varied from 10,000th to 50,000th of an inch in diameter. The peculiar molecular motion in the spores was observable for a short time, until they settled on to the bottom of the glass plate; they then became motionless.

The Mycelium of these minute fungi were similar to that of rust or mildew (as it is commonly named), such as is found on straw or decaying vegetation.

When the bottle had remained for 36 hours in a room at a temperature of 60° the quantity of fungi had visibly increased and the delicate mycelial thread-like roots, had completely entangled the fibrous objects contained in the bottle and formed them into a mass.

On the third day a number of ciliated zoospores were observed moving freely amongst the sporidiæ. I could not detect any great variety of fungi in the contents of the bottle, but I cannot presume to say that all the visible spores belonged to one species, and as there are more than 2,000 different kinds of fungi it is possible that spores of other species might be present, but not under conditions favourable for their development. Some very pretty chain-like threads of conidia were visible in some of the examinations.

The next in quantity is vegetable tissue. Some of this formed a very interesting object, with a high power, and the greater portion exhibited what is called pitted structure. The larger particles of this had evidently been partially burnt and quite brown in colour, and were from coniferous plants, showing with great distinctness the broad marginal bands surrounding the pits; others had reticulations small in diameter. They reminded me of perforated particles so abundant in some kinds of coal.

The brown or charred objects were probably particles of partially burnt wood used in lighting fires.

Along with these reticulated objects were fragments of vegetation, resembling in structure hay and straw and hay seeds, and some extremely thin and transparent tissue showing no structure. These were doubtless some portions of weather-worn vegetation. A few hairs of leaves of plants and fibres, similar in appearance to flax, were seen, and as might have been expected in this city, cotton filaments, some white, others coloured, were numerous; red and blue being the predominant colours. A few granules of starch, seen by the aid of the polariscope, and several long elliptical bodies, similar to the pollen of the lily, were noticed. After this dust from the atmosphere had been kept quiet for three or four days, animalculæ made their appearance in considerable numbers, the monads being the most numerous. Amongst these were noticed some comparatively large specimens of paramecium aurelia, in company with some very active rotiferæ; but after a few days the animal life rapidly decreased, and in twelve days no animalculæ could be detected.

*Hairs of Animals.*—Very few of these were noticed, with the exception of wool; of this both white and coloured specimens were mixed up along with the filaments of cotton.

After each examination as much of the drop of water as could be collected by the pipette was returned to the bottle, in order to ascertain if any new development of animal or vegetable life would take place, and the stopper of the bottle was replaced as quickly as possible to prevent the admission of the particles from the air in the room; and I am tolerably certain that the objects named in this paper are those which the bottle contained when Dr. Smith brought it to me.

The particles floating in the atmosphere will differ in character according to the season of the year, the direction of the wind, and the locality in which they are collected, and, as might be expected, are much less in quantity after rain.

The small amount of fluid now remaining in the bottle emits the peculiar odour of mildew, and at present the fungoid matter appears inactive.

For the purpose of obtaining a rough approximation of the number of spores, or germs of organic matter contained in the fluid received from Dr. Smith, I measured a quantity by the pipette, and found it contained 150 drops of the size used in each examination. Now, I have previously stated that in each drop there were about 250,000 of these spores, and as there were 150 drops, the sum total reaches the startling number of 37½ millions, and these, exclusive of other substances, were collected from 2·495 litres of the air of this city*—a quantity which would be respired in about 10 hours by a man of ordinary size when actively employed. I have to add that there was a marked absence of particles of carbon amongst the collected matter.

## MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 24th, 1868.

J. B. DANCER, F.R.A.S., President of the Section, in the Chair.

Mr. **SIDEBOTHAM** sent two beautifully finished water-colour drawings, accompanied by the following note:—

"I send you a couple of drawings of the dry-rot fungus *Merulius lachrymans*, remarkably fine. Mr. Lynde, our Treasurer, brought me the specimens, which he had met with during the demolition of some old buildings. The drawings are of the natural size. It is very rarely that such perfect specimens are found, showing, as this one does, the curious structure of the species, and its connection with both the pore and the gill-bearing divisions of fungi."

Mr. **DANCER** exhibited some sand from the sea-shore at Santos, South America. This sand was remarkably silvery in appearance, a large portion of it consisting of minute plates of mica, and very transparent fragments of quartz, which made it an interesting object for the polariscope. It was rich also in Foraminifera, spines of Spatangus, and fragments of Coralline.

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* Behind Dr. R. Angus Smith's laboratory.

"Remarks on Molecular Activity as shown under the Microscope," by J. B. DANCER, F.R.A.S.

The Author stated that, during the last 30 years, he had met with many microscopical observers who were not acquainted with the phenomenon, to which the name of molecular action has been given. This class of microscopists had confined their attention to objects requiring a very moderate amount of magnifying power, and generally tó dry objects; but when their investigations extended to minute objects immersed in fluid which required powers of 800 to 1500 diameters, they were startled by the appearance of particles in active motion, not moving in a direct line, but vibrating as if attracted and then repelled by each other, some single, and others in clusters.

Many instances have come under the author's notice, in which these objects have been regarded by microscopists as animalculæ. They have given rise to many very ingenious speculations, some of which are connected with spontaneous generation, these observers would have been saved much labour if they had been acquaintad with the experiments of the late Dr. Robert Brown on active molecules.

The author does not imagine that the members of this section are wholly unacquainted with the experiments of the early microscopists on this subject, but in the absence of more important matter, he thinks a brief account of the early observations on these so called active molecules may interest them.

The moving particles had been noticed by Leewenhock, Stephen Gray, Buffon, and others who supposed them to be animated matter.

In the year 1827, the late Dr. Robert Brown, whilst engaged in the microscopical investigation of unimpregnated ovulum, noticed that the pollen of the clarckia pulchella was filled with particles, which appeared in active motion when immersed in water. These observations were followed by the examination of the pollen of other plants, the particles of which he found to exhibit similar activity. For some time he was exceedingly perplexed with these phenomena, and was disposed to believe that he had really seen in these minute bodies the supposed constituents or elementary molecules of organic bodies, first so considered by Buffon, Wrisberg, Muller, and Milne Edwards, and on examining various animal and vegetable tissues, whether living or dead, he found, as he had expected, that active molecules were visible by merely bruising the substances in water.

Continuing his observations, he found that particles from a bruised specimen of fossil wood appeared to consist entirely of these moving bodies. From this he inferred that these molecules were not limited to organic bodies, or even to their products.

After this he proceeded to examine minerals, simple earths, metals, and many other substance too numerous to mention, and with similar results.

Some writers who commented on these experiments, but who had not carefully followed his communications, asserted that Dr. Brown imagined these particles to be animated,— and this statement was generally believed.

In 1829 the author's late father repeated many of Dr. Brown's experiments, and to prove that these moving particles could not be animalculæ, he placed some crystals and minerals in a crucible which he subjected to a red heat, ground portions of them to powder, then put it into distilled water, and shewed the particles in motion to his scientific friends. It is now well known that all kinds of matter, if reduced to sufficiently small particles, and placed in a medium in which they will not readily sink, will exhibit these movements.

Now, as this phenomenon occurs in the cells of plants, the yolk of the egg, and in decomposing animal and vegetable matter, it is not surprising that the early microscopists, and, indeed, modern ones, should have mistaken them for animalculæ. The particles which exhibit the greatest activity are exceedingly minute, ranging from 10,000th to 30,000th of an inch in diameter; they remain active a considerable time if they are nearly of the same specific gravity as the solution in which they are immersed. One simple mode of producing them is to rub a little gamboge in water, on a glass slide, and place a thin glass cover on it, using a power of from 800 to 1,200 diameters. They can easily be distinguished with less magnifying power, but are not so effectually shown. If they are required for prolonged examination, Dr. Brown recommends that the solution of gamboge be mixed with a little almond oil. The minute globules of water are thus surrounded by oil, and rapid evaporation is prevented.

The cause of the phenomenon is not yet satisfactorily accounted for. Some have imagined that it is the physical repulsion of the particles when uninfluenced by gravitation. The author has tried many experiments with electricity and magnetism without success. He thinks that the movement may possibly be connected with the absorption and radiation of heat.

Those interested in Dr. Brown's experiments and observations on active molecules, can refer to his republished papers in Vol. I. of the Ray Society's publications for 1866.

After the meeting, slides containing active molecules were exhibited to the members.

A conversation took place on the preservation of dried plants, in which Mr. Bailey stated that he uses with success glue with carbolic acid for attaching them to the paper, as a preservation against mites, placing also a few crystals of the dry acid in his cabinet.

## Ordinary Meeting, April 14th, 1868.

R. ANGUS SMITH, Ph.D., F.R.S., Vice-President, in the Chair.

Mr. Wm. Brockbank and Mr. A. Brothers were appointed Auditors of the Treasurer's Accounts.

Mr. S. **BROUGHTON** said that, on recently observing with high powers a group of fine spots on the sun, one of which was of considerable magnitude, it occurred to him to remove the dark glass, and by keeping the eye much beyond the focus of the heating rays, and at such a distance that the spot almost filled the apparent field of the eye-piece, to see if any phenomena could be observed different from those seen through the dark glass.

The spot was at once seen to be of a dark blood red; but, thinking this perhaps might be from the strong contrast of colour, he attached a disc of plaster of Paris to the telescope, and projected the image of the spot on it. On looking at this with a common pocket magnifier, the image was observed to be a dark blood red, although the observatory was not darkened, and the disc merely shielded from the direct rays by an intervening opaque substance.

If these observations are confirmed, it will corroborate the opinion long held that the spots are not black, but appear so by contrast, and, as it would seem, from the intervention of the coloured glass.

"Brief Notes on the Mutations of Imponderable Elements," by J. C. **DYER**, Esq., V.P.

In the history of speculative science we have seen a great many learned and ingenious theories to account for the varied phenomena presented by the elements that evade the direct tests of weight and measure—such as light, electricity, and magnetism, and more especially that called heat or caloric. With respect to the nature of the latter, directly conflicting views have been taken by philosophers of eminence, both ancient and modern, in expounding the causes of calorific phenomena,—on the one hand denying and on the other affirming the identity or reality of heat as a natural element. Among the ancient asserters of the nonentity doctrine, we find the names of Boyle and his followers; and in modern times those of Count Rumford, Sir Humphry Davy, Dr. Joule, and many others of less eminence: and in support of the material nature of heat, we have the names of Boerhaave and his school, and in later times those of Dr. Black, James Watt, Sir James Leslie, Dr. Hulton, Dr. William Henry, and Dr. Dalton, with others of note. Without here discussing the diverse grounds of difference on the questions at issue (having elsewhere dwelt at length on their respective bearings) I proceed to explain those on which the materiality of heat appears to rest, and whereon the entire range of phenomena connected with the imponderable elements may admit of rational solutions.

I assume the existence of four distinct states or conditions of heat, and which are thus defined, viz.—(1) elemental or neutral heat, (2) latent or combined heat, (3) sensible or expanding heat, and (4) radiating heat. Of the two last forms little need be said beyond what is directly seen in their alternate changes into and out of the other forms of heat (explained further on). The existence of a pervading medium in space has been admitted by the most eminent physicists; but no explanations are given of its nature, except that of its elasticity, nor yet of what are the relations of this medium to those of light, heat, electricity, and magnetism. It is treated as a pervading "ether," or "luminous ether," and its resistance to the transit of light through space has been estimated and given in figures, by a profound philosopher, who, in treating of the undulations of light, describes it as "an elastic medium pervading all space, not even excepting what is occupied by what we call material bodies, that is, such as have weight and which to us constitute the visible and tangible universe of things."—(Sir John Herschel, Bart., "On Light," in *Good Words*, for July, 1865.)

In the works of Boerhaave the said medium is described as "a calorific element, *sui generis*, pervading all space and all bodies, and being in itself of the nature of fire." Thus "a pervading element" rests on very high authority, both ancient and modern; and it is of the nature of such element, and its agency in natural phenomena, that I propose to treat, and to show that the different forms of imponderable matter, as light, heat, electricity, and magnetism, are all essentially identical, and in proof of this we must look to their reciprocal interchanges with each other, whereby their common origin, in one pervading element, will be made probable, if not a physical certainty.

The term formerly applied to this pervading element was a "calid" or "calorific" element,—thus conveying the idea of active fire or very great heat; but they do not accord with the nature or properties which I ascribe to such pervading element, that of "neutral or elemental heat;" and the term latterly employed to indicate the same element is also inapplicable, since it only points to one of its properties, viz., elasticity. I have, therefore, adopted the term of "neutral heat," which is meant to convey the idea or notion that this element, in fact, constitutes the source, or fountain, whence all other forms of heat are derived, and into which they alternately return. This neutral element is everywhere of equal density, by reason of its elastic tension, and it therefore cannot exert or possess the force of gravity or of inertia, and it must remain in a quiescent state, except in cases where its equilibrium becomes disturbed by external forces, mechanical or chemical, as after mentioned.

Let us consider the nature of these forces, and, first, the vast mechanical forces continually acting upon this medium by the earth's orbital passage through it. As the earth advances her force will compress the medium in front of her path, but its equilibrium must be instantly restored by its reaction in her wake. This reaction explains the slight retarding force of the medium on the periodic motions of the planets. Now, the disturbing forces of these orbital movements, along with the axial rotation of the earth, serve to generate continuous currents in the medium, and which constitute what we call magnetism, or the magnetic currents, such as flow from the central action in the earth's path towards the north and south poles. In addition to the invisible currents, shown by the magnetic phenomena, we may fairly assume that this mechanical action of the earth generates those luminous currents in the medium that are known, at certain seasons, as ground lightning, and at others as auroras or corruscations of polar lights.

We have next to consider the frictional action of the surfaces of the sun and earth upon that portion of the medium which extends in a right line between the sun and the earth, and which, acting at the ends of the intervening column, excites the medium into those undulations or vibrations that act on our senses, and give the impression of white light when seen through clear air, and of coloured light when the undulations are disturbed by clouds or vapours. But, to receive the impressions, we must be within the intervening column, or near it, as in twilight. Now, although this medium is only perceptible as light, yet the medium itself is at all times present, equally in the night and in dark rooms as in daylight,—for when its action as light is cut off by opaque bodies, still the ambient element continues to act on our feelings and bodies as sensible heat, and this in different degrees of temperature, and when the heat is intense it becomes visible as red, or white heat and light. The duplex nature of light, too, is shown by the action of the prism. In the spectrum, where an increase of heat appears by moving the thermometer from the lightest to the darkest or red rays, and a still higher temperature is shown when the bulb is placed just outside of the visible spectrum, viz., in the dark rays. Thus clearly proving that, by this mechanical action on transmitted light among the visible rays, it is partially converted into heat, and wholly so converted outside of the spectrum or in the dark rays. We find also that sensible heat obeys the same laws of reflection and refraction that govern the motions of light, and differs only in the transmission through opaque or transparent bodies. Besides, we see the mechanical action of the lens gives direct proof of the conversion of light into heat by the simple convergence of its rays upon a point, whereby intense heat is produced. The plain inference, then, is that an element thus alternately exhibiting both the mingled and separate phenomena of light and heat, must possess identical properties, and that those several conditions of it are to be ascribed to the mutations of one element when subjected to the said acting forces. The facts adduced are clearly explained on this assumption, viz.—of the identity of light and heat, whilst they present a strange mass of anomalies, if viewed as produced by the movements of two distinct elements in space.

I pass from mechanical action to consider the chemical forces, continually acting on and by this medium, whereby its mutations are effected, and among these, the most extensive are caused, by the affinities between heat and water, as shown in common surface evaporation. Without here dwelling upon the diverse doctrines or views, of late entertained, respecting the nature of latent or specific heat, it will suffice to state the following facts, viz.—that 212° of sensible heat are required to raise water to the boiling point, under the atmosphere, and then to convert the water into steam, 940° more, sensible heat, are absorbed—this latter disappears, as sensible heat, (call it latent, specific, or otherwise) but when the steam is condensed and returns to water, then the same 940° reappears as sensible heat, Now we all know that such absorption and redischarge of heat is continually taking place, in the evaporation from the earth's surface, by the action of solar heat. This heat then to the extent, at least, of 940° becomes latent in the steam, or invisible aqueous vapour, at the surface, and the same heat is again given out in the upper air, where the vapour is condensed into clouds or fogs. The experiments and estimates of Dr. Halley, enable us in some degree to perceive the stupendous scale of the daily ascent of water in vapour,[*] into the higher regions of the air, when its heat both sensible and latent, is evolved. But this heat, does not thence return to the earth, as sensible heat, because the temperature above the clouds is always at or near the freezing point. What then becomes of these vast floods of sensible heat, that enter the vapour at the surface and entirely disappear in those cold regions above? The plain answer seems to be that the entire heat, so ascending and disappearing, simply passes into its normal, or elemental form, of neutral heat. On this view of the mutations of heat, we see why it must become more or less redundant in the upper air, according to the varying rates of evaporation. These mutations of heat, will account for its becoming redundant, or of higher elastic tension, as a neutral element, in the upper air, and comparatively deficient near the earth, whereby the balance of tension, or its equilibrium, must become more or less disturbed according as evaporation varies at the surface. In this way such continuous fluxions of heat into the higher regions, and its redundant state there, generates and sustains the return currents of the element from above towards the lower clouds and the earth, which elemental movements constitute the electrical currents, such as are shown by electrometers. The diurnal variations of these currents arise from the differing rates of evaporation in the day and night. The ordinary course of these elemental fluxions apply to the common invisible currents, but, in like manner, when the mutations are excessive, they will account for the more rapid and violent electrical phenomena, constituting the thunder storms and lightnings, which so often occur within the tropics, and sometimes in our latitudes when evaporation is very rapid.* In fine, all of the corruscations, or vivid transits of the said element to restore its disturbed equilibrium are to be taken as constituting what we call the electrical and magnetic currents—as before said. Since we find that mechanical forces are exerted, as in telegraphy, &c., both by the electrical and magnetic currents, their materiality is clearly proved, for such action can only be by contact of natural bodies; and by conjoint action they are shown to be merely two conditions of one and the same element, just as the mingled and alternate action of light and heat show them to be but two forms of one element. The amounts of latent heat in different bodies may be here passed by, but one other case of latent heat in aqueous vapour should be noticed, viz. the heat evolved in respiration. Beneficent nature always provides for her own wants in excess. The carbonic acid formed in the lungs gives out the needful heat to keep up the temperature of the warm-blooded animals, and the excess of heat so evolved is taken up by the water converted into steam and which is carried away in breathing. Now, if the whole of the heat so absorbed in the steam were to remain as sensible heat in it, the 940° held latent therein would at once produce a temperature quite destructive of the living textures and would of course extinguish life. Thus we see another of the sublime laws of Providence is made to rest on these mutatations of the imponderable elements. Little need here be said concerning radiating heat as apart from that of light, yet one instance may be cited, viz., the experiment of suspending a thermometer from the top of a glass-receiver on the table of an air-pump, then by rapidly drawing out the air the temperature will fall to or near the freezing point, but the mercury will again slowly rise till it reaches the temperature of the room; but as the thermometer (attached by a non-conductor of heat) is quite isolated in a vacuum, how can the heat reach and re-enter the bulb, except it be by radiating from the external warm air through the glass and the vacuum, and thus restoring the equal temperature inside and outside of the receiver. This process is of the same kind as that of heat radiating from a common fire-place to warm a room.

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[*] Dr. Halley (in the "Philosophical Transactions") "has shown that more than five thousand millions of tons of water, ascend, in vapour, daily, from the surface of the Mediteranean only." Now it follows that a mighty force must be exerted in raising this stupendous weight of water, (to the height of some thousand feet) against the force of gravity, in the water, and the elastic force of pressure by the air. But this evaporating force is solely due "to the known relations of heat and water," and is (as before said) a chemical force.

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* "The phenomena of aqueous meteors, such as rain, fog, and dew depend upon the known relations of heat and water; electricity appears to be a consequent, rather than an agent, in the formation and decomposition of clouds, or if a necessary agent, it is equally so in the boiling of water in our houses or the drying of piece goods in a stove."—Dalton: *Memoirs Literary and Philosophical Society*, vol. 4, 2nd series, page 123.

In my "Notes on Heat in Relation to Atmospherical phenomena," read at the Society, February 9th, 1853, besides adducing reason for assuming that all electrical and magnetical phenomena are simple consequences of the mutations of one pervading element, I have therein shown how most of the great aerial and oceanic currents may be traced to the acting forces generated by the mutations of heat, when they are excited in conjunction with those of gravity.

Now, since all philosophers recognise the presence everywhere of the three elements of light, electricity, and magnetism, as also the fact of their reciprocal interchanges of form, as exhibited in phenomena; and as we find, also, the general admission of "an elastic ether" in space, it would seem far more in harmony with "the simplicity of nature's laws" to ascribe those known forms of "the imponderable elements" to the mutations of one than to the existence, in space, of four distinct elements. I have accordingly aimed to show that each of the known conditions of imponderable matter are traceable to the mutations of the one pervading element, and, for the reasons before stated, I call this elemental or neutral heat.

"Researches on Di-Methyl," by WM. H. DARLING, Dalton Scholar in the Laboratory of Owens College. Communicated by Professor H. E. Roscoe, Ph.D., F.R.S.

The synthesis of carbon compounds forms perhaps the most important and interesting branch of modern chemical enquiry. The most recent developments of these synthetical processes are the now well ascertained facts of the dependence of the chemical properties of the molecule, upon the position of the individual atoms of which that molecule is built up.

Any isomeric modifications of the saturated monovalent compounds containing one or two atoms of carbon can only be explained by the existence of a difference between the four combining powers of each carbon atom, whilst in the tri-carbon and higher series isomerism indicates the difference in the power of combination existing between the end and the middle carbon atoms of the chain.

From Frankland's original observations concerning the difference between the action of chlorine on the so-called di-methyl $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}$ obtained by the electrolysis of an alkaline acetate, and on the hydride of ethyl $\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}$ ,obtained from ethyl compounds, the existence of a difference in the four combining powers of a carbon atom was rendered probable.

The subsequent researches of Schorlemmer have, however, proved that only one hydrocarbon of the formula $C_2H_6$ exists, inasmuch as he succeeded in preparing ethyl chloride from the hydrocarbon di-methyl $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}$, obtained by the electrolytic decomposition of an alkaline acetate (*Proc. R. Soc.*, XIII., 225); as well as from ethyl hydride, obtained from ethyl compounds.—(*J. Chem. Soc.*, N.S., II. 262.)

It appeared of great interest to repeat this synthesis, and to prepare the chloride in larger quantity, from which to prepare ethyl compounds and ascertain their chemical and physical properties.

At the request of Mr. Schorlemmer, I undertook this investigation. I take this opportunity to express my thanks to that gentleman, and also to Professor Roscoe for the kind assistance rendered to me throughout this research.

I prepared the di-methyl by the electrolytic decomposition of acetate of potash, according to the process described by Kolbe. The gas, evolved from a platinum plate contained in a porous cell, was passed first through a solution of caustic potash, to absorb the carbonic acid, afterwards through nordhausen acid, and over pumice stone moistened with oil of vitriol, to free it from a trace of oxide of methyl or hydrocarbon absorbable by this acid, and finally through a solution of caustic potash, to absorb acid fumes, any carbonic acid which had escaped the first wash bottle, or traces of sulphurous acid. The gas thus prepared had a very slight odour, it burnt with a non-luminous flame.

The following analysis of it is according to Bunsen's method :—

| | Volume. | Pressure. | Tempt. C. | Volume at 0° C and 1m. Pressure. |
|---|---|---|---|---|
| (1.) Original volume of Gas (moist) | 183·48 | 0·1402 | 8·5 | 24·94 |
| (2.) After addition of oxygen „ | 338·41 | 0·3919 | 8·0 | 127·00 |
| (3.) „ „ air „ | 567·52 | 0·5221 | 8·5 | 287·5 |
| (4.) „ explosion „ | 505·38 | 0·4591 | 9·2 | 224·7 |
| (5.) „ absorption of carbonic acid (dry) ……… | 447·58 | 0·4156 | 12·8 | 173·9 |

| | | | |
|---|---|---|---|
| Contraction observed……………… | 62·80 …… | calculated | 62·85 |
| Carbonic acid „ ……………… | 50·80 …… | „ | 49·83 |
| Hydrogen in two volumes of gas found | 6·076 …… | calculated | 6·000 |
| Carbon „ „ „ | 2·036 …… | „ | 2·000 |

This gas, treated with an equal volume of chlorine, was exposed to the diffused sun-light, after allowing time for the two gases to mix until nearly colourless, and completed by means of direct sun-light when quite colourless. The bottle was opened under warm water: the hydrochloric acid was absorbed equal to half the capacity of the bottle. The remainder of the gaseous contents not absorbed were displaced by warm water into a receiver, in which a few pieces of stick potash were placed, surrounded by a freezing mixture of salt and ice—a colourless, volatile liquid was condensed.* One hundred grammes of chloride were prepared by the repetition of this process. This first product was separated by distillation into two parts, one which distilled below 30° C., and the other above 30° C. On still further fractionating the first distillate, a portion was obtained boiling at 11 –13° C., whose Sp. Gr. was 0·9253 at 0° C. Pierre found the specific gravity of ethyl chloride to be 0·9241 at the same temperature.

The chloride boiling below 30° C. gave, on heating in sealed tubes with acetate of potash and glacial acetic acid to a temperature of 130°—140° C. for three or four hours, a volatile fragrant liquid, having the characteristic odour of acetic ether, which, after drying over chloride of calcium and magnesia, boiled at 74·0—75·5° C. Kopp gives the boiling point of ethyl acetate at 74·3 under a pressure of 760 mm. of mercury.

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* If the gas or the chlorine was not pure, being mixed with air, very little or no liquid was condensed, being carried off by the current. The same was observed by Mr. Schorlemmer. This will probably account for Frankland's observation that no liquid was condensed at —18° C.

In order to prepare the alcohol, the acetate was heated with crystals of baryta hydrate in sealed tubes for one or two hours, to a temperature of 120° C.; after cooling it was distilled, and the distillate treated with dry carbonate of potash until it separated into two layers, the upper one was decanted upon fused carbonate, afterwards upon anhydrous baryta, from which it was distilled, when it had assumed a light amber colour. It began to boil at 78·1° the whole coming over before the temperature exceeded 79·0° C. Kopp gives the boiling point of ethyl alcohol prepared by fermentation at 78·4° C. under a pressure of 760 mm. mercury, and the specific gravity at 0° C. as 0·8095; calculating by means of his coefficient of expansion the Sp. Gr. at 6° C. would be 0·80446, whilst I found the same to be 0·80302 at the same temperature.

The alcohol thus prepared had very little odour, agreeing in this respect with the observation of Meudelejeff *(Zeitschrift Chemie,* 1865, 257*)* though the specific gravity is slightly higher than his at the same temperature calculated from his coefficient of expansion, viz. 0·80123.

On submitting this liquid to combustion analysis the following numbers were obtained.

No. 1. 0·2834 grm. of liquid gave 0·5357 grm. of carbonic acid, and 0·3314 grm. of water.

No. 2. 0·5533 grm. of liquid gave 1·0481 grm. of carbonic acid, and 0·6480 grm. of water.

| | Percengate | | Calculated from the formula $C_2H_6O$ |
|---|---|---|---|
| | No. 1. | No. 2. | |
| C | 51·56 | 51·65 | 52·17 |
| H | 12·99 | 13·02 | 13·04 |
| O | 35·45 | 35·33 | 34·79 |
| | 100·00 | 100·00 | 100·00 |

The numbers are not all that could be desired when compared with the calculated, this is owing to the difficulty in burning so volatile a liquid, and to the small quantities taken. Dumas and Boullay (*Ann. de Chimic*, 1827, 36, 299) state that in order to obtain agreeing results upwards of a grm. of liquid was necessary, in one combustion 1·742 grms. were used, with ether a still greater quantity was required.

The alcohol still remaining in the carbonate of potash and in the dilute solution was separated by distillation, this distillate was oxidised by a mixture of bichromate of potash and sulphuric acid, when the characteristic odour of aldehyde was recognised, the oxidation was continued until it had disappeared, on distilling to dryness an acid distillate was obtained, this was neutralised with pure carbonate of soda, and yielded on evaporation needle-shaped crystals of acetate of soda; the mother liquor was distilled to dryness with sulphuric acid, the distillate neutralised with pure carbonate of silver, filtered and boiled, on cooling it yielded colourless transparent flat needles, which, after drying over sulphuric acid, gave on analysis the following numbers.

No. 1. 0·4142 grm. of salt gave 0.2663 grm. of metallic silver.

No. 2. 0·5095 grm. of salt gave 0·3274 grm. of metallic silver.

No. 3. 0·3650 grm. of salt gave, after drying at 100° C. in a water bath for one hour, 0·2349 grm. of metallic silver.

No. 4. 0·2483 grm. of salt gave, after drying at 100° C. in a water bath for two days, 0·1604 grm. of metallic silver.

Estimation No. 1 gave 64·30 per cent. of silver.
„ No. 2 „ 64·25 „ „
„ No. 3 „ 64·36 „ „
„ No. 4 „ 64·60 „ „

Calculated from the formula of acetate of silver $\left.\begin{matrix}C_2H_3O\\ Ag.\end{matrix}\right\}O$ yields 64·68 per cent. of silver.

That portion of the mixed chlorides which distilled above 30° C. was fractionated, when two-thirds of the total volume distilled over between 57°-59° C. The specific gravity was found to be 1·198 at 6·5° C. Regnault found the Sp. Gr. to be 1·174 at 17° C. and the boiling point to be 64° C. (*Ann. Ch. Phys.* [2] LXXI. 355)* Submitted to analysis the following numbers were obtained.

No. 1. 0·5206 grm. gave 0·3691 grm. of chlorine.
No. 2. 0·4491 „ „ 0·3186 „ „
No. 3. 0·4292 „ „ after drying over stick potash for a week, 0·3010 grm. of chlorine.

Per centage of chlorine found by No. 1, 70·89
„ „ „ No. 2, 70·94
„ „ „ No. 3, 71·76

The percentage required by the formula $\left.\begin{matrix}C_2H_4Cl\\ Cl\end{matrix}\right\}$ of mono-chlorinated ethyl chloride is 71·73.

Hence the mono-chlorinated ethyl chloride was formed in quantity by the action of excess of chlorine on di-methyl.

Having commenced the examination of the di-methyl obtained, 1st by Frankland's method from iodide of methyl and zinc, and 2nd by Schutzeuberg's process, I hope to communicate the results to the Society.

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* Beilstein (*Ann. Chem. Pharm.* V. 113-110) has shewn that mono-chlorinated ethyl chloride, and chloride of ethyleden, obtained by action on aldehyde with perchloride of phosphorus, are identical, the boiling point of the latter as observed by Wurtz is 58-59, and the specific gravity as determined by Geuther is 1·189 at 4·3° C., these numbers agree with those I found. Beilstein remarks that the higher boiling point as observed by Regnault would result from the presence of higher chlorinated products.

## PHYSICAL AND MATHEMATICAL SECTION.

### Annual Meeting, March 24th, 1868.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section in the Chair.

The following Gentlemen were elected Officers of the Section for the ensuing year:—

**President.**
ROBERT WORTHINGTON, F.R.A.S.

**Vice-Presidents.**
E. W. BINNEY, F.R.S., F.G.S.
JOSEPH BAXENDELL, F.R.A.S.

**Treasurer.**
MR. THOMAS CARRICK.

**Secretary.**
G. V. VERNON, F.R.A.S., F.M.S.

"Results of Rain-Gauge and Anemometer Observations made at Eccles, near Manchester, during the year 1867," by THOMAS MACKERETH, F.R.A.S., F.M.S.

The following amounts of rainfall are obtained from two gauges 3 feet from the ground, and one gauge 34 feet from the gronnd. One of the gauges 3 feet from the ground has a round receiver 10in. in diameter, the other has a 5in. square receiver; the edges of both are turned inward. These two gauges stand close to each other on the same plane, 75 feet from my house, and free from every obstruction. The other gauge is a 5in. square one like the one near the ground. It is 4 feet above the ridge of my house, and free from every obstruction. First I represent the rainfall for 1867, as measured by the 10in. gauge 3 feet above the ground and 145 feet above the sea. This I have compared with the average fall for 7 years at Eccles.

| Quarterly Periods | | 1867. | Fall in Inches. | Average of 7 Years. | Differences. | Quarterly Periods | |
|---|---|---|---|---|---|---|---|
| Average of 7 Years. | 1867. | | | | | Average of 7 Years. | 1867. |
| Days | Days | | | | | | |
| 51 | 55 | January ......... | 3·306 | 2·600 | +0·706 | 7·425 | 7·469 |
| | | February ......... | 2·590 | 2·250 | +0·340 | | |
| | | March ......... | 1·573 | 2·575 | —1·002 | | |
| 46 | 35 | April ......... | 4·503 | 1·982 | +2·521 | 7·081 | 7·641 |
| | | May ......... | 1·788 | 2·224 | —0·436 | | |
| | | June ......... | 1·350 | 2·875 | —1·525 | | |
| 50 | 59 | July ......... | 5·331 | 3·310 | +2·021 | 10·526 | 10·380 |
| | | August ......... | 1·628 | 3·289 | —1·661 | | |
| | | September ...... | 3·421 | 3·927 | —0·506 | | |
| 52 | 51 | October ......... | 4·100 | 3·540 | +0·560 | 9·640 | 10·025 |
| | | November ...... | 2·159 | 3·385 | —1·226 | | |
| | | December ...... | 3·766 | 2·715 | +1·051 | | |
| 203 | 211 | Totals ......... | 35·515 | 34·672 | +0·843 | | |

This table shows that the rainfall at Eccles has been nearly an inch in excess of the average of 7 years. The summer months from May to September, with the exception of July, had a fall below the average. The wettest months were at the beginning and end of the year. According to the 7 years average April has the minimum, and September the maximum rainfall of the year. The next table shows the monthly amounts that fell in each gauge, and the number of miles of horizontal movement of the air.

| 1867. | Rainfall in 10in. Round Gauge, 3 feet from Ground. | Rainfall in 5in. Square Gauge, 3 feet from Ground. | Rainfall in 5in. Square Gauge, 34 feet from Ground. | Amount of Horizontal Movement of the Air in Miles. |
|---|---|---|---|---|
| January ...................... | 3·306 | 3·179 | 2·558 | 5,396 |
| February ...................... | 2·590 | 2·547 | 1·775 | 5,334 |
| March ...................... | 1·573 | 1·539 | 1·120 | 6,884 |
| April ...................... | 4·503 | 4·582 | 4·023 | 6,867 |
| May ...................... | 1·788 | 1·745 | 1·703 | 5,137 |
| June ...................... | 1·350 | 1·312 | 1·205 | 3,817 |
| July ...................... | 5·331 | 5·297 | 4·711 | 3,677 |
| August ...................... | 1·628 | 1·512 | 1·301 | 1,744 |
| September...................... | 3·421 | 3·320 | 2·846 | 3,389 |
| October...................... | 4·100 | 4·031 | 3·526 | 3,371 |
| November...................... | 2·159 | 2·070 | 1·966 | 3,018 |
| December ...................... | 3·766 | 3·695 | 3·333 | 4,121 |
| Totals............. | 35·515 | 34·829 | 30·067 | 52,755 |

This table shows that the greatest horizontal movement of the air happens in the winter months, or rather from December to April; and as the rainfall appears to diminish from December to April, it might be expected that the movement of the air and the fall of rain should be the inverse of each other. Yet from the observations of both, which I have, it appears that an excess of windy weather in any month of the year is nearly always attended with an excess of rainfall.

TABLE

Showing the relative proportion of the winds for each month reduced to the four cardinal points, the number of days on which rain fell, together with the average amount received in each gauge.

| 1867. | Relative Proportion of the Winds to the Days of each Month. | | | | No. of Days on which Rain fell. | Average Amount of Rain received in each Gauge. | | |
|---|---|---|---|---|---|---|---|---|
| | | | | | | 10in. Round Gauge 3ft. from Ground. | 5in. Square Gauge 3ft. from Ground. | 5in. Square Gauge 34ft. from Ground. |
| | N. | E. | S. | W. | | | | |
| January | 8 | 8 | 7 | 8 | 18 | 0·183 | 0·176 | 0·142 |
| February | 3 | 5 | 9 | 11 | 18 | 0·143 | 0·141 | 0·098 |
| March | 9 | 15 | 4 | 3 | 19 | 0·082 | 0·081 | 0·059 |
| April | 6 | 4 | 6 | 14 | 27 | 0·166 | 0·169 | 0·149 |
| May | 7 | 9 | 8 | 7 | 15 | 0·119 | 0·116 | 0·113 |
| June | 12 | 5 | 3 | 10 | 13 | 0·103 | 0·101 | 0·092 |
| July | 12 | 4 | 3 | 12 | 14 | 0·380 | 0·378 | 0·336 |
| August | 9 | 3 | 5 | 14 | 17 | 0·095 | 0·088 | 0·076 |
| September | 10 | 6 | 3 | 11 | 19 | 0·180 | 0·174 | 0·149 |
| October | 7 | 2 | 4 | 18 | 23 | 0·178 | 0·175 | 0·153 |
| November | 10 | 8 | 6 | 6 | 9 | 0·239 | 0·230 | 0·218 |
| December | 11 | 7 | 3 | 10 | 19 | 0·198 | 0·194 | 0·175 |
| | 104 | 76 | 61 | 125 | 211 | 0·172 | 0·168 | 0·146 |

TABLE

Showing the average daily fall in each kind of gauge, when the velocity of the wind has ranged between the number of miles indicated in the first column.

| Daily Movement of Wind. | No. of Days on which Rain fell. | 3 feet from the Ground. | | 34 feet from Ground. | Difference between (*a*) and (*b*). |
|---|---|---|---|---|---|
| | | 10in. Round Gauge. | 5in. Square Gauge. (*a*). | 5in. Square Gauge. (*b*). | |
| Miles. | | In. | In. | In. | In. |
| 0 to 50 | 21 | ·158 | ·154 | ·146 | ·008 |
| 50 to 100 | 40 | ·151 | ·148 | ·137 | ·011 |
| 100 to 150 | 32 | ·178 | ·173 | ·154 | ·019 |
| 150 to 200 | 39 | ·215 | ·210 | ·185 | ·025 |
| 200 to 250 | 23 | ·134 | ·132 | ·102 | ·030 |
| 250 to 300 | 28 | ·208 | ·205 | ·169 | ·036 |
| 300 to 350 | 6 | ·224 | ·226 | ·175 | ·051 |
| 350 to 400 | 5 | ·191 | ·178 | ·127 | ·051 |
| Above 400 | 3 | ·251 | ·251 | ·190 | ·061 |
| MEAN<br>200 to 250 | 22 | ·190 | ·186 | ·154 | ·032 |

This table shows that rainy days are most numerous, with a proportionate amount of rainfall, when the horizontal movement of the air, at 34 feet above the ground, is about five miles per hour. But the number of wet days diminishes as the hourly movement of the air increases, though the heaviest falls of rain happen as the movement of the air increases to an average ranging from 8 to 12 miles per hour. The mean difference of the rainfall, 34 feet and 3 feet from the ground, is ·032 inch. This difference is decreased to its minimum in calm weather, and increases to its maximum when the horizontal movement of the air at 34 feet above the ground is about 16 miles per hour.

In May last I commenced to measure the amount of rainfall twice in 24 hours. The results of these measures I represent in the following table.

| 1867. | Rainfall from 8 a.m. to 8 p.m. | Rainfall from 8 p.m. to 8 a.m. | Difference between the Day and Night Fall. |
|---|---|---|---|
| | In. | In. | |
| May | 0·932 | 0·856 | —0·076 |
| June | 0·748 | 0·602 | —0·146 |
| July | 3·354 | 1·977 | —1·377 |
| August | 0·779 | 0·849 | +0·070 |
| September | 2·740 | 1·681 | —1·059 |
| October | 2·091 | 2·009 | —0·082 |
| November | 1·281 | 0·878 | —0·403 |
| December | 1·775 | 1·991 | +0·216 |

## Annual Meeting, April 28th, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Professor JOHN TYNDALL, LL.D., F.R.S.; Professor PETER GUTHRIE TAIT, M.A., F.R.S.E.; and CHARLES DARWIN, Esq., M.A., F.R.S., were elected Honorary Members of the Society.

The following Report of the Council was read by one of the Secretaries:—

The Council have again the satisfaction to report that the Society's finances continue in a healthy condition, the general balance of the Treasurer's account on the 1st of April, 1868, being £267 19s. 2d., against £250 2s. 1d. on the 1st of April, 1867.

The number of ordinary members on the roll of the Society on the 1st of April, 1867, was 180, and four new members have since been elected. The losses have been by death, four; by resignations, four; and defaulters, two. The number of members on the roll on the 1st of April, 1868, was 174. The deceased members are, Mr. John Clowes Grundy, Mr. James Aspinall Turner, Mr. Thomas Heelis, F.R.A.S., and Mr. Henry Houldsworth.

Mr. Turner held a high place among the principal merchants and manufacturers of Manchester, and was twice chosen by his fellow citizens (in 1857 and 1859) to represent them in parliament, which he continued to do till 1865. It may be mentioned that he was the nephew of the Rev. William Turner, who was an honorary member of the Literary and Philosophical Society for many years, and whom he always held in the greatest reverence. While actively engaged in business, and holding several public offices, such as that of Chairman of the Manchester Commercial Association, President of the Natural History Society, and Chairman of Manchester New College, he yet found time to cultivate his strong taste for natural history, and was no mean proficient in certain branches of entomology and botany, and had expended a large amount both of time and money in forming collections illustrative of these subjects.

Mr. Heelis was elected a member of the Society on the 19th of April, 1859, and for three years filled the office of secretary of the Physical and Mathematical Section. He was a very intelligent and accurate observer, and in the course of several voyages during which he visited India, China, the Cape of Good Hope, North America, and the coasts of the Mediterranean, he accumulated a large mass of valuable observations, some of the results of which were given to the Society in the following papers and communications:—

*November* 10*th*, 1859.—"On Storms, with some attempt to ascertain their tracks in the neighbourhood of the British Islands, and their analogy to other Cosmical Phenomena."

*January* 5*th*, 1860.—"On R. Bohun's old work 'On the Origin and Nature of Wind,' published at Oxford in 1671."

*April* 26*th*, 1860.—"On the Observations of the Sun made by Hevelius in the years 1642-3."

*November* 7*th*, 1861.—Observation of the Zodiacal Light at Smyrna, September 13th, 1861.

*March* 18*th*, 1862.—"On Abnormal Trade Winds."

*November* 13*th*, 1862.—"Notes of Observations on $\eta$ Argûs."

*November* 13*th*, 1862. — "Hydrometric Observations of the Water of the Mediterranean."

*January* 27*th*, 1863.—"On Swell observed at Sea, particularly in the Regions of the South-East Trade Wind."

*April* 2*nd*, 1863.—"Notes of Observations of the Speed and Order of Succession in Magnitude of Waves observed in Gales of Wind off the Cape of Good Hope."

*October* 15*th*, 1863.—"On Fog in the Mediterranean."

*November* 12*th*, 1863.—"On a Meteor seen by Daylight."

*January* 12*th*, 1865.—Account of a Fireball seen December 13th, 1864.

*October* 31*st*, 1865.—"On a Coal Basin between Mount Olympus and the Bay of Oraniska."

*December* 7*th*, 1865.—"On Meteors."

*December* 11*th*, 1866.—"On Cautionary Storm Warnings."

*December* 26*th*, 1866.—"On some recent Observations on the Specific Gravity of Sea Water."

Mr. Houldsworth was born in Nottinghamshire, on the 4th of June, 1797. His father had much mechanical taste and knowledge, and at the time of his eldest son's birth (the late Henry Houldsworth) he was engaged in developing the spinning mule, and, as afterwards turned out, in laying the foundation of the cotton spinning concern of Thomas Houldsworth and Co., with which his son subsequently became closely connected. The late Henry Houldsworth, though born in Nottinghamshire, was educated chiefly in Glasgow (at which place his father, shortly after the year 1800, went to reside). He was a student in Glasgow University, and about the year 1818 showed his love of science and his powers of invention by publishing in the *Journal of Arts* the first idea of a gas meter. In 1822 he came to live permanently in Manchester, and joined his uncle Thomas Houldsworth in the cotton spinning business. From that year till about 1835, when he became interested in railways, he was continuously engaged in experimenting upon and improving every process in fine cotton spinning, and in every department he produced improvements, some of which are in use to this day, while others have been superseded. Most of these are only of interest to those engaged in the business with which they are connected. But some, such as "the differential motion" (described to the Society already by Mr. Dyer), various applications to spinning mules, and his improvements upon Heilman's combing machine, have probably some interest in a scientific point of view, as exhibiting the thorough knowledge of mechanical principles which he undoubtedly possessed, as well as an aptitude for invention which was very remarkable, all the more because it was generally applied to what were comparatively trivial subjects. He made very extensive experiments in smoke burning and in raising steam economically. He also improved considerably Heilman's embroidery machine, and during the last few years of his life, when he retired from Manchester, he introduced several important improvements in the construction and adaptation (especially to stiff lands) of the steam plough. This is a general summary of the results of a life extending over 70 years, more marked by constant, conscientious industry and love of knowledge and improvement than by any great achievement.

The following papers and communications have been read at the ordinary and sectional meetings of the Society during the present session:—

*October* 1*st*, 1867.—"On Sections of Volkmannia," by E. W. Binney, F.R.S., F.G.S.

*October* 1*st*, 1867.—"On the Portraits of Sir Isaac Newton," by Samuel Crompton, M.D.

*October* 1*st*, 1867.—"On Pholas holes in the Limestone Rock on Grin Edge, near Buxton," by R. D. Darbishire, B.A., F.G.S.

*October* 7*th*, 1867.—"On Silk-producing Worms from Natal," by Mr. A. G. Latham.

*October* 15*th*, 1867.—"Note on the Occurrence of Sulphocyanide of Ammonium in Gas Mains," by Mr. Peter Hart.

*October* 15*th*, 1867.—"Jupiter, as observed at Ardwick, on the night of August 21st, 1867," by J. B. Dancer, F.R.A.S.

*October* 15*th*, 1867.—"Notes on some Superficial Deposits at Great Orme's Head, and the Period of its Elevation," by R. D. Darbishire, B.A., F.G.S.

*November* 4*th*, 1867.—"Note on the Ship-Barnacle," by Mr. J. Sidebotham.

*November* 7*th*, 1867.—"Note on the Colour of the Moon during Eclipses," by A. Brothers, F.R.A.S.

*November* 12*th*, 1867.—"On the Posidonian Schists of the Isle of Man," by E. W. Binney, F.R.S., F.G.S.

*November* 12*th*, 1867.—"On Exhibitions on the Continent," by R. Angus Smith, Ph.D., F.R.S.

*November* 26*th*, 1867.—"On a Thermometer unaffected by Radiation," by J. P. Joule, LL.D., F.R.S., &c.

*November* 26*th*, 1867.—"On Solar Radiation," by J. Baxendell, F.R.A.S.

*November* 26*th*, 1867.—"Solar Radiation Observations made at Old Trafford, Manchester," by G. V. Vernon, F.R.A.S., F.M.S.

*November* 26*th*, 1867.—"A Comparison of Solar Radiation on the Grass, and at Six Feet from the Ground," by Thomas Mackereth, F.R.A.S., F.M.S.

*December* 2*nd*, 1867.—"On Plants appearing in successive years on land prepared for Plantations in Cheshire," by Mr. H. A. Hurst and Mr. George Carter.

*December* 10*th*, 1867.—"On the Age of the Hæmatite Iron Deposits of Furness," by E. W. Binney, F.R.S., F.G.S.

*December* 24*th*, 1867. —"Memorandum on the Evaluation of Integrals," by the Hon. Chief Justice Cockle, F.R.S., &c., of Queensland. Communicated by the Rev. Professor Harley, F.R.S.

*December* 24*th*, 1867.—"On some Points in the Restoration of Euclid's Porisms," by T. T. Wilkinson, F.R.A.S., Corresponding Member of the Society.

*December* 24*th*, 1867.—"On the Examination of Water for Organic Matter," by R. Angus Smith, Ph.D., F.R.S.

*December* 31*st*, 1867.—"Solar Radiation Observations made at Eccles, near Manchester," by Thomas Mackereth, F.R.A.S., F.M.S.

*January* 7*th*, 1868.—"Variable Spot on the Moon's Surface," by W. R. Birt, F.R.A.S. Communicated by J. Baxendell, F.R.A.S.

*January* 7*th*, 1868.—"On Pholas Borings, as described in Mr. Charles P. Jopling's Sketch of Furness and Cartmel," by E. W. Binney, F.R.S., F.G.S.

*January* 7*th*, 1868.—"On a Method of determining the Absolute Horizontal Intensity of the Earth's Magnetism," by J. P. Joule, LL.D., F.R.S., &c.

*January* 7*th*, 1868.—"On the Examination of Water for Organic Matter," Part II., by R. Angus Smith, Ph.D., F.R.S.

*January* 21*st*, 1868.—"Notes on Some of the Rarer Plants found near Llandudno," by Mr. J. Sidebotham.

*January* 21*st*, 1868.—"Some Remarks on Crystals containing Fluid," by J. B. Dancer, F.R.A.S.

*January* 28*th*, 1868.—"On Solar Radiation," Part II., by J. Baxendell, F.R.A.S.

*February* 4*th*, 1868.—"On some Constituents of Cotton Fibre," by E. Schunck, PhD., F.R.S., &c., President of the Society.

*February* 24*th*, 1868.—"Remarks on Molecular Activity as shown under the Microscope," by J. B. Dancer, F.R.A.S.

*February* 25*th*, 1868.—"On the Aurora Borealis as a Weather Prognostic," by Mr. Thomas Ogilvy. Communicated by Murray Gladstone, F.R.A.S.

*February* 25*th*, 1868.—"On the supposed Influence of the Moon on the Temperature of the Atmosphere near the Surface of the Earth," by J. Baxendell, F.R.A.S.

*February* 25*th*, 1868.—"On the Rainfall at Old Trafford, Manchester, during the year 1867," by G. V. Vernon, F.R.A.S., F.M.S.

*March* 3*rd*, 1868.—"On the Lunar Crater Linné," by Herr J. F. J. Schmidt. Communicated by J. B. Dancer, F.R.A.S.

*March* 3*rd*, 1868.—"Brief Notes on the Laws of Physical Force," by Mr. J. C. Dyer, V.P.

*March* 17*th*, 1868.—"An Original Letter of the late Sir Walter Scott." Communicated by E. W. Binney, F.R.S., F.G.S.

*March* 17*th*, 1868.—"Note on an 'Essay on the Resolution of

Algebraic Equations, by the late Judge Hargreave, LL.D., F.R.S.,' " by the Rev. Thomas P. Kirkman, M.A., F.R.S., Hon. Member of the Society.

*March* 17*th*, 1868.—" On the Origin of Colour and the Theory of Light," Part II., by J. Smith, M.A., Perth Academy.

*March* 24*th*, 1868.—" Results of Rain-gauge and Anemometer Observations at Eccles, during the year 1867," by Thomas Mackereth, F.R.A.S., F.M.S.

*March* 31*st*, 1868.—" On the Solution of Algebraic Equations," by the Rev. Thomas P. Kirkman, M.A., F.R.S., Hon. Member of the Society.

*March* 31*st*, 1868.—" Description of a Dolerite at Gleaston, in Low Furness," by E. W. Binney, F.R.S., F.G.S.

*March* 31*st*, 1868.—" A Search for Solid Bodies in the Atmosphere," by R. Angus Smith, Ph.D., F.R.S.

*March* 31*st*, 1868.—" Microscopical Examination of the Solid Particles from the Air of Manchester," by J. B. Dancer, F.R.A.S.

*April* 14*th*, 1868.—" Brief Notes on the Mutations of Imponderable Elements," by Mr. J. C. Dyer, V.P.

*April* 14*th*, 1868.—" Researches on Di-Methyl," by Mr. W. H. Darling, Dalton Scholar in the Laboratory of Owens College. Communicated by Professor H. E. Roscoe, Ph.D., F.R.S.

The printing of the third volume, third series, of the Society's Memoirs, containing papers read during the sessions 1864-5, 1865-6, and 1866-7, has been completed; and a new volume has been commenced. Many of the papers in the above list will appear in this volume.

The Sections have again contributed several interesting and important papers and communications to the Society's Memoirs and Proceedings; and as the system of admitting Sectional Associates continues to work satisfactorily, the Council have decided to recommend its renewal for the ensuing year.

The Librarian reports that the Society continues to receive the various publications of the Associations in correspondence with the Society. The number of learned bodies, &c.,

now exchanging their memoirs, &c., with this Society is 272, 196 of which are Foreign, and 76 are British; this is an increase of 16 upon the number at the corresponding period in 1867.

The binding of the works in the Library is making satisfactory progress, 421 volumes having been bound during the last few months, and there are as many more in progress.

The following journals, &c., are regularly purchased by the Society:—

The London, Edinburgh, and Dublin Philosophical Magazine.
The Annals and Magazine of Natural History.
The Quarterly Journal of Science.
The Zoologist.
The Publications of the Palæontographical Society.
" " " Ray Society.
" " " Cavendish Society.
The Quarterly Journal of Pure and Applied Mathematics.
Genera plantarum, par G. Bentham et J. D. Hooker.
Les Mondes: revue hebdomadaire des sciences, par M. l'Abbé Moigno.
Journal de l'Ecole Impériale Polytechnique.
Les Annales de Chimie et de Physique.
Les Annales des Sciences Naturelles; Zoologie et Botanique.
Le Journal de Pharmacie et de Chimie.
Les Archives des Sciences Physiques et Naturelles.
Prodromus systematis universalis regni vegetabilis, par Alphonso de Candolle.
Die Astronomische Nachrichten.
Die Annalen der Physik und Chemie.
Die Annalen der Chemie und Pharmacie.
Die Journal fur die reigne und angewandte Mathematik.
Neues Jahrbuch fur Mineralogie, Geologie, und Palæontologie.
Die Zeitschrift fur wissenschaftliche Zoologie.
Allgemeine Encyklopädie der Physik.
Astronomische Beobachtungen zu Bonn.

On the motion of Mr. W. B. JOHNSON, seconded by Mr. H. WYLDE, the Annual Report was unanimously adopted.

On the motion of Mr. BROTHERS, seconded by Mr. BINNEY, it was resolved unanimously—"That the system of electing Sectional Associates be continued during the ensuing session."

The following gentlemen were elected officers of the Society for the ensuing year:—

### President.

**JAMES PRESCOTT JOULE, LL.D., F.R.S., F.C.S., &c.**

### Vice-Presidents.

**EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.**
**ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.**
**EDWARD WILLIAM BINNEY, F.R.S., F.G.S.**
**JOSEPH CHESBOROUGH DYER.**

### Secretaries.

**HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S.**
**JOSEPH BAXENDELL, F.R.A.S.**

### Treasurer.

**ROBERT WORTHINGTON, F.R.A.S.**

### Librarian.

**CHARLES BAILEY.**

### Of the Council.

**REV. WILLIAM GASKELL, M.A.**
**PETER SPENCE, F.S.A., F.C.S.**
**GEORGE VENABLES VERNON, F.R.A.S.. F.M.S.**
**JOHN BENJAMIN DANCER, F.R.A.S.**
**WILLIAM JACK, M.A.**
**WILLIAM LEESON DICKINSON.**

## R. WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

**Dr.**

FROM 31ST MARCH, 1867, TO 31ST MARCH, 1868.

**Cr.**

| | | | £ s. d. | £ s. d. | £ s. d. |
|---|---|---|---|---|---|
| 1867. April 1. | —To Balance in Bank of Heywood Brothers and Co. | | | 314 9 1 | |
| ,, | Balance in Treasurer's hands | | | 8 3 0 | 322 12 1 |
| 1868. Mar. 31. | To One Composition | | | | 26 5 0 |
| | To *Members' Contributions:* | | | | |
| | Arrears, 1865-66 | 1 .. | | 2 2 0 | |
| | ,, 1866-67 | 12 .. | | 25 4 0 | |
| | Members on the Roll, April 1, 1867, 180 at 42s. | | £378 0 0 | | |
| | Deduct Compounders | 4 | | | |
| | ,, Members deceased | 2 | | | |
| | ,, Members in Arrear, March 31, 1868 | 14 | 42 0 0 | | |
| | | 20 | | 336 0 0 | |
| | Four Members elected in 1867, April—December | | | 8 8 0 | |
| | Four Admission Fees | | | 8 8 0 | |
| | Seven Associates of Microscopical Section at 10s. | | | 3 10 0 | |
| | One Associate Arrears ditto at 10s. | | | 0 10 0 | |
| | Five Associates of Photographic Section at 10s. 6d. | | | 2 12 6 | |
| | One Associate of Physical and Mathematical Section at 10s. 6d. | | | 0 10 6 | 387 5 0 |
| ,, | To *Sale of Publications:* | | | | |
| | Proceedings | | | | 1 3 0 |
| ,, | To *Sundry Income:* | | | | |
| | Sec. Contrib.: Microscopical | | £2 2 0 | | |
| | ,, ,, Photographic | | 2 2 0 | | |
| | ,, ,, Physical and Mathematical | | 2 2 0 | 6 6 0 | |
| | Interest allowed by Bankers | | | 4 6 5 | 10 12 5 |
| | | | | | £747 17 6 |

| | |
|---|---|
| Composition Fund | £98 15 0 |
| General Balance | 267 19 2 |
| | £366 14 2 |

| | | £ s. d. | £ s. d. |
|---|---|---|---|
| March 31, 1868. | By *Charges on Property:* | | |
| | Chief Rent | 12 14 0 | |
| | Fire Insurance | 5 12 6 | |
| | Inhabited House Duty | 6 7 6 | 24 14 0 |
| ,, | *House Expenditure:* | | |
| | Water, Gas, Candles, and Coal | 18 9 7 | |
| | Cleaning and Petty Expenses | 2 4 11 | |
| | Furniture | 1 14 4 | |
| | Repairs | 4 11 1 | |
| | Tea and Coffee at Meetings | 18 17 2 | 45 17 1 |
| ,, | *Administrative Charges:* | | |
| | Wages—Keeper of Rooms | 52 0 0 | |
| | Attendance on Sections | 6 6 0 | |
| | Postage and Parcels | 20 16 4 | |
| | Stationery, Printing Circulars, &c., and Receipt Stamps | 16 6 5 | 95 8 9 |
| ,, | *Publishing:* | | |
| | Memoirs—Printing, Engraving, &c. | 50 5 6 | |
| | Printing Proceedings | 56 9 0 | |
| | Editor of Memoirs and Proceedings | 50 0 0 | 156 14 6 |
| ,, | *Library:* | | |
| | Periodicals, Binding Books, &c. | 55 6 0 | |
| | Subscription to Palæontographical Society | 1 1 0 | |
| | ,, to Ray Society | 1 1 0 | |
| | ,, to Cavendish Society | 1 1 0 | 58 9 0 |
| | Total Disbursements | | 381 3 4 |
| | Balance in Bank of Heywood Brothers and Co. | | 366 14 2 |
| | | | £747 17 6 |

17th April, 1868.

ROBERT WORTHINGTON, TREASURER.

Audited and found correct, April 20, 1868.

WM. BROCKBANK,
ALF. BROTHERS, } AUDITORS.

The following letter from Sir John F. W. Herschel, Bart., M.A., F.R.S., &c., dated Collingwood, April 28th, 1868, was read:—

I am so accustomed to see passages from my own writings cited in direct opposition to their literal meaning and in support of views directly adverse to those entertained by myself, and which they were written to explain or to advocate, that I should hardly have thought it worth while to notice one more instance of the kind in the paper read at the meeting of the Phil. and Lit. Society on the 14th inst. by Mr. J. C. Dyer, but for a general impression of the inexpediency of silence on such occasions. I allude to his statement that in treating of the undulations of light I have somewhere or other estimated and given in figures the amount of "resistance to the transit of light through space" of what he terms the "luminous ether," (the double commas in this last case being his own)—meaning, I suppose, the luminiferous ether—a distinction ever by myself most carefully attended to. This is very much as if one should speak of the resistance of air to the propagation of sound—of steam power to the conveyance of information by mail-train or of electricity to its conveyance by telegraph—or of the resistance of gunpowder to the propulsion of a cannon ball. It is true that in the article cited I have endeavoured to estimate and to state in figures the intensity of the elastic forces *effective in the propagation* of light—but assuredly I never expected to see this construed (if that be the estimate referred to) into the exact opposite of what it obviously means.

If, on the other hand, Mr. Dyer should have had in view any passage in any other of my writings in which I may have spoken of the doctrine of the imperfect transparency of the celestial spaces, propounded (if I mistake not) by Olbers; I may be allowed to explain once for all, that I never advocated that doctrine and do not believe in it.

Mr. DANCER exhibited some interesting specimens of American photography, lent by Thomas Higgins, Esq., of Liverpool. One of these pictures represented a portion of the valve of the Diatom Pleurasigma angulatum. This object has on its surface striæ estimated at 52,000 in the inch. This was magnified 2,344 diameters by one of Powell and Lealand's objectives of 1-50th of an inch. Another of the same object was magnified 2,540 diameters with one of Wale's 1-8th objective with amplifier. Another picture exhibited these photographs enlarged to 19,050 diameters. Notwithstanding the immense magnifying power employed, these photographs were remarkably sharp and distinct. Some of the other photographs exhibited magnified portions of the Podura scale. These photographs were taken by Brevet-Major Curtis, of the U.S.A.

"Notes on some of the Characteristic Plants of the Channel Islands," by CHARLES BAILEY, Esq.

Two visits to the Channel Islands, made in July, 1863, and August, 1867, enable me to record additional localities for some of the species, as well as exhibit others which have not hitherto been included in the flora of the Islands.

*Ranunculus peltatus*, Fries. var. *a*, *vulgaris*, E. B. A dwarf form of this plant occurs on L'Ancrene Common, Guernsey, and in marshy ground at the eastern end of St. Brelade's Bay, Jersey.

*R. Flammula*, L. var. *β*, *pseudo-reptans*, E. B. St. Saviour's Valley, Jersey.

*R. ophioglonifolius*, Vill. I exhibit the foliage only, for though this species is very abundant in St. Peter's Marsh, Jersey, the peculiar tenure on which the marsh is held causes it to be mown as smooth as a bowling green early in the year.

*Matthiola sinuata*, R. Br. A few seedlings from St. Brelade's Bay, Jersey; but the fine specimens in the room, showing the muricated glandular pods, were gathered in the neighbourhood of St. Owen's Pond, where the plant is abundant.

*Branica adprena*, Bois. Plentiful on the western shore of Jersey.

*B. tenuifolia*, Bois. Common in Jersey, though not included in the lists of plants of that island.

*B. Cheiranthus*, Vill. Met with only on the embankment of the new road between St. Aubyn's and St. Brelade's.

*Helianthemum guttatum*, Mill. Forming almost the entire herbage of some parts of the sandy tract of St. Brelade's Bay; it is common also at l'Etac. All the specimens are much branched, some of the plants making little bushes a foot or more in height. Compared with *H. Breweri*, Planch, the flowers are smaller and of a much paler yellow than in the Anglesea specimens shown by Mr. Sidebotham at a recent meeting. The spot at the base of the petals is of a livid brown, and very different from the larger bright reddish-chocolate of that plant; the seeds are very similar, the pearly tubercles being perhaps closer and better developed in the Jersey plant.

*Dianthus prolifer*, L. Plentiful in the lanes leading from St. Owen's Manor House to the shore; also at L'Etac Point, Jersey.

*Silene conica*, L. Plentiful on the Quenvais, Jersey.

*Cerastium quaternellum*, Fenzl. Not infrequent in Herm.

*Spergularia rupestris*, Lebél (non Camb.). The commonest species of Spergularia in the islands, though often passed over as *S. marina*. Its longer peduncles, black seeds, and glandular pubescence are ready marks of identification. Though generally found on walls and rocks, some of the specimens shown were gathered in salt water ditches.

*Lavatera arborea*, L. Cobo Bay, Guernsey.

*Hypericum linariifolium*, Vill. A few plants met with in a glen near St. Aubyn's, Jersey. Compared with continental examples the Jersey plant is much more stunted, with more of the habit of *H. humifusum*.

*H. Elodes*, L. Plentiful in a marsh at St. Brelade's, Jersey.

*Oxalis corniculata*, L. A common garden weed in the north of Guernsey; also near St. Peter's, Jersey.

*Ulex Galii*, Planchon. This species occurs near the martello tower, St. Brelade's, Jersey. No doubt it is often passed over as *U. nanus*.

*Trifolium enincarnatum*, E. B. Common in Jersey, where it is cultivated for fodder.

*Lotus diffusus*, Tm., and *L. hispidus*, Desf. Common in Guernsey, and especially so on the headland which terminates in Jerbourg Point. I am unable to separate these species with satisfaction, the length of the pod, direction of the beak, and number of the flowers varying considerably in the Channel Island specimens.

*Rubus thyrsidens*, E. B. 3rd. ed. Above St. Peter's Port, Guernsey.

*Epilobium lanceolatum*, Sebast et Maur ? I quote the name of this species with doubt for a plant occurring on road sides in southern Guernsey.

*Œnothera odorata*, Jacq. Well-established on the sands of St. Brelade's Bay, Jersey. Near the same locality grows Exholtzia californica, and a species of Mesembryanthemum.

*Polycarpon tetraphyllum*, L. In considerable abundance in the eastern and southern parts of Guernsey. It is much less plentiful in the other islands.

*Bupleurum aristatum*, Bartl. L'Ancrene Common, Guernsey.

*Galium elatum*, Thuill. Plentiful near St. Peter Port, Guernsey.

*Dipsacus sylvestris*, L. Near St. Owen's Church, Jersey.

*Hieracium strictum*, Fries. I collected on the rocks in the northern part of Sark some plants which I am inclined to refer to this species.

*H. umbellatum*, Vill. Narrow-leaved, slender plants of this species are plentiful on Noirmont Point, Jersey.

*Tragopogon parrifolius*, L. Common in Jersey.

*Diotis maritima*, Cass. In great luxuriance near the Martello Towers, St. Owen's Bay, Jersey.

*Gnaphalium luteo-album*, L. The Quenvais, Jersey, but infrequent.

*Petasites fragrans*, Presl. In a small copse near St. Aubyn's, Jersey, but apparently not flowering.

*Imila crithmoides*, L. Plentiful on the rocks near the the silver mine, Sark; and in salt water ditches near St. Sampson's, Guernsey.

*Cuscuta epithymum*, L. Covering the Gorse everywhere; it is called by the islanders "Fairies' Hair."

*Scrophularia Scorodonia*, L. In great abundance and luxuriance in the fosse of Fort George, Guernsey.

*Linaria arenaria*, D. C. Occurs in great plenty on the neighbouring shores of Normandy and Brittany, and it is not unlikely to be found in Jersey.

*Orobanche Hederæ*, Duby. Common in Guernsey.

*Mentha rotundifolia*, L. Abundant by the side of a brook running into Moulin Huet Bay, Guernsey.

*Calamintha Nepeta*, Clairv. In the greatest profusion in Herm, particularly on the eastern side.

*Leonurus Cardiaca*, L. La Havre Gosselin, Sark.

*Scutellaria minor*, L. Common in a marsh over Bonley Bay, Jersey.

*Echium plantagineum*, L. South-western parts of Jersey; frequent.

*Armeria plantaginea*, Willd. Covering the sands in the western and southern parts of Jersey; exhibiting great variety in the breadth and vernation of the leaves.

*Atriplex arenaria*, Woods. Plentiful St. Brelade's Bay, Jersey.

*Orchis laxiflora*, Lam. Marshy meadows to the south of St. Owen's Pond, Jersey, with Spiranthes autumnalis.

*Allium sphærocephalum*, L. A few plants met with amongst old timber in St. Aubyn's Bay.

*Scilla autumnalis*, L. Very luxuriant at Noirmont Point, and Point La Frette, Jersey. The absence of bracts is often relied upon as a specific character, but I noticed many examples in Sark bearing these appendages.

*Alisma ranunculoides*, L. var *β*, *repens*, Sm. L'Ancrene Common, Guernsey, and St. Brelade's, Jersey.

*Ruppia maritima*, L. In salt-pans near the Vale Church, Guernsey.

*Cyperus longus*, L. More plentiful in Guernsey than in the other islands. The farmers leave patches of this plant unmown in their meadows, in order that it may be gathered in the autumn for making into cordage; the fishermen consider that ropes made from this plant wear much better in the sea than ordinary ropes.

*Cladium Mariscus*, R, Br. St. Owen's Pond, Jersey, growing with Scirpus Tabernœmontani and Scirpus pungens. It is remarkable that so conspicuous a plant has not hitherto been recorded as occurring in the islands.

*Polypogon monspeliensis*, Desf. Road-sides, St. Sampson's, Guernsey.

*Lagurus ovatus*, L. In the greatest profusion on the shores of L'Ancrene Bay, and Grande Havre, Guernsey.

"Brief Notes on Latent Heat," by J. C. DYER, Esq., V.P.

The origin and nature of Heat having engaged so large a share of scientific attention of late years, and directly conflicting principles in physics having been assumed, to explain calorific phenomena, render it desirable to obtain, if we can, some reliable tests of the several grounds on which the opposing theories are based.—On the one side it is affirmed that, "Heat is a material element pervading the universe." On the other, "that it is, in fact, a nonentity, and merely the result, or consequent effect, of the exertion or expenditure of moving forces."—In purely physical enquiries we must be guided by ascertained facts, rather than by authority, even of the most eminent physicists; wherefore I shall adduce some of the well-known facts bearing upon the points at issue. The materiality of heat cannot be sustained, except upon the admission of Dr. Black's doctrine of latent or specific heat in bodies; on this assumption, then, I shall cite some of the cases that go to prove the existence of heat in bodies, beyond what is sensible or thermometric in them; and, therefore, in a neutral or latent state. And first, I take the results of the carefully conducted experiments of Dr. Fairbairn, showing the relations of heat and water in their different forms of union. The following passage is from his valuable work:—"Useful information for Engineers," London, Longman & Co., 1866. pp. 3–4. "When water attains the temperature of 212° Fahr. under the atmospheric pressure, it boils and gives off steam, which, when slowly formed, contains about 934·6° of latent heat, or heat not indicated by the thermometer; if to this be added the sensible heat of 212°, we have 1146·6° of heat required to raise, say one pound of water into steam, under the atmosphere of 14·7lbs. pressure. But at a pressure of 25lbs. per inch, the boiling point rises to 240°, at 50lbs. to 281°, and at 100lbs. it is of $327\frac{1}{2}$° of temperature." The temperature or sensible heat in steam is thus gradually raised by the increasing pressure or density of the steam, and saturated steam, or steam raised in a boiler over water, is found to take up nearly double quantity of water to form steam of double density or pressure; but the latent heat in steam is directly as the quantity of water converted into steam, whilst the sensible heat in it is only as above stated, viz.:—

from 212° at 14·7lbs., to 240° at 25lbs., increase 28°
„ 240° at 25lbs., to 281° at 50lbs., „ 41°
„ 281° at 50lbs., to 327½° at 100lbs., „ 46½°

The decreasing latent heat in steam is found to be nearly as the increasing sensible heat, or, in other words, the absolute heat is nearly in proportion to the water changing its form of a liquid to an elastic vapour.

By the increase of sensible heat, all liquids and solids are gradually expanded, and by the abstraction of such heat, they are contracted in bulk; but the degrees of expansion and contraction are different in bodies, as shown in the mercurial and spirit thermometers, the Wedgwood pyrometers, and the different metallic rods used for compensation balances and pendulums. In all such cases, the gradual changes of bulk by the gain and loss of sensible heat, the *law* governing the changes is quite distinct from that which rules in the cases of sudden changes of water and other liquids into steam and again back into the liquid form, and by which changes of constituent forms, absorption and liberation of heat takes place, to the extent before shown, beyond what is indicated by the thermometer.

It thus appears quite clear that the entering of heat latent in steam is a consequence of water being converted from its liquid state to that of aqueous vapour, and the reappearance of the same heat, in a sensible state, when the steam is converted into water, is equally the obvious consequence of the change. Such reciprocal changes of the constituent forms of bodies, clearly serve to explain the changes of heat into its latent and sensible forms, as before stated. We find too, all solids show the *gradual* loss and gain of heat, however high the temperature may be raised, unless the body itself be converted into a liquid or vapour, and then the sudden absorption of latent heat in them is found, as in the case of steam from water. Taking the total amount of sensible heat in steam of 50lbs. pressure (as in common use) at 281°, as above shown, and the latent heat in the same steam, as given in the tables, at 2,670°, the total absolute heat in such steam will equal 2,951 degrees Fahr. Now, if the entire heat so contained in steam of 50lbs. pressure, were to remain thermometric heat, it would of course raise the temperature of the pipes and engine to the same degree, which would soften the metal and destroy the vessels conducting it, to say nothing of the danger of attending or even approaching the engine. I am aware of the allegation, that heat, not appearing sensible in steam beyond the boiling point, is "because of the expansion of water into some 1,640 times its liquid bulk when converted into steam." Well, even though we allow this way of accounting for the *insensible* state of the heat in steam, still the fact remains that the 934·6° of heat exists in the steam, for that amount is carried over by it to the condensor, and reappears as sensible heat in the water.

From these facts it would seem that the *term* "latent heat" is the only matter in question; for the entire amount of heat, say of 934°, that disappears as sensible heat in the steam, reappears sensible in the condensor. It is quite familiar to all, that by the forming of steam, as in common evaporation, sensible heat is absorbed, and the air cooled, also that when aqueous vapour is condensed, heat is given out; so that heat is thus continually changing its form from sensible to latent, and *vice versâ*, by the changes of water into the alternate states of liquid and vapour.

The same phenomena, but to a less extent, appears in the freezing and thawing of water, namely, when a pound of ice is melted in warm water, the amount of sensible heat required to convert it into water is about 140° Fahr. more than appears in the water, viz,, the heat to maintain its liquid form is 140° in excess of what is contained in the state of ice at the melting point; and when the same pound of water is again converted into ice it gives out the said 140° of latent heat from the liquid in the act of freezing.

A hundred other cases equally familiar might be adduced to show the disappearance and reappearance of sensible heat in bodies upon their changes of constituent forms, as solid, liquids, and vapours, and in all such cases the increments and decrements of heat, in its sensible and latent states, take place suddenly, just as the bodies undergo those changes, and in strict accordance with known scales of specific heat in them, as published in the tables of such heat.

I may here glance at another aspect of latent heat. We have seen that steam of double density, viz., from 25lb. to 50lb. per inch, is raised by the addition of 41° of sensible heat; and again, from 50lb. to 100lb. pressure by the further absorption of 46½° of sensible heat. These small accessions of heat in its sensible state are accounted for by the amount of latent heat in the extra water, as before stated; for, to double the pressure of isolated steam requires some 460° of sensible heat.

If we look to the 140° of latent heat in water over that in ice, it will be seen that in lieu of being expended by the heat thus absorbed in melting the ice, the water is contracted in bulk; for at temperatures from 32° to 40° ice floats with about one sixth of its bulk above the surface. The notion, therefore, that heat ceases to be sensible by reason of the expansion of bodies when it becomes latent in them is clearly shown to be a fallacy.

I come now to notice the latent heat that becomes sensible by the union of a solid with a gas. The latent heat in oxygen and carbon being in excess of that in carbonic acid, by the formation of this acid in breathing, the supply of sensible heat is duly maintained to keep up the temperature of warm-blooded animals, and the excess of heat evolved in the lungs is carried off by converting water into steam, which passes out with the breath. If the whole heat so evolved were to remain sensible, it would destroy the living functions.

Many other mutations of heat depending on the vital energies were explained in a former paper, and may be here passed by, with the remark that this entire class of changes, as in the case of respiration, are effected by chemical forces called into action by vital organisms. The alternate changes of sensible and latent heat depending on the constituent forms of bodies, and upon the chemical forces so called into action, might be shown in a hundred other cases, but the above will suffice to mark the principle on which the mutations of heat depend. Apart then from the chemical relations of heat with other bodies, I proceed to notice some of the cases in which the appearance and disappearance of sensible heat that do depend on the action of mechanical forces; these are of so wide a range, that only a few of them can or need be given. It must be kept in view that the latent heat in bodies, besides depending on their constituent forms, as solids, liquids, vapours, and gases,—is also proportionate to their bulks, in the same form; wherefore, by compressing a body of any kind, sensible heat is given out, and by expanding it sensible heat is re-absorbed, or taken up in a latent state; so that mechanical forces continually acting among bodies around us bring about the alternate conditions of heat so familiar to everyone. The cause therefore is thus plainly seen why sensible heat in bodies is produced by mechanical forces. But this affords no ground for assuming the generation *de novo* of any heat by the exertion of any force—either chemical or mechanical.

We have sensible heat from the solar rays, and that produced by combustion, and other chemical changes of bodies, but have strong grounds for concluding that all forms of sensible heat produced by mechanical forces are derived from the latent heat, rendered sensible by compression or the abrasion of bodies by such forces. But the alternation of sensible and latent heat thus caused by mechanical action is seen in an endless range of compressions and expansions from the lighting of a match in the air syringe to the showers of sparks issuing from the particles of steel torn off in *dry grinding,* and ignited by the intense heat from the compressed air between the steel blade and the grindstone. I need not enlarge on the numerous other facts in accordance with the foregoing, but may venture to say that those cited must be directly met by adducing others to show *how* sensible heat is made to come and go otherwise than by the changes of heat from one to the other of its states of sensible and latent, before the principle of such mutations of heat can be superseded by the new theory. To say that "*work is done*," when water passes into steam, equal to the heat entering the steam,—and, again, that when the steam is condensed mechanical force is excited equal to the heat re-appearing in the water,—are naked assertions, with no shadow of proof to sustain them, for no mechanical force is called into action in forming the steam, nor is any such force exerted by it in returning to water. So that *force* has nothing to do with those changes, except it be the chemical forces, depending on the known relations of heat and water. We all know that there is nothing *occult* in the laws of dynamics, and that all moving forces can be distinctly *measured* and their amount and direction plainly proved, which forbids the admission of any mystery in the science of mechanics. Now, in the face of these established laws, we find the following announcement by the able expounders before cited of the New Philosophy, viz.—"When the motion of a body is arrested, the motion of the mass is transferred to a motion of the molecules—or particles composing it—and this motion is heat." And again, that "Mathematical formulæ have been deduced from it, and thus a quantity has been found, termed the Mechanical equivalent of heat." That the motion of the mass is transferred to the molecules is an assumption devoid of any foundation in known facts, for we have no means of proving any kind of dynamical action by such intangible, unknown, and suppositious molecules, or ultimate particles of bodies. And to base mathematical formulæ on such visionary forces and motions can hardly comport with the dignity of the exact sciences, or tend to shed any light upon physical truths, which after all must be sustained by known facts, rather than by theories, old or new. As these two theories are based on opposite principles, they cannot both be true; and without referring here to any of the other cases (adduced in former papers) that appear to prove the untenable nature of the new theory, I conclude by submitting the above short outlines of the phenomena resting on, and explaining, the doctrine of latent heat.

## MICROSCOPICAL AND NATURAL HISTORY SECTION.

March 23rd, 1868.

J. B. DANCER, F.R.A.S., President of the Section, in the Chair.

Mr. SIDEBOTHAM stated that on opening a box containing specimens of Limnœa clodes from Canada, sent to him by Prof. Billings, a living bee, apparently the common hive-bee, crawled out.

Dr. ALCOCK showed a living frog-tadpole, two and a half inches long, with a body as large as a hazel nut, but the hind legs not more than a quarter of an inch long. He stated that it was taken from a pond in Higher Broughton on the 7th of March, at a time when the frogs were beginning to spawn. It was evident that it had lived through the winter, growing larger but advancing very little in development; and he could only account for its occurrence in that condition by supposing it had grown from spawn deposited in the autumn of last year.

Mr. SIDEBOTHAM read the following paper, entitled "Further Notes on Wood-feeding Coleoptera."

A short time ago I brought before you a few notes on some of the wood-feeding beetles found in this neighbourhood, and pointed out the very great damage they cause to growing timber. I will now refer to a few of the species which chiefly attack timber when it has been cut down and used for various purposes.

It appears to be one of the provisions of nature that as speedily as a tree is separated from its root it is destroyed as soon as possible, and various means are at once brought into play to this end. Very soon after the tree falls, it is attacked by various species of coleoptera, the bark mined and the trunk pierced in all directions, the sharp mandibles of the little creatures working into the hardest wood. The holes thus made let in the rain, and then the half-rotting wood is attacked by other species, and in a very short time the whole is reduced to dust, which if left to its own natural decay would encumber the ground for years.

Unfortunately, these little beetles cannot distinguish between what is of value and what is not; stumps and rails look to them the same as old trees, and they treat them in the same way.

In this part of the country, three species of the genus Anobium, and Ptilinus pectinicornis, another nearly allied species, are the most destructive. We often see rails and gate posts perforated as if by small shot, and if we examine them carefully we find the inside of the timber a mass of dust.

Where timber is of much value, or where much labour is spent upon it, some steps ought to be taken to prevent its destruction, either by kyanizing it, or by coating it with paint and varnish before it is attacked. Anobium tesselatum, and Anobium striatum, are species that feed upon wood and furniture, in the insides of buildings, and often cause great destruction before their presence is known, although the first named, which is the well-known death watch, makes itself heard, often to the alarm of the ignorant; but as a writer 150 years ago remarks, "We look on it as an idle superstition, for were anything in it, inhabitants of old houses were in a melancholy condition." It appears to be an instinct of these beetles to remain inside the timber, burrowing their holes in all directions, and when they come near the surface to turn away, without making a visible hole. The female rarely leaves the burrows, but lays her eggs inside them, the only holes apparent being those by which the males have escaped. Thus it may happen that a beam of a house apparently strong, and in which only a few small holes are to be seen, is in reality a mere shell full of dust. My cousin, the late Mr. Ashworth, once shewed me a large box full of wooden silk bobbins. They were of a size not often wanted, and had remained in this box about two years; when required, it was found the whole of the wood had been eaten by Anobium striatum, leaving the exterior of the bobbins of the thickness of paper, and apparently quite perfect.

No wood appears too hard or too dry for these creatures. A piece of furniture which had been in my possession many years, and had belonged to my father, was attacked by this species. The first intimation I had was the finding of some of the beetles inside the window of my bedroom, and on searching I found the holes from which they had escaped. I at once had the wood washed with a solution of bichloride of mercury, and the holes well saturated with it, and have since seen no more.

In a private house the presence of this beetle may be soon seen, but in a church or public building much injury may be done before it is discerned. When it is, the timber infected should, if possible, be removed, or well washed with bichloride of mercury and mineral naptha. It should also be covered with a fine net in the month of May, so that the beetles then assuming a perfect state and emerging from the timber could be prevented going to other parts of the building.

The injury to timber is caused almost entirely by the larvæ, the perfect insects simply drilling holes to make their escape on their arrival at maturity. Remembering this, and knowing the habits of the various species, and the times when they leave their holes, much damage may be prevented by the destruction of the perfect insects before they have time to lay eggs. A year or two ago, a friend called my attention to a small grub, which was eating the corks of his wine bottles, and doing considerable damage by boring holes through the corks and letting out the wine. The grub was the larva of a small beetle, Mycetæa hirta, and I reared some of the beetles from the larvæ in one or two corks. The perfect insects emerge early in June, and I think if the wine cellar in such a case were well whitewashed at that time, when the insects are on the ceiling and walls, they would be destroyed; and if the corks were covered well with sealing wax, one or two seasons would be sufficient to banish this beetle altogether from the place.

I may just remark that I think it would be quite worth the trouble of every one to know some little of the appearance and habits of the beetles usually found in houses, if for no other reason than to preserve their property. A wasp destroyed in the spring is the destruction of what would be a wasps' nest in the autumn; in like manner, the capture of a small beetle on a window may prevent the destruction of a valuable cabinet, or the injury of an entire library.

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## April 20th, 1868.

J. BAXENDELL, F.R.A.S., in the Chair.

Mr. CHARLES BAILEY read a paper "On Some of the Characteristic Plants of the Channel Islands," and exhibited a large collection of specimens.

[This paper was afterwards read at the annual meeting of the Society, held on the 28th of April, 1868. See p. 194.]

The **Secretary** read a communication from the Rev. J. E. Vize, "On the Mite of the Wood Pigeon."

It was observed that within a short time of the death of the pigeon these mites appear much more frequently than when the body of the bird was warm; that they come and go very much like a dissolving view; moreover, they always make ingress and egress through the vanes of the feathers of the bird; these they open, and then pass away, not head first, but just as a heavy weight would do when left on water to gravitate for itself.

They are very nimble, and always clasped the feather by at least two of their feet, never less. Hence their tenacity, for it is most difficult to remove them from their holdfast. And here is a beautiful and natural means of self-preservation: if they lost their security and became detached from the bird in flight, they could not regain their place, and probably must die. They walk differently to most insects, using invariably the central foot on the right side, and the first and last of the opposite side, and *vice versa*. When quiescent they adhere very tightly by the pair of feet nearest to the head, being aided in this by a pair of very small, but most useful, clips or hooks at the extremity of each of their feet.

As to the antennæ, they denote very great excitement being in rapid restless movement. Whilst the insect is alive, they are at something like right angles to the body of the bird, but are nearly always parallel to the body when dead.

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## Annual Meeting, May 4th, 1868.

A. G. **Latham**, Esq., Vice-President of the Section, in the Chair.

The Annual Report was read, approved, and ordered to be printed and circulated among the members.

The following Officers were elected for the ensuing session:—

### President.

JOHN B. DANCER, F.R.A.S.

### Vice-Presidents.

ARTHUR G. LATHAM.
JOSEPH BAXENDELL, F.R.A.S.
JOHN WATSON.

### Secretaries.

THOMAS ALCOCK, M.D.
CHARLES BAILEY.

### Treasurer.

HENRY ALEXANDER HURST.

### Of the Council.

W. C. WILLIAMSON, F.R.S.
ALFRED BROTHERS, F.R.A.S.
SAMUEL COTTAM.
THOMAS COWARD.
JOSEPH SIDEBOTHAM.
JAMES G. LYNDE, F.G.S., F.R.M.S., M.Inst.C.E.
HENRY SIMPSON, M.D.
JOHN BARROW.

Mr. C. **Bailey** exhibited a sample of the shell-beach at Herm, and explained how shells are thrown up at that particular spot in consequence of the direction of the several currents around and between the neighbouring islands.

Mounted specimens of Meridion circulare from the entrance to the Peak Cavern, Castleton, were sent for exhibition by Mr. Dancer, together with a stock of the same Diatoms presented for distribution.

Leaves of Hippophaë rhamnoides from Folkstone were presented by Mr. Sidebotham for distribution among the members.

## PHYSICAL AND MATHEMATICAL SECTION.

April 21st, 1868.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section, in the Chair.

"On Ozone and its Probable Connexion with Solar Radiation," by THOMAS MACKERETH, F.R.A.S., F.M.S.

Many hypotheses have been set forth respecting this mysterious something, ozone. My present object is to bring together as many known facts as I am able respecting it, and to draw therefrom such conclusions as I think fairly legitimate. The deductions that may be drawn from experiments made in a chemical laboratory are no doubt important and useful; but if to these we can add observations from the great laboratory of nature, we may hope to arrive at fuller and better information.

First.—*Where ozone cannot be found.* Attempts were made for some time to detect it in the vicinity of the Manchester Royal Infirmary, but without success. Mr. Samuel Marshall, of Kendal, states that "in a locality west of Birmingham, they have indications of the presence of ozone when the wind is from the west, or before it reaches the town; but when the wind is in the opposite quarter, and passes over the town before reaching there, none is found." In addition to this evidence of the absence of ozone in towns, I can state that, from observations I have made during the last six months in Salford, hardly any has been detected with the most sensitive tests. Hence it is clear that large towns either destroy ozone or prevent its manifestation.

Second.—*Where it is partially found amongst populations.* I lived for four years in close proximity to the most populous part of the village of Eccles. This village contains a population of two or three thousand. Its sanitary condition is most unsatisfactory. Its mean height above the sea is about 112 feet. It is situated about four and a half miles west of Manchester. I made observations of ozone at that station for more than three years, and the amounts detected with Dr. Moffat's test papers were considerably below those obtained by Mr. Vernon, F.R.A.S., at Old Trafford, two miles nearer Manchester, but where the population is less dense and the sanitary condition far superior. I infer, then, that density of population, with imperfect sanitary regulations, either destroys ozone or prevents its formation.

Third.—*Where variable amounts of ozone are found out of large towns.* Ozone is detected in considerable quantities on the sea-coast, and in rural valleys far removed from towns; but it is well known that far larger amounts are detected in elevated districts, as on hills and mountains. The amounts detected by Mr. Heap, of Royton, near Oldham, and Mr. Thompson, of Wilmslow, far exceed anything that is found on low rural lands or near the sea. Mr. Heap's station is 544 feet above the sea, and Mr. Thompson's 251 feet. Mr. Samuel Marshall, of Kendal, also writes—"I am much surprised at the results of experiments made by my friend Edward Hayton, at his residence, High House, which is about 7½ miles N.W. from Kendal, and two miles nearly west from the railway station at Windermere—636 feet above the sea level. * * * No material interruption of hills between it and the sea, Morecambe Bay, which lies a little west of south. The amount of ozone collected here is surprising, being nearly three times the quantity of that at Kendal. It may be said that sea breezes conveying ozone from the Irish Sea meet with no material obstacle before reaching High House; but they meet with comparatively little before reaching Kendal, and yet the results are so very different." Mr. Marshall's hypothesis that ozone comes from the sea does not enable him to discover why there is more ozone at High House than at Kendal, which is several miles nearer the sea. During my several visits to Westmoreland I have made ozone experiments, and have always obtained less ozone in the valleys and low grounds than on the hills and highlands. The amounts obtained have increased according to the height at which I have placed tests. All my experiments were made around Windermere. These facts simply show that elevated regions are more conducive to the production of ozone than low lying districts, however free from population or near the sea.

Third.—*Times when variable amounts of ozone are found.* On looking carefully through my registers for the last five or six years, I find that, as a rule, the amounts of ozone increase as the height of the barometer decreases. It is well known that the largest amounts of ozone are detected when high moist winds prevail, and during magnetic and electric storms; but when it is considered that winds are a consequence of the removal of atmospheric pressure, it may be expected that we shall find atmospheric conditions answering to the reduced amount of pressure. As the amount of atmospheric pressure is less on high than on low lands, and as more ozone is found on high than on low lands, so if all other conditions remain the same, it will happen that as the atmospheric pressure is reduced on the low lands a proportionate increase of amount of highland ozone will be found.

Fourth.—*The relation of ozone to oxygen.* It is well known that if oxygen be eliminated from water by means of electric currents, ozone is found in the oxygen so eliminated. The force or activity thus given to oxygen causes it to manifest ozone in large quantities. Clearly then oxygen and ozone are intimately related; and it is not unreasonable to suppose that oxygen is more easily rendered active at a high position in the atmosphere than a low one, that is, rendered more active under a light pressure than under a dense one.

Fifth.—*The apparent connexion of Ozone with Solar Radiation.* Whether we liberate oxygen chemically, or by electricity, the force which produces heat has to be employed. The source of terrestrial heat is the sun; and this heat, in the main, is the cause of all our atmospheric phenomena, especially of those movements of the air called winds, and the magnetic and electric storms the great companions of ozone manifestations. Two important papers have been read before this society by Mr. Josh. Baxendell, F.R.A.S., "On Solar Radiation," which have been printed in the 7th vol. of the proceedings of the Society. In the first of these papers it is shown—

"1st. That the calorific intensity of the sun's light is subject to periodical changes, the maxima and minima of which correspond respectively with those of the solar spot frequency."

"2nd. That the intensity of a ray of direct sun-light on its arrival at the earth's surface in the latitude of Oxford is greater in April and September than in June, when the sun's meridian altitude is greatest.

"3rd. That the curve representing the mean monthly values of solar radiation on cloudless days, has its times of maxima and minima corresponding with those of the curve representing the mean monthly diurnal ranges of the magnetometer."

And at page 105, Mr. Baxendell further shows "That clouds and haze are less prevalent during the day, or their power to intercept the heating rays of the sun is less active in the spring and autumn than in the winter and summer months." In the first of these papers, Mr. Baxendell gives the mean monthly differences between the maximum temperature in the shade and that in the sun during the six years 1859 to 1864. They are as follows:—

| | | | |
|---|---|---|---|
| January | 5·51° | July | 13·81° |
| February | 9·16° | August | 14·08° |
| March | 10·08° | September | 13·71° |
| April | 13·28° | October | 9·66° |
| May | 12·90° | November | 8·00° |
| June | 12·93° | December | 4·50° |

Here are represented two maxima of solar radiation which occur in April and August, and two minima in May and December. Below I represent the mean monthly amounts of ozone for five years, from 1862 to 1867:—

| | | | |
|---|---|---|---|
| January | 1·15° | July | 1·29° |
| February | 1·44° | August | 1·34° |
| March | 1·85° | September | 1·01° |
| April | 2·51° | October | 0·80° |
| May | 2·00° | November | 0·88° |
| June | 1·48° | December | 1·25° |

From the foregoing table two maxima will be seen identical with those of Solar Radiation, viz. one in April and another in August. The minima are not satisfactory. There are reasons for this. The first four years' observations were made near the village where the amounts, for the reasons already given, were small and fitful; and the test papers used were not so sensitive as those I have since used. The amounts I have obtained for the last year and a half are more than double the amounts I obtained at my previous station. I am much farther from the village, several feet higher, and use very sensitive test papers. I anticipate that future observations at my present station will far more fully concur with the Solar Radiation deductions above quoted. In proof of this, I give below the mean monthly amounts of ozone for 1867.

| | | | |
|---|---|---|---|
| January | 2·74° | July | 1·48° |
| February | 3·40° | August | 1·42° |
| March | 3·57° | September | 1·96° |
| April | 5·60° | October | 1·48° |
| May | 2·29° | November | 1·00° |
| June | 1·86° | December | 2·70° |

But there is a still more striking relationship between the amounts of Solar Radiation and Ozone. In Mr. Baxendell's second paper on Solar Radiation at page 104, we are presented with the reduced annual values of Solar Radiation at Eccles, together with their Ratios, as follows:—

| Year. | Reduced Values of Solar Radiation. | Ratios. |
|---|---|---|
| 1863 | 9·82 | 1·63 |
| 1864 | 9·84 | 1·51 |
| 1865 | 8·32 | 1·57 |
| 1866 | 7·06 | 1·17 |

After this is added, for comparison, the number of groups of solar spots observed in each year, by Schwabe, to which I have added a column giving the mean annual amount of ozone in each of these years:—

| Year. | Ratios of Solar Radiation. | Mean Annual Amount. | Number of Groups of Solar Spots. |
|---|---|---|---|
| 1863 | 1·63 | 1·55 | 124 |
| 1864 | 1·51 | 1·12 | 130 |
| 1865 | 1·57 | 0·95 | 93 |
| 1866 | 1·17 | 0·97 | 45 |

Here it is obvious that the amounts of ozone have decreased each year with the amount of solar radiation, and pretty nearly too as the groups of solar spots have decreased.

Now, if it be determined that the intensity of the solar radiation is coincident with the periodic activity of the solar photosphere—which I think Mr. Baxendell's papers largely, if not conclusively, confirm—and if we find a periodicity of ozone intensity nearly identical with the periodicity of solar radiation, there is a warranty for concluding,—

1st. That ozone is the result of an activity produced in oxygen by the direct action of solar radiation.

2nd. That as a highly compressed, dense, or confined atmosphere impedes the effects of direct solar radiation, so less ozone is found in valleys and low-lying districts than on mountains and highlands.

3rd. That as direct solar radiation is impeded in large towns and populous districts, and the action of oxygen is in many ways neutralized, therefore little or no ozone is found in such places.

"Notes of Comparisons of Ozone Papers," by G. V. VERNON, F.R.A.S., F.M.S.

Mr. Mackereth, F.R.A.S., having kindly supplied me with papers prepared by Dr. Moffat's method, but on unsized paper, I thought it would be interesting to see what the relative amounts of ozone found would be as compared with Dr. Moffat's ordinary papers. The papers were exposed in precisely a similar manner, and quite free from any solar influence.

The observations only extending over a few months, there is of course but little to be determined from them so far, except the fact of the much greater amount of ozone shown by the papers prepared from unsized paper. With these papers amounts are frequently registered when the sized papers give no indication whatever, and in addition to this the average daily amounts are much greater. The details I have annexed in Table I.

TABLE I.

| | MOFFAT OLD. | | MOFFAT UNSIZED. | | Excess of Days of Unsized Paper. |
|---|---|---|---|---|---|
| | Days. | Mean Daily Amt. | Days. | Mean Daily Amt. | |
| 1867. | | | | | |
| August | 14 | 2·26 | 15 | 3·48 | 1 |
| September | 13 | 1·80 | 16 | 2·71 | 2 |
| October | 7 | 0·93 | 12 | 2·59 | 5 |
| November | 3 | 0.19 | 6 | 1·22 | 3 |
| December | 5 | 0·52 | 10 | 1·84 | 5 |
| 1868. | | | | | |
| January | 19 | 3·06 | 19 | 4·97 | 0 |
| February | 19 | 1·80 | 24 | 5·40 | 5 |
| March | 18 | 2·30 | 22 | 6·20 | 4 |
| April | 11 | 1·56 | 14 | 3·80 | 3 |
| Sums | 109 | ... | 137 | ... | 28 |

TABLE II.

| | Sized Papers. | Unsized Papers. | Ratio. |
|---|---|---|---|
| 1867. | | | |
| August | 2.26 | 3.38 | 1·49 |
| September | 1·80 | 2·77 | 1·54 |
| October | 0·93 | 2·26 | 2·43 |
| November | 0·19 | 0·60 | 3·13 |
| December | 0·52 | 1·26 | 2·42 |
| 1868. | | | |
| January | 3·06 | 4·97 | 1·62 |
| February | 1·80 | 5·03 | 2·79 |
| March | 2·30 | 5·77 | 2·50 |
| April | 1·56 | 3·20 | 2·06 |
| Mean | ... | ... | 2·22 |

In Table II. I have given the mean daily amounts with each kind of paper, omitting, however, from the observations with unsized paper all those days upon which the sized paper gave no indications. In the last column of this table are given the ratios between the amounts given by each paper in the previous columns, in order to see the relative values of the indications. The mean ratio appears to be 2·22 for the period in question, showing that the observations made with the old Moffat papers require multiplying by this amount to correspond with the new papers. These ratios seem to vary a good deal, and do not appear to depend upon the amount of ozone observed.

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Extract of a letter received by the Editor from Mr. Vernon, F.R.A.S. May 7th, 1868:—

As bearing upon a point in the paper read by Mr. Mackereth, F.R.A.S., on ozone observations, I may state that between 1856 and 1862 I found from observations made at Newby Bridge, Lancashire, and the summit of a hill at the same place having a difference of about 500 feet of altitude above the sea, that the amount of ozone at the higher station was invariably greater than that found at the lower one. This I considered was owing to the upper station being much more exposed to strong atmospheric influences. This corroborates strongly what Mr. Mackereth has found in the neighbourhood of Windermere.

It is very unfortunate that we have no ozone observations round our eastern coasts, as that would tend to solve the question as to the very small amounts found during the prevalence of easterly winds, and we should then be able to see if an easterly sea breeze developed it. Some of the favourite watering places on the Yorkshire coast — Scarborough, Whitby, Filey, Flamborough Head, &c.—would just be the places for making these observations.

"Note on the Correction of an Algebraic Solution," by the Rev. THOMAS P. KIRKMAN, M.A., F.R.S., Hon. Member of the Society. Received by the Editor June 8th, 1868.

An important step in the reasoning is omitted at p. 147. It is not true that the four functions in lines 2, 4, 8, and 9 are rational functions of the coefficients of $G=0$. They are all five-valued functions of $x_1x_2 \ldots x_5$, and their values are are first to be formed on the following five shapes of our group of 60 positive substitutions, of which the first only is expanded in page 145, each group to be followed by its negative derivate of 60,

| | | |
|---|---|---|
| 12345 | 12345 | 12345 |
| 23145 | 21435 | 23451 |
| 31245 | 34125 | 34512 |
| | 43215 | 45123 |
| | | 51234 |

| | | |
|---|---|---|
| 12345 | 12345 | 12345 |
| 23145 | 21543 | 23451 |
| 31245 | 35142 | 34512 |
| | 53241 | 45123 |
| | | 51234 |

| | | |
|---|---|---|
| 12345 | 12345 | 12345 |
| 14352 | 21354 | 23451 |
| 15324 | 45312 | 34512 |
| | 54321 | 45123 |
| | | 51234 |

| | | |
|---|---|---|
| 12345 | 12345 | 12345 |
| 12453 | 32154 | 23451 |
| 12534 | 42513 | 34512 |
| | 52431 | 45123 |
| | | 51234 |

| | | |
|---|---|---|
| 12345 | 12345 | 12345 |
| 12453 | 13254 | 23451 |
| 12534 | 14523 | 34512 |
| | 15432 | 45123 |
| | | 51234 |

The specific difference of these five forms of the same group of 3·4·5=60 lies in the group of 3·4=12. The four functions in lines 2, 4, 8, 9 of p. 147 are to be constructed in all their five values on these five forms.

Page 143, lines 21 and 24, for *ambiguous* read *unambiguous*.
„ 141, over C, for 4123 read 4231 ;
„ 144, line 14, for $\alpha(x_5-x_2)$ read $\alpha(x_4-x_2)$ ;
„ „ line 15, for $\alpha(x_6-x_3)$ read $\alpha(x_5-x_3)$ ;
„ „ line 17, for *cube* read *sixth*.

P.S.—Sept. 7. I have to confess that my attempt in the preceding number to solve the quintic, &c., is, like all such efforts, a failure. My friend, Professor Harley, has proved to me that $N_i$, $P_i$, &c., pages 142, 143, though symmetrical in $x_1x_2x_3x_4$, are not so in $x_0x_1x_2x_3x_4$, in spite of the cyclical character of the values of H. But, I trust that what I have written is not utterly useless in connection with the values of functions, nor do I despair of its leading to the coveted solutions.

I have been asked for my proof that $P + Q$ must be symmetrical in the $z$'s, if $P^5 + Q^5$ is so, page 133. I say that if $P^5 + Q^5$ is symmetrical, $P^5$ cannot have more than two values by the permutations of the $z$'s, viz., $P^5$ and $Q^5$. If $P + Q$ is not symmetrical, $P$ will have more than two values by such permutations, and every substitution among the $z$'s which turns $P$ into $P_1$ or $P_2$ will turn $PPPPP$ into $P_1P_1P_1P_1P_1$ or $P_2P_2P_2P_2P_2$, *i.e.* $P^5$ will have more than two values.

"Note on the Resistance of Fluids," by Dr. J. P. JOULE, F.R.S., President. Received by the Editor, July 21st, 1868.

Coulomb appears to have been the first who pointed out clearly the two sources of resistance to bodies moving in fluids, the first varying in the simple ratio, the second in the duplicate ratio of the velocity. He found the simple ratio to prevail only at the very slow velocity of not more than half an inch per second. I have long had in view experiments for the purpose of further illustrating this subject, and possess an apparatus which, although constructed so long ago as 1849, I have not experimented with before the last few weeks. It consists of two vertical discs of turned and polished steel, each 18 inches in diameter, which revolve between corrugated iron plates at about one quarter of an inch distance. The axle passes through a leather collar, and the weight of the discs, which is considerable, is supported by friction wheels. Revolution is effected by the agency of weights and pulleys. A train of wheels in connection with the axis, by which a papered drum is moved under a pen worked by a pendulum, furnished the means of measuring the velocity. The following are the results at which I have already arrived. They are referred to one disc:—

| Fluid. | Velocities of Rim in feet per second between which the Experiments were made. | Index of the Power of the Velocity which is proportional to the Resistance. |
|---|---|---|
| Air | ·7 and 5·36 | ·64 |
| | 5·36 „ 13·2 | ·98 |
| | 13·2 „ 23·03 | 1·7 |
| Water | ·47 „ 5·2 | 1·084 |
| | 5·2 „ 13·44 | 1·78 |
| Oil | ·19 „ 1·78 | 1·24 |

I infer from the above that in the circumstances of my experiments the resistance of liquids up to five feet per second is nearly in the simple proportion of the velocity. The following are my results as to the resistance due to a given velocity :—

| Fluid. | Pressure on Rim in grains. | Velocity of Rim. |
|---|---|---|
| Air | 853 | 23 |
| Water | 6267 | 13·44 |
| Oil | 6715 | 1·79 |

When the liquids were exposed to three atmospheres' pressure, the resistance appeared to be slightly increased. I hope to clear up this and other points, and to obtain more exact results, when the apparatus shall have received the improvements which have suggested themselves in the course of this preliminary enquiry.